REALE ACCADEMIA DELLE SCIENZE DI TORINO

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MISCELLANEOUS

WORKS

AND

CORRESPONDENCE

OF

THE REV. JAMES BRADLEY, D.D. F.R.S.

ACCADEMIA
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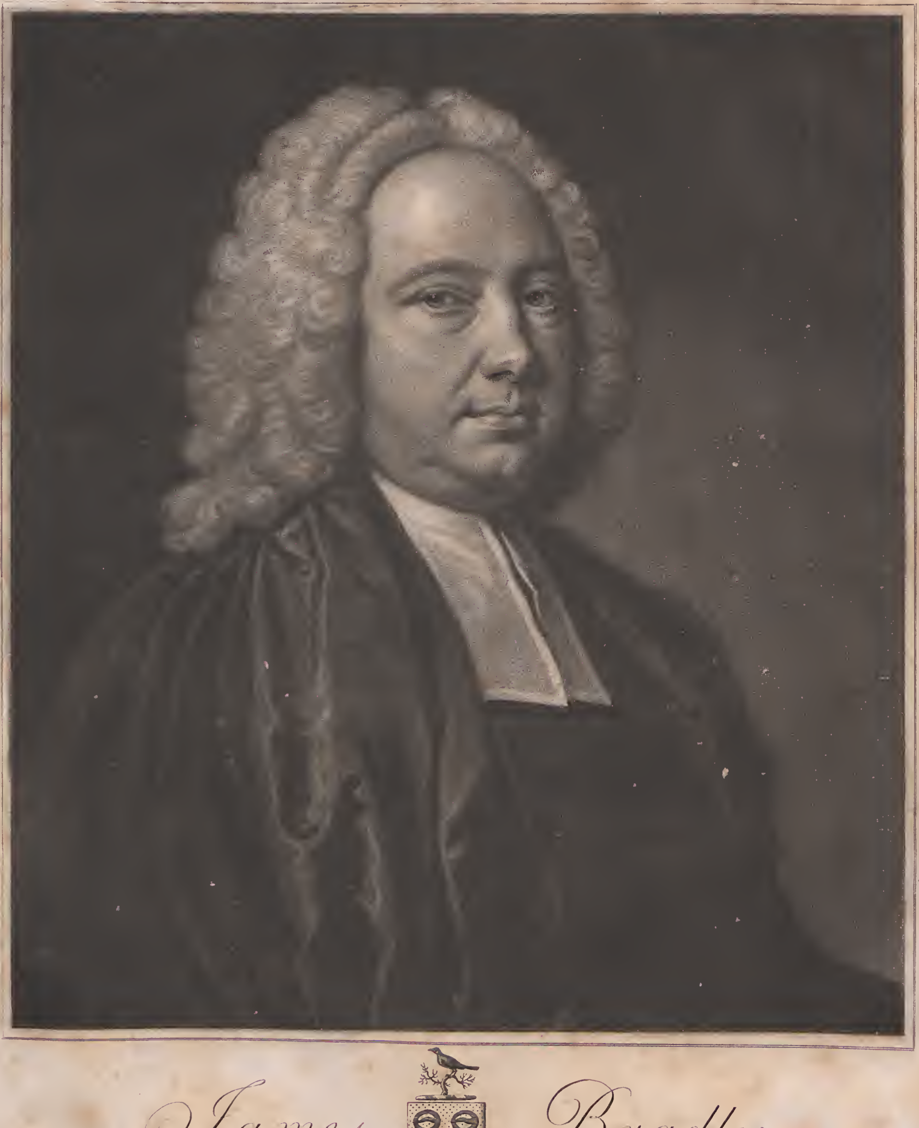
DI TORINO



James Bradley.

D.D. & F.R.S.

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THE REV. JAMES BRADLEY, D. D. F. R. S.

ASTRONOMER ROYAL, SAVILIAN PROFESSOR OF ASTRONOMY IN THE
UNIVERSITY OF OXFORD,
&c. &c. &c.

OXFORD,
AT THE UNIVERSITY PRESS.
MDCCCXXXII.

PREFACE.

DR. BRADLEY'S Manuscripts were given by his son-in-law, the Rev. Samuel Peach, to Lord North, who presented them to the University of Oxford, of which he was then Chancellor: they were placed in the hands of Dr. Hornsby, Savilian Professor of Astronomy, with a view to publication; and the observations made with the new instruments at Greenwich, from 1750 to 1762, were printed at the University Press. The original books, containing these and the other Greenwich observations, were then deposited in the Bodleian Library, with a small number of loose papers (in one of them) from which some additions had been made to the original publication. Repeated inquiries have of late years been made for Bradley's other remains; but no traces of them could be found, until by a combination of fortunate circumstances it was discovered that very many were still extant among Dr. Hornsby's own papers. A representation of the fact having been made to his family, they were readily restored to the University; and as I had been the means of recovering them, they were in the summer of 1829 placed in my hands, with a request that I would prepare for the press whatever might be found fit for publication.

The task was not easy. The papers were in great confusion; and it was necessary to look repeatedly through the whole before even a general arrangement of them could be made. A more minute examination required further time: sometimes, a whole book full of calculations afforded little for the object in view; and at others, a memorandum on a scrap of paper, or on the back of a letter, conveyed information, which was not to be found elsewhere. It was necessary to look to every thing; and after a second and a third revision, there was more than one instance in which I had the mortification of discovering some previous oversights. This was more particularly the case in collecting the Miscellaneous Observations, which are printed p. 339—380. These were found, some in books, others on loose paper, sometimes interspersed among other entries, and sometimes, when not mixed up with different matter, disjointed from their chronological order. The printed sheets were always carefully corrected by the originals; but even after the short interval from the copy's being set up at the press, it has often required an effort of recollection, to recover the place from whence a particular observation was transcribed. This perplexity, it is hoped, will be deemed a sufficient excuse for the few which are printed out of their regular places at p. 380.

It was soon seen that the Kew and Wansted observations would be incomplete without the accounts that Bradley had himself printed of the discoveries which they enabled him to make. A similar difficulty, though not so strong, occurred, with respect to other papers; and as Bradley's works had never been collected, I thought it desirable to take this opportunity of bringing together as much as possible from what could be found of his writing, either in print or manuscript. What had been previously published was so little, that some time elapsed before I could satisfy myself that there was not more than I had been able to meet with; and after a careful search for any new matter which might be added to that which had now been found, I met with no success but at Greenwich, where, from Bradley's handwriting being familiar to me, I was able to discover some books and papers which were not noticed as his in the Catalogues. For the readiest and most effectual assistance in this search, and for much information relative to the Observatory, I am indebted to Mr. Taylor, who has now been connected with the establishment for nearly five and twenty years. The Astronomer Royal gave me the free use of all that I found; and, although it was not considerable in bulk, it supplied me with several valuable particulars.

PREFACE.

Among Harriot's papers there were known to be observations of Halley's comet when it appeared in 1607; and through the Rev. T. Sockett, Rector of Petworth, I applied to the Earl of Egremont for permission to examine them. His Lordship was kind enough, not only to intrust me with the originals, but to allow me to make use of any part that I might wish to publish; and I thought that the Appendix, which will be found at p. 511, would be a valuable and appropriate addition to Bradley's observations of the same comet in 1759.

In the examination of Bradley's papers many notices occurred of what was either entirely new, or only imperfectly known; there were several particulars likewise connected with them, which were passing fast into oblivion. It is probable that no one, for a long time, will again undertake to look through them so minutely as it was incumbent on me to do, in the discharge of the duty that I had undertaken; and I felt unwilling to leave any call on others to spend their time on what had already occupied so much of mine. Now many of these notices were of such a nature, that, if introduced into the body of the publication, they would have required many notes of explanation sometimes longer than themselves. Again, it was repeatedly found that papers, the whole of which did not require to be printed, admitted of extracts being made from them that were well worth preserving. These were all connected more or less with Bradley's studies and pursuits, and they threw new light on the objects which engaged his attention. It appeared, therefore, that they could be presented to the world in no way, which would make them more clear or more useful, than if they were connected with a narrative of his progress through life. It was impossible, likewise, not to feel a deep interest about such a man, when I was in the daily habit of studying his works, and of being brought, as it were, so much nearer to him, by having them as they came immediately from his own hand. All this naturally led to an inquiry into his personal history; and it was found that there was hardly any thing generally known of it but what was contained in a few pages of the Supplement to the Biographical Dictionary, (8vo. 1767,) and in the Eloge, which was pronounced on him by De Fouchy, before the Royal Academy of Sciences at Paris, in 1762: but these were evidently both drawn up from the communications of those, who were personally acquainted with him.

Seventy years have nearly elapsed since the death of Bradley, and the generation of those who knew him has passed away; some little, however, might be expected to remain in traditionary remembrance. The rapid course of time would soon have swept away that little, and have impaired some of the means, which are still in our power, for understanding what may exist in written documents. It seemed desirable, therefore, to try what might yet be collected; and though it proved to be far short of what could be wished, I indulge the hope of its being authentic and accurate. I have in every instance in my power derived my information from original authorities, and have done my best to verify the facts and dates which are taken from printed accounts: wherever I thought any thing was to be learned, I did not hesitate in making inquiries; and they were uniformly met with a kindness and liberality which claim my warmest acknowledgments.

There are many to whom the expression of my thanks for their assistance will be more suitable, if offered individually; but there are others whose names I ought not to suppress, not only because in some instances my sole pretensions for addressing them were on public grounds, but because in others it will give authority to my statements, when the sources are known from which they are derived.

To the Rev. Daniel Lysons, who passed much of his earlier life in the closest intimacy with his uncle Mr. Peach, who married Dr. Bradley's daughter, I am indebted for an account-book of Mr. Pound[a], from which several interesting particulars were collected:

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[a] Mr. Lysons is likewise in possession of Pound's account of the Massacre at Pulo Condore, and of the watch which Graham made for Bradley—possibly the very watch alluded to in P. 420.

and the communications which I received from him and Rich. Best, Esq.[b] of Greenwich, were of the greatest service in tracing out Dr. Bradley's family and connections.

To J. Shephard, Esq. the Bishop of London's Registrar, I am indebted for the particulars of Bradley's ordination as deacon.

Archbishop Wake's papers are preserved in the library at Christ Church, and by the permission of the Dean and Canons the extracts were made from them respecting the election to the Savilian Professorship of Astronomy.

The Rev. E. Bowlby, of Ilford, gave me his assistance in searching for the house in which Mrs. Pound resided after her husband's death; and the Rev. W. Gilly, Rector of Wansted, so zealously cooperated in this inquiry, that at his request the Hon. Mrs. Rushout was induced to take the trouble of examining her title-deeds, to ascertain the limits of Mr. Wymondesold's property.

To the Earl of Macclesfield my acknowledgments are most especially due: for he not only admitted my repeated applications to him, but gave me all the information in his power, and allowed me freely to make use of the manuscripts in his valuable library.

To Dr. Brewster I am indebted for taking the trouble of inquiring for me into a fact, which I might otherwise have had considerable difficulty in verifying. Wishing to refer, as far as I was able, in every instance to those who were best acquainted with the subject, I communicated with Capt. Kater on the pendulum experiments, and with Sir J. Herschell on the early observations of Castor and γ Virginis. I have to offer my thanks to them for their attention, as well as to the Rev. W. Lax, who upon this occasion, as upon every other for a series of years, has proved to me the warmth and constancy of his friendship. But there is no one to whom I am so much indebted as to the Bishop of Cloyne—I add no epithet to the advice which he gave me—the name of Dr.

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[b] For Mr. Best's connection with Bradley, see p. xi. note [a]: he gave me Bradley's coat of arms, which is engraved under the portrait which is prefixed to this volume.

Brinkley is sufficient to suggest its value to all who have any knowledge of astronomy; but I must express my deep sense of the kindness with which that advice was given, and which increased while it endeavoured to lighten the obligation.

How far due advantage has been taken of such opportunities, it rests with others to decide. I will only observe, that the present publication is not one which was undertaken by choice, for which materials had been already collected, and the composition of which could be carried on at leisure: the task came upon me most unexpectedly, and there were many reasons which militated against delay in the execution of it. I could therefore only build on an imperfect stock of previous information, add what could be learned as I went on, and reduce the whole into the best order that was practicable, during the occasional intervals of other occupations, which had the first claim on my time and thoughts.

These circumstances will account for my duties as editor not having been in some respects more completely executed. I could have wished, likewise, to have reduced the observations, and ascertained their particular results before they went out to the world: but if I had attempted this additional object, the publication must have been deferred to a much later period. I determined, therefore, not to indulge my own gratification at the expense of public expectation; but to satisfy myself with executing what was most useful. I may be contented with having reclaimed and prepared the ground, and I must not impede others who would join in the cultivation of it.

S. P. RIGAUD.

Radcliffe Observatory,
Oxford, Nov. 29, 1831.

CONTENTS.

[An asterisk is prefixed to those articles which have been taken from previous publications. The rest, with the exception of what are noticed at pp. 197, 482, 499, and 510, are now printed for the first time.]

MEMOIRS of Dr. James Bradley.

* A letter to Dr. Edmund Halley, giving an account of a new-discovered motion in the fixed stars .... P. 1
* A letter to George, Earl of Macclesfield, concerning an apparent motion observed in some of the fixed stars 17
* Observations upon the comet that appeared in Oct. Nov. and Dec. 1723 42
* Observations upon the comet that appeared in Jan. Feb. and March, 1737 49
* Observations upon the comet that appeared in Sept. and Oct. 1757 53
* The longitude of Lisbon and the Fort of New York from Wansted and London, determined by eclipses of the first satellite of Jupiter 58
* An account of some observations made in London, and at Black River, in Jamaica, to determine the lengths of the isochronal pendulums in those places 62
* Directions for using the common micrometer 70
* Extracts from Dr. Maskelyne's observations on the latitude of the Royal Observatory at Greenwich 75
* Observations on Tables of Jupiter's satellites 81
* Report on Mayer's Lunar Tables, 1756........ 84
* Report on Mayer's Lunar Tables, 1760........ 86

Description of Molyneux's instrument set up at Kew 93

Observations made at Kew........ 116

Memoranda respecting Bradley's instrument at Wansted 194

Observations made at Wansted 201

Demonstration of the rules relating to the apparent motion of the fixed stars upon account of the motion of light 287

Reduction of the Wansted observations 302

Miscellaneous astronomical observations 339

State of the instruments at the Greenwich Observatory in 1742........ 381

Experiments to determine the length of the pendulum vibrating seconds at Greenwich 384

Memoranda for observing the transit of Venus........ 388

CORRESPONDENCE 391

APPENDIX 511

ADDITIONS AND CORRECTIONS 523

⁂ To save repetitions, the use of the capital P has been confined to references in the Memoirs which are made to the pages of the present volume.

DIRECTIONS TO THE BINDER.

Place the Portrait opposite the title page.
Plate A at the end of the Memoirs, opposite to page cviii.
The autograph opposite to page 134.
Plates I. to V. at the end of the volume.

MEMOIRS

OF

BRADLEY.

CHAP. I.

His Family—Education—Residence with his uncle at Wansted—Early observations—Ordination and preferment in the Church—Election to the Savilian Professorship of Astronomy—Paper on the Comet of 1723—Death of Pound—Paper on the Longitudes of Lisbon and New York.

THE Bradleys are an ancient family, having been settled, as far back as the fourteenth century, at Bradley Castle, near Wolsingham, in the county of Durham. A branch of them passed into Yorkshire, some of whose descendants became stewards to the Guises of Rendscombe, and settled in consequence near Cirencester. This could not have been later than the middle of the seventeenth century, for Lewis Bradley, of Hampnet, near Northleach, married his first cousin Eleanor, whose father was of the same place; and their third son William in 1678 married Jane Pound of Bishop's Canning in Wiltshire.

James Bradley, the subject of these Memoirs, was the third son of this last marriage; he was born at Sherbourn in Glocestershire, and, according to all the printed accounts of him, in 1692; but it is added in the Supplement to the Biographical Dictionary [a], that "the parish church having been rebuilt about this time, no register was kept for some "years, nor can we by any other means trace out his birthday." The registers, indeed, are deficient from 1690 to 1703, but there is good ground, as we shall see hereafter, for believing that the common date is a year too early.

He was educated in the grammar school at Northleach, "under the tuition of Messrs. "Iles and Brice [b]." Hugh Westwood, esq., the founder, placed it under the superintendence of Queen's college, Oxford, and from the registers of that society, it appears that Iles (not Egles, as De Fouchy has written it) was master at this time: Brice was his assistant. Bradley continued under their care till he went to Oxford, and those who are interested in the credit of the school still cherish the tradition of his having been educated at it.

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[a] Pag. 54.

[b] Supp. to Biog. Dict. p. 54.

He was admitted a commoner at Balliol college on the 15th of March 1710-1, being then, as we learn from the matriculation books, in the 18th year of his age, which is clearly incompatible with his having been born in 1692. There seems, indeed, to be no very certain authority for this date, and there is an easy way of accounting for a probable mistake in it. It is well known that previous to the introduction of the new style in 1752 the legal year in England did not begin till the 25th of March. This has occasioned frequent confusions in the first three months of the year. From this very cause Bradley is said, in the printed accounts of him, to have entered at the university in 1710 instead of 1711, which is the real time according to our modern method of reckoning. It will not therefore be difficult to admit that 1692 may in the same manner have been substituted for 1692-3, and if he was born on any of the days between the 15th and 25th of March 1693, he would have been seventeen on the birthday immediately preceding the 15th of that month in 1711. This supposition will likewise agree with his having been in his 70th year at the time of his death, which is recorded on his tomb to have taken place iii Id. Julii A. D. MDCCLXII ætatis LXX.

Bradley proceeded regularly to the degrees of B. A. October 15, 1714, and M. A. June 21, 1717, but much of his time was spent in Essex with his maternal uncle the Rev. James Pound[c]. This kind-hearted and amiable man is not known so well as he deserves to be. Bradley had the small pox in 1717, and it was Pound who took care of him; if money was wanted beyond what his father's limited means could supply, it was the uncle who advanced or gave it to him[d]. These, however, were private benefits:

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[c] James Pound was born at Bishop's Canning in Wiltshire in 1669, and on the 16th of March 1687 he entered at St. Mary Hall, Oxford; on the 27th of February 1694 he was at Hart Hall, and took his first degree; June 6, 1694, he became M. A. He was now at Glocester Hall, and October 21, 1697, he took the degree of B. M. with a license to practise medicine. He does not, however, appear to have continued long in the profession, for he took orders, and went out as chaplain to the settlement in Pulo Condore. This probably took place in 1700. There is a letter of bishop Tanner's in the Bodleian, dated September 1704, in which he says, "My brother Moore "is come home from the East Indies; left our "honest countryman Dr. Pound well . . . . . "he has a mind to come home, but the go-"vernor tells him, that if the Doctor goes, he "and the rest of the company will not stay "behind." Hearne says, "he got much in the "plantations, but lost all in an insurrection of "the Indians." This refers to the destruction of the settlement in 1705, from which Pound was one of the very few who escaped. In July 1706 he returned to England, and in July 1707 he was presented by sir Richard Child to the rectory of Wansted in Essex. February 14, 1710, he married Sarah, the widow of Edward Farmer, esq., by whom he had a son who died immediately after his birth, and a daughter Sarah born September 16, 1713, who died, unmarried, at Greenwich, on the 19th of October 1747. His wife died in June 1715. In January 1720 he was presented by lord chancellor Macclesfield to the rectory of Burstow in Surrey, which had become vacant by the death of Flamsteed. In October 1722 he married Elizabeth, sister of Matthew Wymondesold, esq., a fortunate speculator in the South Sea scheme, who was then possessed of the property at Wansted, which now belongs to the honourable Mrs. A. Rushout. With two livings and his wife's fortune Pound must now have been in affluence, but he did not long enjoy it, for he died November 16, 1724.

[d] These particulars are collected from the private entries in Pound's account book.

a more weighty debt of public gratitude is due to him for having cultivated Bradley's extraordinary talents. Pound, at that time, was one of the best observers in England; Halley used to apply to him for assistance, and Newton made use of the corrections which he supplied for determining the places of the comet of 1680[e]. If he did not originally implant the fondness for astronomy in the mind of his nephew, he certainly fostered it; and by associating him with himself in his favourite pursuit, he communicated that skill which by long experience he had acquired. It is pleasing to trace the handwriting of each as they have entered their observations one after the other on the same paper, and to think of the increased zeal and delight which both must have derived from this union of their studies. With reference to these joint exertions the following letter[f] from Halley to Pound possesses considerable interest.

“ London, Sept. 6, 1716.

“ Dear Sir,

“ Entreating you to pardon the frequent trouble I give you, these
“ are to let you know that by the distances you took the other night I find the place of
“ the nebula in Hercules to be ♏ 25° 6′, with the latitude 58° 1′, whereby it appears
“ that it precedes π of Hercules 33′¼ minutes of time with very little difference of declination.
“ That in the foot of Antinous I find to have long. ♑ 8° 55′½, with north
“ latitude 16° 37′½, preceding the bright foot of Antinous (λ Bayero). Bayer has two
“ stars preceding λ, to which he has put no letters, but they are *i* and *k* in Mr. Flamsteed's
“ Catalogue; the preceding of which *k* follows the nebula 6 minutes of time, and
“ is 22′½ minutes more northerly than it. By these Mr. Bradley may at his leisure examine
“ the above situations of the nebulæ, which are fitted to Mr. Flamsteed's epocha,
“ or anno ineunte 1690.

“ On Saturday next, Sept. 8^{vo}, about 5 in the morning, (if you please once more to
“ verify the places of the stars in the beginning of Virgo, to which the comet was applied
“ in November 1680,) you will find Mars close to the two contiguous *d* and *e*, in
“ Transactions N°. 342, which are but 7 minutes asunder. According to my calculus
“ I expect him nearer to each of them than they are to one another, and about 4 minutes
“ more northerly than they, which have precisely the same declination. I fear it
“ will be too light for me to see so small stars, when Mars gets over the houses, or you
“ should not have had this trouble from, kind Sir,

“ Your much obliged, faithful servant,

“ EDM. HALLEY.”

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[e] There are two curious entries in Pound's account book:

“ 1719 July 13, to a free gift rec^{d}. from Sir
“ I. Newton 52*l.* 10*s.*

“ 1720 April 28, to a gift rec^{d}. of Sir I. Newton
“ 52*l.* 10*s.*”

It is well known that Newton was extremely liberal, and it is very probable that these sums were presented in acknowledgment of the assistance which he had derived from Pound.

Among Bradley's papers there are the original observations by which Pound determined the relative positions of the stars in *sinistro calcaneo Persei*, which are referred to in the Principia, and it appears that the measures of their distances from one another were taken in September 1716.

[f] The original is preserved in the Observatory at Oxford.

How soon Bradley began to make entries of his observations cannot now be ascertained: the earliest which have been found are dated in 1715. In the 30th vol. of the Philosophical Transactions, Halley published two made in 1717 and 1718; he describes him there[g] as "eruditus juvenis, qui simul ingenio et industria pollens his studiis promo-"vendis aptissimus natus est:" it is probable, therefore, that this was the first occasion on which his labours had appeared before the public. At the end of the same volume there are other observations[h] which we now know also to have been his, although they are not distinguished from the rest which his uncle had communicated to the Royal Society.

The next mention which we find of him is made again by Halley in the 31st vol. of the Phil. Trans.[i] In some remarks which he made on Cassini's method of observation, by applying threads at half right angles in the common focus of the telescope, he speaks of the exactness with which "Dr. Pound and his nephew Mr. Bradley did, my-"self being present, in the last opposition of the sun and Mars, this way demonstrate "the extreme minuteness of the sun's parallax, and that it was not more than 12″ nor "less than 9″." These observations will be found in p. 353 of the present collection; they were all entered by Pound, but the calculations made from them are still in existence, and they are almost entirely in Bradley's handwriting. Halley called the attention of astronomers to the opposition which would take place in the following October, (1721), and we find that Bradley took advantage of it to repeat the observations on the apparent motion of the planet. These will be found at p. 350; they are much more numerous than his uncle's, and they enabled him to find the sun's mean parallax still nearer to the truth by 1″.2.

Wallis, in a very curious paper[j] on the parallax of the fixed stars, pointed out the use which might possibly be derived for determining the question from observations of stars which were very close to each other. With this view, Bradley seems to have directed his attention most particularly to Castor[k], and we find that he has noted the relative position of the two stars of which it consists on March 30, 1719, and October 1, 1722. The method by which he determined their position did not admit of very minute accuracy, but, if the mean of both observations is taken, the result will agree in a most extraordinary manner with the motion which has been found to exist in them by Mr. Herschell and sir J. South[l].

On March 12, 1718, we find that he also repeated and confirmed an observation which his uncle had made the night before on the double star γ Virginis. The position of these stars, as given in the Phil. Trans. for 1824[m], points out that their angular velocity of revolution has been constantly increasing, and this will agree very well with the data which Pound and Bradley have transmitted to us. Could we likewise have known how far the stars were at the same time apart, we should probably have found a confirmation of another very curious fact, for most of the later observations indicate that the distances have diminished while the angular velocity has increased. This may be the consequence of our looking obliquely on the plane of their orbit, but should future observations prove that one of the stars is situated in the focus of an ellipse which the other describes about it, we shall have a most beautiful example of the effects of gravitation in the simplest case of its action on two bodies without any perturbations from the influence of a third.

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[g] Pag. 853.

[h] 1719. April 22, May 16.

[i] Pag. 114.

[j] Phil. Trans. vol. XVII. p. 844.

[k] Phil. Trans. 1803. p. 363.

[l] Phil. Trans. 1824. p. 105.

[m] Pag. 172.

It is not, indeed, among the earlier observations, but as connected with the present subject, we may take this opportunity of preserving a memorandum of Bradley's which was found on a loose paper. It is as follows: "By Mr. Flamsteed's observa-" tions, Dec. 9, 1689, and Dec. 3, 1690, the small star that precedes the pole star was " farthest from the pole then by 18′ 9″. In 61 years that difference should be $4''\frac{1}{2}$ less, " therefore in 1751 the difference ought to be $18'\ 4''\frac{1}{2}$. By our observations 'twas $18'\ 1''\frac{1}{2}$ " or but 3″ less. Hence 'tis reasonable to conclude that neither of these stars have any " particular motion distinct from the other."

Bradley from his earliest application to astronomical observations seems to have directed his attention very particularly to the places of Jupiter's satellites. He was for many years in the habit of calculating each eclipse which he observed, and marking the quantity against the observation of it by which it differed from the times which were derived from the Tables. Indeed the correction of the Tables seems to have been one of the many objects to which he was led by his uncle, who had himself drawn up a set for the conjunctions of the first satellite. Pound's Tables were founded on Cassini's of 1693, the corrections being introduced which subsequent observations had shewn to be necessary. There were, indeed, other improvements, but the most remarkable is the introduction of that admirable arrangement by which all the equations are made additional. They were communicated to Halley, who not only printed them in the Phil. Trans. [n], but likewise inserted them in his collection of Tables, where they form an Appendix to the general Tables for the four satellites which had been given him by Bradley. The corrections introduced into these last were not derived from theory, but from the quantities which were found to be necessary to make them best agree upon the whole with observation: there was no occasion, therefore, for reprinting them now that the use of them is superseded. The remarks, however, which Bradley drew up to accompany them are inserted in the present collection, because they contain some curious notices of what he had observed in the motions of the satellites. They were originally written in Latin, as they appear immediately prefixed to the Tables; but the translation of them has been preferred, which was inserted in the precepts drawn up by Dr. Bevis when the whole was published in 1752. It is very faithful, and in one or two places seems to express the meaning more perspicuously than the Latin. There can be very little doubt that, if not made by Bradley himself, it was at least sanctioned by his approbation, for Dr. Bevis, when engaged in this publication, was in constant intercourse with him, and frequently consulted him on the several parts of it.

Hodgson of Christ's Hospital published Tables of Jupiter's satellites in 1749, in which he seems studiously to avoid all notice of Bradley, with whose previous labours there cannot be the slightest doubt of his having been intimately acquainted. More justice was done to him by Bailli [o], who gives him credit for having detected the greater part of the inequalities which had been since recognised in the motions of Jupiter's satellites.

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[n] Vol. XXX. p. 1023.

[o] Essai sur la Théorie des Satellites de Jupiter 1766. p. xxxi.

Cassini had first admitted and then rejected Roemer's suggestion with respect to the time to be allowed for the passage of light across the earth's orbit: Pound introduced an equation for it, and so did Bradley, but Delambre[p] expresses some astonishment at his having contented himself with 14′, at which it had been taken by Cassini, instead of making this part of his Tables more accurate, by calculating from the results of his own discoveries. It must however be recollected, that the aberration of light was not detected till 1728, and that Halley's Tables, though they were not published before the middle of the last century, were all printed off in 1719.

Bradley had now become known, and was justly esteemed among men of science. On the 6th of November 1718, he was elected fellow of the Royal Society; but it could not be foreseen that his astronomical labours would lead to any establishment in life, and it became necessary for him to embrace a profession. [q] He had been brought up for the church, and had applied himself to those studies which might qualify him for it, but he did not take orders as soon as he was old enough to be admitted to them. The vicarage of Bridstow near Ross in Monmouthshire, becoming vacant in 1719, Hoadly, who was then bishop of Hereford, gave him the presentation to it. On this title he was ordained deacon on the 24th of May, by Robinson bishop of London, and if any additional proof were wanted of his habitual residence with his uncle it would be found in the testimonials which he procured on this occasion, for they were signed by Pound, by Chishull vicar of Walthamstow, and by Chisenhall vicar of Barking, who state that he had been known to them for the six preceding years[r]. On the 25th of the following July bishop Hoadly admitted him to priest's orders, and two days after instituted him to Bridstow[s]. In the beginning of the next year he had some addition to his preferment. Llan ddewi felfry in the hundred of Narberth and county of Pembroke is a divided living. The rectory is the first portion and a sinecure. The patronage of it belonged to the prince of Wales, and it was procured for Bradley by his friend Samuel Molyneux[t], esq. who was secretary to his Royal Highness. His income from both these pieces of preferment could not have been large, but the duties of a clergyman are not to be measured by the emolument which he derives from them, and fortunately in this case they were of a description which called for less of his time than otherwise might have been required from him. A sinecure of course could not be the cause of much interruption, and as the population of Bridstow in 1801 did not exceed 471, it is probable that early in the preceding century the number of parishioners in it was much smaller. Bradley therefore continued his visits to Wansted as much probably as he could, and this may have occasioned the belief of his having been the curate of that parish[u]. In Pound's account book, however, there are no traces of any thing like payments to him as a stipendiary assistant. There are several entries of receipts through his hands of dues for the offices of the church, but these prove no more than that occssional assistance in which, when absent from his own cure, he might wish to be professionally employed.

The time, however, was now at hand which enabled him to give himself up entirely to the pursuit of science. The Savilian Professorship of Astronomy in the University of Oxford became vacant in August, 1721, by the death of Dr. John Keill. Lord chancellor Parker (afterwards earl of Macclesfield) was one of the electors, and was anxious in the first instance for Pound's being appointed to the vacant office, considering him as "the fittest man perhaps in Europe[x]." Indeed it seems to have been generally understood that he would have been elected[y] if he had not been unwilling to give up his preferment in the church, with which, by the statutes of the founder, the professorship was untenable. When he declined, the Rev. Jos. Atwell, fellow, and afterwards rector, of Exeter college, was spoken of, although it does not appear that he ever declared himself a candidate; Gagnier the orientalist made direct application to archbishop Wake; and much interest was exerted for the Rev. J. Whiteside, F. R. S. of Christ Church, a man of consideration among his contemporaries, and who had been a candidate when Keill was elected. The lord chancellor, however, now exerted himself for Bradley, who was indebted through life to the constant friendship and patronage of the earls of Macclesfield. On the present occasion, however, he had other supporters; and we may judge of the estimation in which he was held from the following letter to archbishop Wake from Martin Foulkes, who was afterwards president of the Royal Society.

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[p] Hist. de l' Astr. au xviii siecle, p. 421.

[q] Hist. de l' Ac. R. des Sciences 1762, p. 231.

[r] Reg. Dioc. Lond.

[s] Supp. to Biog. Dict p. 54.

[t] Suppl. p. 55.

[u] Suppl. p. 55.

"London, Sept. 4, 1721.

"My Lord,—I humbly take the liberty of applying to your grace on the occasion "of the vacancy at Oxford in the Astronomy chair, occasioned by the decease of the late "Dr. John Keill, in behalf of Mr. Bradley, a gentleman who is thoroughly qualified for "that part, and will be, I make no doubt, every way recommended to your grace's satis"faction. Your grace, I am informed, has the first vote, and the calling the election; "besides that, your countenance will be of the greatest weight with the other electors. "Mr. Bradley was of Oxford, and is five years master of arts: he has lived for some "years with his uncle, Mr. Pound of Wansted, where he has had great opportunities of "joining to his theory the practical part of astronomy, in which he has made himself "very eminent, having prepared for the press accurate Tables of the satellites of Jupiter, "with some other curious pieces; and I am satisfied his being professor will do honour "and service to the science. I shall only take the liberty of adding, that he is perfectly "approved, and will be entirely recommended by Sir Isaac Newton, whom your grace "knows for the great judge of this sort of learning. I most humbly ask your grace's "pardon for this liberty, and begging your blessing, desire the honour of subscribing "myself,

"My lord,

"Your grace's most dutiful nephew and most obliged

"M. Foulkes.

"Mr. Bradley is fellow of the Royal Society, but not the same as has published some "late books on gardening, &c."[z]

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[x] Abp. Wake's papers.

[y] Hearne, N°. 92. p. 146.

[z] This was Richard Bradley, the first professor of botany at Cambridge in 1724. The caution was not unnecessary. In the Index to the Phil. Trans. the very first article which is mentioned under the head of James Bradley belongs really to Richard.

Under these auspices, success could not be doubtful, and his election accordingly took place on the 31st of October, 1721.[a] This laid him under the obligation of resigning his livings. The immediate loss could not have been very great, but the sacrifice of future prospects must have been considerable. With such friends as Molyneux and lord Macclesfield, and such a patron as Hoadly, to whom he was now chaplain, there could be no doubt of great and rapid advance in his profession. But he abandoned all for the sake of science. De Fouchy[b] says, "Son cœur vraiment droit souffroit depuis long tems de se voir "partagé entre ses devoirs et son inclination, et il saisit avec empressement l'occasion de "se délivrer de cette contrainte." Now the time required for two different objects may occasion them to interfere with each other, without there really being a distaste for either; and it seems unjust to attribute Bradley's conduct upon this occasion to any aversion from his profession[c]. We have no proof of such an aversion having been felt, and certainly in the nature of his pursuits there was nothing which should lead us to a presumption of it. Paley[d] remarks, that "astronomy is not the best medium through "which to prove the agency of an intelligent Creator, but this once proved, it shews, "beyond all other sciences, the magnificence of his operations. The mind which is once "convinced, it raises to sublimer views of the Deity than any other subject affords." Such a pursuit cannot be uncongenial with the professional habits of a clergyman. In Bradley, however, it had grown into an all-absorbing passion, and although he was of that kind and gentle disposition which is peculiarly adapted to parochial cares, he determined to sacrifice all his prospects rather than continue in a situation where he found himself continually drawn off from the regular discharge of its duties. He seems, indeed, to have been actuated by a deep sense of the sacredness of those duties, and this feeling appears through life to have been strongly impressed upon his mind. For when the vicarage of Greenwich, which became vacant in 1751 by the death of the Rev. R. Skerret, was offered to him by his friend Mr. Pelham, who was chancellor of the exchequer, although he was then resident in the parish, he honourably refused the preferment, because "the "duty of a pastor was incompatible with his other studies and necessary engagements[e]." The usefulness however of these studies and engagements ought not to be forgotten, nor the close connection between moral and intellectual improvement, between revealed and natural religion. Instead therefore of hastily admitting unworthy motives for Bradley's disinterested conduct, we should feel grateful for the services which it enabled him to render to mankind, by laying open some of the grandest operations in nature, and extending our knowledge of those laws by which the Creator regulates the universe.

Though elected in October, Bradley was not admitted to his office till Dec. 18, 1721,

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[a] In the account book there are the following entries:

1721. Sept. 2.	By coach hire, pocket expenses, &c. about the Oxford professorship	2	12	0
Oct. 31.	By cousin Bradley lent him........	4	4	0
	By pocket expenses in London, &c. ..	0	11	3

It is probable, therefore, that Pound accompanied his nephew to London on the day of election, and we find from another part of the accounts that the four guineas were supplied "to give to the door-keepers of the house of "lords."

[b] Hist. de l'Acad. des Sciences 1762. p. 233.

[c] See Biog. Brit. 1766. vol. VI. Sup. p. 257.

[d] Natural Theology, chap. xxii.

[e] Sup. to Biog. Dict. p. 57.

and on the 26th of April in the following year he read his inaugural lecture. This, according to Hearne[f], did not add to his reputation; but Hearne was no judge of the subject: and, although he only set down the reports which reached him, he had very strong prejudices; and being intimate with Whiteside, was not inclined to form a favourable opinion of the person whose success had been the cause of his friend's disappointment. This lecture is still in existence, and is a respectable composition; but it could not add to the high name which Bradley afterwards established, and contains nothing which can call for its publication. The same remark applies to other Latin lectures which he afterwards delivered in the university.

Reflecting telescopes had been invented in the middle of the seventeenth century; but the mechanical difficulty of perfecting the mirrors was long an obstacle to the use of them. Hadley applied himself to overcome it, and in 1723[g] communicated to the Royal Society an account of his success, making them a present of one of his instruments. This Pound compared with the Huygenian telescope of 123 feet, and he tells[h] us that although the focal length of the object-metal was not quite $5\frac{1}{2}$ feet, it bore an equal magnifying power, and represented an object as distinctly as the refractor, although not altogether so clearly and bright. Bradley assisted him in the comparison, and, encouraged by what he saw of it, applied[i] himself afterwards to the construction of mirrors; but it appears from his communication to Dr. Smith, (inserted in the correspondence at the end of this collection,) that having[k] been taken off from this pursuit by other occupations, he did not return to it. He had however the great advantage of being himself a workman, and we find him making repeated mention of having cleaned his clocks, or having fitted up and repaired his instruments.

Halley[l] records the observation of the transit of Mercury over the sun's disc, on Oct. 29, 1723, which will be found much more completely detailed in p. 355. The measurements were made with the Huygenian glass, which has just been mentioned: Derham, in the preface to his Astro-Theology, speaks of its having been lent by the Royal Society to Pound, who fitted it up at Wansted in the spring of 1718[m]. Huygens'[n] own contrivance for making use of these long object-glasses is much praised by Smith[o]; who says, that it was " successfully practised by Dr. Pound, and his nephew Mr. James " Bradley." We have indeed an extraordinary instance of Bradley's skill in the management of these instruments, for we find him measuring the diameter of Venus on the 27th Dec. 1722[p] with a telescope of $212\frac{1}{4}$ feet. There is no error in the figures, the observation has been entered twice with exactly the same numbers in both places. The value, likewise, of each revolution of the micrometer screw may be considered to vary inversely, as the focal length of the object-glass; and in the present instance, we have 29 revolutions, which for the glass of 123f. would, by a MS. table of Pound's, have been equal to 1′ 39″,76; but the quantity given for reduction of the observation is 57″,8, which is precisely equal to $99'',76 \times \frac{123}{212,35}$. The Royal Society also is in possession of several old object-glasses, and the focal length of one of them (made by Huygens, and given in 1686 by the rev. Mr. Burnet) is described to be 210f. It is highly probable that this is the identical glass which Bradley used, and as the planet was not, at the time, 12° above the horizon of Wansted, a pole of about 45f. would have been sufficient for the support of it.

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[f] N°. 94. p. 61.

[g] Phil. Trans. vol. XXXII. p. 303.

[h] Vol. XXXII. p. 382.

[i] Smith's Optics, §. 782.

[k] P. 401.

[l] Phil. Trans. vol. XXXIII. p. 229.

[m] The date is taken from Pound's Account-book, in which we find the following entries:

1717, Sept. 18. by tin and brass work for Hugens's telescope . . . . 0*l*. 4*s*. 6*d*.

1718, April 25. by an eyeglass for the long telescope . . . . 0*l*. 2*s*. 6*d*.

May 13. by drink for men who raised the pole 2 0 0

May 16. by several men, paid them half a day's work for assisting in raising the pole 0 17 0

[n] Opera Varia, vol. I. p. 261.

[o] Optics, §. 892.

[p] P. 354.

On the 9th of October 1723, Halley discovered a comet, and immediately communicated the intelligence to Bradley, who saw it the next night, and continued with Pound's assistance to observe it at Wansted till the 14th of November; his affairs then called him to Oxford, where he had no convenience for continuing his observations. This he alludes to in an academical lecture, which he delivered " de cometa" on the 28th of the same month, mentioning the advantages which he had for observing at Wansted " nactis " instrumentis accuratissimis...qualia (uti spero) in hac illustrissima scientiarum sede " academiæ hujusce gubernatorum cura et munificentia ad scientiam astronomicam olim " consecrabit."

He did not satisfy himself with recording the general appearances which he had observed, or with forming conjectures on them. He combined them together, and calculated the elements of the comet. These, with the original observations, form his first contribution to the Philosophical Transactions. The paper[q] is reprinted at p. 42, of the present collection: it contains the parabolic path, calculated after Newton's method; and this must have been done immediately, for the elements[r] are the same (only detailed with more minuteness) as he had already stated in his public lecture at Oxford. He did not attempt to determine the true ellipse, considering that the observations were too limited, and that there was no inducement to undertake the additional labour from a similarity in this comet to any one of which the particulars had been previously recorded. The determination of his problem may not appear at present to be one of very great difficulty, but in the early part of the last century it was not so familiar as it has since become to astronomers. Bradley's calculations are extant, and they fill thirty-two pages of foolscap for the present comet: in the same book also there are nearly sixty pages full of numbers for the determination of the elements of the comet of 1737; and in another book there are seventeen employed on the comet of 1707. These facts would alone be a sufficient reply to the suggestion, " qu'il[s] calculait peu." Mr. Delambre had indeed a strong leaning to what he describes as the seductive[t] luxury of calculation, and he had not the immediate evidence which we possess of Bradley's indefatigable exertions: he probably did not know that his health was seriously injured by the computations, in which he was afterwards engaged for the purpose of examining and correcting Mayer's Tables, but still it might have occurred to him that no trifling effort was required in those times to make out the orbit of a comet. Bradley, as Delambre himself particularly mentions[u], was the first who in this respect attempted to follow the steps of Halley; and Newton certainly did not think lightly of his doing so. He mentions him among those from whom he had derived the few additions which are enumerated in the short preface to the third edition of the Principia. Indeed, in the 42d Proposition of the third book, after enlarging on Halley's labours, and pointing out the support which they afforded to his own view of the nature of comets; he adds, "confirmatur etiam theoria per motum retrogradum "cometæ, qui apparuit anno 1723.—computante D. Bradleio, astronomiæ apud Oxoni-"enses Professore Saviliano[x]."

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[q] The original manuscript of this paper is preserved in the library of the Observatory at Greenwich.

[r] P. 46.

[s] Delambre Hist. de l'Astr. au xviii siecle, p. 425.

[t] Astronomie, vol. III. p. 212.

In 1724, the rev. J. Pound died on the 16th of November. In him Bradley lost not only the companion of his studies, but also a "relation to whom he was dear, even "more than by the ties of blood[y]." It has been supposed that Bradley inherited his property, but that is certainly erroneous. His death was probably sudden, or after a short illness, as the last entry in his Account-book contains the expenses of a "journey "to town" on the 9th. At all events he left no will, for it appears from the records in the Prerogative Office that his widow administered to his effects, which of course belonged to herself and the only child who survived him, the daughter of his former marriage. The family, however, was not separated by this melancholy event; and, with the exception of the long Huygenian telescope, we find Bradley continuing to observe with the same instruments[z] which his uncle made use of. These comprised a transit, a reflector[a] constructed by Hadley, the 15f. glass which was most used, and one of 7f.; mention likewise is made of a 10f. and 11f. glass; besides these, "Mr. Graham's instrument" is spoken of, which appears to have been used in 1727 and 1731 for observing equal altitudes.

In 1726, Bradley communicated a paper to the Royal Society, in which he determined the longitudes of Lisbon and New York, from eclipses of Jupiter's first satellite. This is reprinted at p. 58 of the present collection. The most remarkable part of the dissertation consists in the calculations, which were made from observations which were not simultaneous; the quantities, which were allowed for the several intervals, being ingeniously derived from what he had himself remarked in the motion of the satellite.

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[u] Hist. de l'Astr. au xviii siecle, p. 189.

[x] Twelve copies of the Principia were printed in 1726, on a very large thick paper. They seem all to have been originally bound with gilt leaves in red morocco, to a pattern which was much used for the Harleian Library. One of these magnificent books was presented by Newton to Bradley: it came into the possession of the Rev. G. Powell, of Balliol coll., who left it, with his other mathematical books, in 1830, to the Observatory at Oxford. The letter from Halley to Pound which is printed at p. iii. was found in it.

[y] Sup. to Biog. Dict. p. 55.

[z] The pecuniary value of these was not very great. In the account book there is a loose paper of Bradley's which was written after Pound's death, and contains an account of his personal property: one of the items is as follows. "By mathematical instruments and "glasses, 25*l.*"

[a] Bradley's youngest sister Rebecca, married John Dallaway, (whose grandson the Rev. J. Dallaway, is secretary to the Earl Marshal, &c. &c.) their daughter Mary married Richard Best, whose son, Richard Best, Esq. of Greenwich, is in possession of the 5 feet Newtonian reflector which was used by Bradley, and of a quadrant which belonged to him, 18 inches in radius.

CHAP. II.

Paper on Aberration—Parallax of the Fixed Stars—Hooke's Observations—Molyneux's Instrument at Kew—The motion observed in γ Draconis—Picard's Observations at Uranibourg—Examination of the laws of the star's motion, and speculations on the cause of it during the year 1726—Solar Nutation, Refraction, Parallax.

IN 1729, Bradley communicated to the Royal Society his paper on the aberration of light. Trifles derive an interest from a connection with such a subject. It may not therefore be impertinent to state, that a wrong year has been universally assigned to this paper, from its having been printed in the Philosophical Transactions for 1728. Such irregularities are not uncommon in the earlier volumes, but the paper was not read before the Society till the 9th and 16th of January 1729, and one of the manuscript copies of it which are still extant, is dated Jan. 1, 1728-9. The historical part of it is painfully concise. Philosophy has few nobler or more instructive objects of contemplation, than the successful efforts of man's inventive faculties. In the present instance, we naturally feel anxious to observe all the various workings of a mind which seized the clue as soon as the scarcely perceptible extremity of it presented itself, which with unremitting diligence pursued the path to truth, and gradually subdued every difficulty, till the whole was brought out in that clear and brilliant light which leaves us almost in wonder at its having remained so long in obscurity[b]. He made us as it were even independent of himself. We now stand no longer in need of his observations to determine the maximum of aberration, or of his assistance in demonstrating the rules for its application: for such objects the present publication would be superfluous, but for the history of astronomy it supplies a link which is not only of intrinsic value, but has double interest from the fear which was once felt of its most important parts having been irrecoverably lost[c].

When Copernicus first published his system, it was recommended by the beautiful simplicity of its regular construction: but notwithstanding the support which it derived from the splendid discoveries of Galileo and Kepler, a wish was still felt for farther facts, and for some additional proof of the earth's motion in its orbit. The question, therefore, of a parallax of the fixed stars, arrested the peculiar attention of astronomers in the middle of the seventeenth century, and Hooke made one of the earliest attempts to ascertain it by direct observation.

The first proposal of his plan seems to have been offered to the Royal Society in 1666[d]: this was renewed in the beginning of the following year, and he was then exhorted by the president to pursue it with all convenient speed[e]. He did not however begin his observations till 1669, nor did he print the account of them till 1674; when he published his "Attempt to prove the motion of the earth from observations." Hooke shewed much originality in this, as he did in most of his projects; and although he certainly failed in attaining his object, the failure was in consequence not so much of any errors in his views, as of the impossibility at that time of carrying them into execution.

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[b] La Place, Systeme du Monde, p. 367.

[c] Delambre, Hist. de l'Astr. au xviii siecle, p. 419.

[d] Birch's Hist. of the Royal Society, vol. II. p. 98, 109.

[e] Birch, p. 139.

To avoid the uncertainty which might be introduced by refraction [f], he determined upon observing γ Draconis, which passed near the zenith of Gresham college[g]. For this purpose he cut an opening[h] in the roof of his house, and fixed a square tube in it, (see fig. 1. plate A.); a second square tube carrying the object-glass moved in the first, so that it could be adjusted by means of a screw, which retained it in its proper place. (see fig. 2.) The focal length of his glass was thirty-six feet, and as so long a tube would have been attended with inconvenience, he merely cut a hole through the floor at r, r, and confined his observation to the measure, with a micrometer, of the distance at which the star passed from the centre of his field of view. That this might be accurately adjusted to the zenith, he had a small bar at a, a, that crossed directly under the centre of the aperture of the object-glass, in which were drilled two small holes at equal distances from the middle of the glass, and through them were fixed the upper ends of threads of silk d, d, which were stretched with leaden weights. Care was taken to make the bar lie as exactly north and south as could be, and by means of these plumblines the eyeglass f and the micrometer, by which the deviations of the star from the zenith might be measured, were fixed in their proper places.

He gives an account of only four [i] observations, but he considered them as proving[k] the existence of a parallax in the fixed star. His pamphlet however consists of one of his Cutlerian lectures which he read in 1670, and when he was afterwards exhorted [l] by the Royal Society to prosecute the inquiry, he spoke with much more hesitation respecting the result of it.

Flamsteed's well-known letter [m], in which he deduces the parallax from his observations of the pole star, was liable to serious objections, which were pointed out by Cassini [n]. Nothing therefore satisfactory [o] had been ascertained on this important point, even in the beginning of the eighteenth century, and the world is indebted to Molyneux for having resumed the inquiry. He was a gentleman of fortune much attached to science, and particularly to astronomy. In 1725, he was living at Kew, and Graham made him an instrument for this especial purpose. Bradley mentions it [p] as "constructed upon almost the same principle" as Hooke's. There was however a strong distinction between them, for Hooke's, as we have seen, had no tube, and the axis of it was always intended to be directed to the zenith; whereas Graham's plan was to direct the axis of his instrument constantly to the star, so that the deviations might be determined by the angles through which it might be necessary for the telescope at different times to be moved. For this purpose the glasses were fixed in a tube[q] made of strong firm tin plate, very carefully and closely soldered together, which moved on an horizontal axis[r] at the top of it. The whole was supported by some iron-work, which was fixed very strongly to a “ [s]large stack of brick chimneys, which were quite within the house, “ and scarce at all exposed to the weather, and were very strong old built chimneys[t], “ some part of the house being near three hundred years old. [u]On the north side was “ fixed—a small brass screw which—touched with its point, which was directed southward, “ —the lower end of the tube—and the telescope was gently pressed against the point of “ this screw," being drawn northward by a string passing over a pulley, with a small weight at the lower end of it. As it must naturally be supposed that the pulley would be attached to the wall, it will follow from this description, that the instrument was not on the north side of the stack of chimneys. Neither was it hung on the east or the west side: for the stool, on which the observer lay, was always placed to the west[y] of the telescope; and the shelf on which the lamp was placed to illuminate the cross hairs, extended five feet to the east of it[z]. The whole appears likewise to have been placed in a room on the south side of the house; for in order to set the upper axis duly east and west, a meridian line[a] was drawn by the sun above in the garret, while its transit over the meridian was carefully observed by an instrument, made use of for that purpose in the library below.

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[f] Attempt, &c. p. 13, 15.

[g] The observations were not made in the apartments to which Hooke was entitled as professor of geometry, but in those belonging to the professor of astronomy in which he then lodged, as his own were taken up for the use of the city.—Ward's Lives of the Professors of Gresham College, p. 176, note [a].

[h] Attempt, &c. p. 17.

[i] Attempt, &c. p. 23.

[k] Attempt, &c. p. 25.

[l] Birch, vol. II. p. 478.

[m] Wallis's Opera, tom. III. p. 701. In the library of Corpus Christi college, Oxford, there are some original letters of Wallis, in which he urges Flamsteed to let him publish the account of this supposed discovery: from one of them, it appears that Wallis translated it himself into Latin.

[n] Mem. de l'Ac. des Sciences 1699. p. 177.

[o] Rowley is said by Lalande (Astron. §. 2802.) to have been desirous of placing an object-glass in one of the towers of St. Paul's, in order to make observations on parallax, and to have been prevented by Newton, who, as he says, "craignit que le bâtiment venant à changer ne "dérangeât la situation de la lunette." Now this could not have taken place unless the glass had been set in a telescope which was fixed to the walls; and as it would in that case have been Rowley's plan to follow Hooke's method of observation with a micrometer, the objection was perfectly valid. There is no occasion for imagining, with Lalande, that extraneous motives may have operated upon Newton's mind to make him desirous of creating difficulties.

[p] P. 1.

These particulars, by a fortunate concurrence of circumstances, are sufficient to mark the precise spot on which the instrument was erected. Molyneux[b] married lady Elizabeth Capel, and lived in a large mansion at the western extremity of Kew Green, which was the property of her family: about 1730, or soon after, Frederic Prince of Wales took a long lease of it; and on the death of the Princess Dowager, it became a residence of his late Majesty King George the Third. The elevation of the south or garden-front has been copied in the vignette prefixed to these Memoirs. It will be seen that there were two lower wings at the two extremities, but the telescope could not have been set up in either of them, for the focal length of the object-glass was[c] 24 feet 3 in. and the eyeglass was $3\frac{1}{2}$ feet above the ground floor[d], which would require an elevation of nearly 28 feet, while the ridge of these roofs did not rise more than 23. Besides, it is clear from Molyneux's own description, that the chimney was within the body of the house. Now sir W. Chambers in 1763 published very particular plans and drawings of this house, from which it appears that the only chimney, on the ground floor, which faced the south, was in the eastern of the two compartments which flank the centre of the garden-front. The structure of this part will likewise answer in other particulars: the length of the instrument made it necessary to cut holes for it through the floors[e] of the house, and in this compartment the second ceiling from the ground was at the height of 23f. 10in. The top of the telescope must therefore have reached into the uppermost story, and yet it would not have been too high for a shelf of five feet[f] to have been placed between it and the sloping roof. No southern window indeed appears in this garret, for the meridian line to be drawn as it is described in p. 100; but this difficulty is not insuperable. All the views represent the front of a modern building, which is very different from the appearance it must have had in the beginning of the last century. When the house was taken down in 1802, it was found to have been originally built of red brick, worked in ornamental grooves and patterns: over this, wooden planks had been fastened, on which a smooth coating of stucco had been laid. No tradition could be traced among the old servants of the palace, by which any thing could be ascertained with respect to the garrets, which do not seem to have been inhabited of later years. Unfortunately the immediate object of the present inquiry did not present itself soon enough. If the building were now standing, it is not improbable that the traces of garret windows might be found in the south wall, and it certainly is not clear that they may not have been covered by the battens. It also is evident, that any objection derived from this circumstance will apply with greater force to any other part of the house. In the centre, the upper windows belong to rooms which cannot with any propriety be called garrets; and it appears[g] that the top of the instrument was immediately under the roof of the building[h].

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[q] P. 93. [r] P. 95. [s] P. 94.

[t] At p. 139, it is said, that close, warm, or damp weather seemed to bring the tops of the chimneys southward; and dry, clear weather seemed to bring them northward. The same remark is repeated in other places, but it is very difficult to understand how this effect could be produced on such chimneys as are here described. It is much more probable that it was the consequence of some alterations in the iron-work which supported the instrument, and which, being fixed in a wall facing the south, would have carried the top from it in warm weather, and brought it back to the northward when contracted by cold.

[u] P. 97.

[x] Some general idea of the structure of the instrument may be collected from Smith's drawing of it, which is copied in Plate II. fig. 5. Bradley certainly thought of publishing a detailed account. He mentions this particularly in the early part of the printed paper on aberration, and in a manuscript copy of the first part of that paper, he enters more fully into this description; but even there he breaks off by saying, “ it will be too foreign to “ my present purpose to give you a particular “ description of all the parts of this curious in- “ strument, since it could not be easily done “ without several draughts; but if what I now “ send you shall be thought worth communi- “ cating to the curious, I may hereafter give “ them a more particular account, not only of “ this, but also of my own instrument." As Molyneux put the description into his hands, which is now printed, (at p. 93.) with permission to make any use he pleased of it, (see p. 158.) the plates which were required to accompany the publication, were probably the principal obstacle. The Philosophical Transactions in those days, were published by the secretaries, and not by the Royal Society. Had Molyneux not died so soon, he might have supplied what was required.

[y] P. 100. [z] P. 108. [a] P. 100.

[b] Manning and Bray, History of Surrey, vol. I. p. 446.

The instrument was set up on the 26th of Nov.[i] 1725, and some days were employed in adjusting and preparing every thing for the observations. If there were any observable parallax in fixed stars, they would appear to have the smallest latitude when in conjunction, and the greatest when in opposition. The object, therefore, in the present instance, was to determine whether an analogous variation in declination could be detected in γ Draconis. It passes the southern meridian at noon in December, and its position was particularly noted by Molyneux on the 3d of that month. The motion which might be produced in it by parallax towards the pole, would at this time have been very gradual; and although observations[k] were made on 5th, 11th, and 12th, they seem rather to have been looked upon as means of verifying the place of the star, than from any expectation of promoting the great object of the inquiry. But Bradley[l] repeated the observation on the 17th, and perceived that the star passed a little more southerly than when it was observed before. He had[m] examined every thing with great care before and after the observation, and had satisfied himself by repeated trials that the instrument could be always adjusted within half a second; but still he was at first inclined to attribute the difference to the uncertainty[n] of the observation. The star's declination, however, was then varying at the rate[o] of about a second in three days, which must have made a difference between the 12th and the 17th, that exceeded these limits. Bradley to satisfy himself, resolved to pursue the inquiry; and on doing so, he found that the star passed still more southerly than in the former observation. He says in his paper, that this took place on the 20th of December; but it appears from the Kew observations to have been on the 21st[p]. The variation may be very easily accounted for. The star now having passed the conjunction, came to the meridian before noon, and consequently it was observed, according to astronomical reckoning, on the 20th, but according to the civil division of the day, on the 21st. This day (o. s.) in 1725, as it is stated, fell on a Tuesday; but there is a much more curious proof of the true date. The identical loose piece of paper has been accidentally found, on which Bradley made the original memorandum of the observation. A facsimile of this extraordinary document will be found opposite to p. 134; and if it is compared with Molyneux's account, it may be seen that they both agree in the time, as well as in all other essential particulars.

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[c] P. 98. [d] P. 100. [e] P. 94.

[f] P. 108. [g] See p. 153.

[h] It may be well to preserve the recollection of another astronomical station at Kew, although of inferior importance. Mr. Kirby's house, in which Dr. Bevis mentions (Phil. Trans. vol. LIX. p. 189.) that he observed the transit of Venus in 1769, was near the bank of the Thames, directly opposite to Brentford-ferry.

[i] P. 99.

In the Histoire de l'Astronomie au dix-huitieme siecle, the remarks on this observation are very inaccurate. Delambre wrote with great rapidity, and in the vast extent of his reading, it is not wonderful that matters of detail should sometimes have escaped his attention: allowance likewise must be made for his not having lived to publish this part of his work; but still the accounts which it contains, and the opinions which are founded on them, have gone out to the world with the sanction of his high authority, and therefore must not be left unnoticed.

He says[q], "Samuel Molyneux, auteur d'une Dioptrique dont nous avons déja parlé... "qui avait si bien étudié l'un des livres de Picard, devait probablement avoir une égale "connaissance du voyage d'Uranibourg où Picard déclarait assez clairement que les mouvemens observés par lui ne tenaient ni à la parallaxe ni aux réfractions." Instead of waiting for the accelerated motion which might be produced by parallax, "il aurait donc "fallu continuer assidûment les observations commencées, ne fût-ce que pour mieux constater l'état stationnaire de l'étoile, ou pour n'avoir rien à se reprocher, si les mouvemens venaient d'une cause encore inconnue, et suivaient une autre marche que la

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[k] P. 2. [l] P. 2. [m] P. 109, 110. [n] P. 2, 3. [o] P. 3. [p] P. 134. [q] Hist. p. 414.

"parallaxe. C'est ce que pensa probablement Bradley, qui se trouvait heureusement à "Kew." He then gives the account of what took place on the 17th and 20th of December, and of the motion which was afterward ascertained. He concludes by saying, "Comme Picard, Molyneux trouve à l'étoile un mouvement de 39″ dont la période est "annuelle." Now it certainly is too much to assume that the knowledge of one treatise must prove an equal acquaintance with any other work of the same author; but even if this were admitted, it does not apply to the individual in question. The treatise of Dioptrics, which is the foundation of all the reasoning, must be the Dioptrica Nova which was published by William Molyneux in 1692, when Bradley's friend, Samuel Molyneux, was only three years old. Neither is it quite fair to argue from what we now know of the event, that he ought not to have made any intermission in his observations; on that, however, there may be a difference of opinion, but with respect to the fact there can be none. There is no reason whatever for supposing that the motives assigned by the historian were even present to Bradley's mind: on the contrary, he tells[r] us distinctly himself, that it was chiefly curiosity, which tempted him to make the observation on the 17th. Delambre seems to have been actuated by too eager a zeal for the reputation of his illustrious countryman. Picard was the first to bring forward an undeniable[s] objection to Hooke's supposed discovery of the parallax of the fixed stars. He also detected some extraordinary annual motion in the pole star, which during a portion of the year could not be accounted for either by parallax or refraction; but instead of his observations having given the direction to Bradley's, or having formed the foundation[t] of his discoveries, it was by those very discoveries (which were necessary to explain them) that they were recalled into notice, and that their value was established.

This is not said to diminish the merit which belongs to Picard, but merely to correct an historical statement, which points most inaccurately to a connection which never existed. There is no reference to the authority for Picard's having assigned 39″ for the limits of annual variation in declination, and it is not contained in the Voyage d'Uranibourg[u]: indeed so precise a determination of the quantity could not have been made in that work, wherein he does not claim for his observations to be certain within less than[x] 10″ of the truth. Molyneux, indeed, found 39″[y], but it was for γ Draconis, whereas Picard's remarks refer to Polaris: and although this circumstance would only have increased the variation by 1″, still there seems to be a confusion in taking both the stars together under the single designation of "l'étoile." This is of the more importance to the present question, because Picard seems to have considered the motion which he describes as belonging especially to the pole star. We should therefore hesitate in blaming Molyneux, even if his plan did not extend to the verification of the observations which had been made at Uranibourg. These, likewise, although they indicated the existence of some other independent cause, by no means proved, even to Picard himself, that this cause might not be combined with refraction and parallax; and the obscurity arising from this complexity, might in those times have tended to throw them into hopeless, though undeserved neglect.

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[r] P. 2.

[s] Le Monnier, Hist. Celest. p. 252.

[t] Lalande Astr. §. 2798.

[u] He says, indeed, "que l'étoile polaire "s'approche annuellement du pole d'environ "20″," but this refers to the regular annual variation from precession, &c.: if this were not sufficiently clear from the context, all doubt on the subject must be removed, by his saying afterwards, that "vers la fin de l'année, tout se "trouve compensé, en sorte que la polaire "paroist plus proche d'environ 20″ qu'elle "n'étoit un an auparavant."

[x] Voyage, fol. p. 19.

[y] P. 3.

There is no reason, however, for wandering into this wide field of conjectures. Bradley, above an hundred years ago, told us[z] plainly that the observations at Kew were begun in hopes of verifying and confirming those of Dr. Hooke; and he returns to this subject at the end of his paper, where he states, as the result of the original object of his inquiry, that if the parallax of the fixed stars had amounted to 1″, he thinks he should have perceived it. We have now likewise the concurrent[a] testimony of Molyneux, and there cannot be the slightest reason for doubting the accuracy of their statement; nevertheless, it is not without its interest to trace the accidental accordance of independent circumstances with the truth. Bradley[b] had been acquainted with the whole contrivance of Graham's instrument; it is not improbable, therefore, that Pound (who only died a year before it was set up) had likewise had his share in arranging the object and plan of inquiry: there are entries in his account book[c] which agree well with the supposition of his being in the habit of visiting Molyneux. Now their papers contain no reference whatever to Picard; but we find among them copies carefully made out and particularly noted of Flamsteed's observations of Polaris, which, as Bradley remarks in his postscript, really give the apparent motion of the star, both in quantity and direction very nearly the same as would be deduced from his own. Yet so completely were the minds of both occupied by the single object of making out a parallax, that they appear, like Flamsteed himself, to have considered its existence to be indicated by these very facts.

The method of making the observations with Graham's sector was very simple; a plumbline was hung over the centre of the axis[d] upon which the instrument turned, and was made to pass over a fine point on the lower part of the telescope[e]. This coincidence was adjusted when the axis of vision was directed to γ Draconis on the 3d of December[f], and the subsequent deviations were then easily measured by the micrometer screw. The time at which this adjustment was made happened to be most favourable. If the observations had been begun three months sooner, the star by the effect of aberration would have been moving southward; but that is the direction in which it would then have been carried by parallax. The rate of variation would indeed have been different from the two causes, and a continuance of the observations beyond the conjunction would have marked still more decidedly the difference of their effects; but if any accident had operated to prevent this continuance, a false conclusion might have been drawn, and at all events an impression for three months would have existed in favour of parallax, which, as there was no small opinion[g] of the correctness of Hooke's observations, would have thrown difficulties in the way of arriving at the truth. There is little doubt that Bradley would have disentangled the clew, but it was fortunate that this trouble was spared him, and that the first step to his great discovery presented itself clearly and decidedly to his view. The observations were carefully pursued, and the southerly motion which was detected in December was found to continue advancing in the same direction till the beginning of the following March[h], when it had carried the star 20″ from the place in which it had first been seen. By the middle of April the star appeared to be returning back again towards the north, and about the beginning of June it passed at the same distance from the zenith as it had done in December. It continued to move in this manner, northward, till September, being then no less than 39″ more northerly than it was in March. From September, the star returned towards the south, till it arrived, in December, at the same situation it was in twelve months before, allowing for the difference of declination on account of the precession of the equinox.

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[z] P. 1.

[a] P. 93.

[b] P. 109.

[c] 1722, May 21. By expenses at Kew, and going.......................... 0*l.* 15*s.*

Aug. 4. By expenses at Kew, and in town, going and coming .... 1*l.* 18*s.*

1723, Oct. 11. By a journey to Richmond, and pocket expenses, and things bought 2*l.* 18*s.*

[d] P. 95.

[e] P. 97.

[f] P. 105.

[g] P. 16.

A phenomenon so contrary to what had been looked for, naturally suggested in the first place an examination of the instrument by which it had been seen; but repeated trials having shewn its great exactness, and the existence of the star's apparent motion being thus placed beyond a doubt, the next inquiry was for the cause of it. "[i]A nutation of the earth's axis was one of the first things that offered itself upon "this occasion, but it was soon found to be insufficient; for though it might have "accounted for the change of declination in γ Drac., yet it would not at the same time "agree with the phenomena in other stars, particularly in a small one almost opposite in "right ascension to γ Drac., at about the same distance from the north pole of the equa-"tor: for though this star seemed to move the same way as a nutation of the earth's axis "would have made it, yet, it changing its declination but about half as much as γ Drac., "in the same time, (as appeared upon comparing the observations of both made upon "the same days, at different seasons of the year,) this plainly proved that the apparent "motion of the stars was not occasioned by a real nutation, since, if that had been the "case, the alteration in both stars would have been equal." This is conclusive, but there is some little difficulty in understanding how that can be said to have been soon done, which was the result of observations at different seasons of the year. The words, however, in the parenthesis seem to have been added in confirmation of the previous assertion, without its being noticed that a discrepancy was introduced by the addition: and this supposition is perfectly consistent with the facts. As early as the 3d of Jan. 1726, within a fortnight after Bradley's first decisive observation, we find them looking for a star opposite to γ Draconis in right[k] ascension. Molyneux's instrument took in a very small range, but he found what was sufficient for his purpose: he calls it "Tele-"scopica in Auriga," and Bradley informs[l] us that it was the same with Flamsteed's[m] 35th of the Hevelian constellation of the Camelopard. Its zenith distance was not observed on this night, when the great object seems to have been to fix upon an Anti-Draco, (as it is afterwards[n] called,) but the position of it was taken on the 7th of January, and on the 3d, 12th, and 20th of February. During this time, its motion was uniformly towards the north, and was found to amount upon the whole to 5″: between the 3d of January and the 13th of February γ Draconis had been found to move

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[h] P. 3. [i] P. 3. [k] P. 140. [l] P. 11, 212.

[m] Flamsteed only gives its declination; but the exact place is assigned to it in Bessel's Fund. Astr. p. 176, where it is described as being of the seventh magnitude.

[n] P. 141.

9″,1 to the south; it was therefore soon seen that a nutation of the earth's axis, though it might be consistent with the direction, was insufficient to account for the relative quantities of motion in the two stars. But it may be remarked, that the instrument worked to a degree of accuracy for which they were not prepared[o], and they could hardly at this early period have acquired complete confidence in their observations of such minute quantities; the star, likewise, began to approach its conjunction in the spring, and as it was too small for them to be able to observe it by daylight, they were obliged to suspend their inquiries on this point to a "different season of the year." It will be seen that they returned to it in the autumn, but in the meantime they did not abandon the speculations on nutation. There is a paper in Molyneux's handwriting, in which he says, "If the angle of inclination of the equator to the ecliptic be less on December 10 than "on March 10, then a telescope directed to the zenith on the 10th of December, and "continuing so directed to the 10th of March, will point more northerly in March than "in December preceding; and, consequently, the fixed stars viewed through it will "have a seeming or apparent motion southward, and will appear most southward in "March. This exactly agrees with what we have observed in the vertical instrument; "for Caput Draconis appeared to go southward from December 1725 to March 1725-6; "and in the whole went south 21″." The paper was probably drawn up in the month of March, since the alteration in the direction of the motion which was observed in April would otherwise have naturally been taken notice of. But there is another paper about which there can be no doubt in this respect, as Molyneux has endorsed it— "Mr. Bradley, July 2, 1726, Nutation of the earth's axis." In this it is supposed that the effect would "make Caput Draconis seem to move south from September to March, "and north from March to September," which shews that the laws of a solar nutation had not been rightly ascertained at the time: but their dwelling on the subject proves that they did not consider their instantia crucis to have been completed, by the observations which they had hitherto been able to make on the Telescopica in Auriga.

In the same interval also, they considered "what refraction might do[p]." It is not easy to imagine how a regular annual effect could be produced by such a cause; but there is a paper which explains the manner in which it was supposed that it might have operated; and figs. 4 and 5, plate A, are copies of the diagrams drawn to explain the hypothesis. It depends upon the alteration which the figure of the atmosphere might be supposed to undergo from the earth's possibly moving in a resisting medium. In such a case, the upper surface "will probably not preserve its exact spherical figure, but will be altered into an "oblong spheroidical figure, whose longest diameter will be in the direction wherein the "earth is then moving in its orbit, and the earth will be nearest to the advancing end "of this spheroid. The immediate consequence of this will be, that all objects seen in "the zenith of any part of the earth will suffer a small refraction, the rays coming from "such object being bent in the upper regions of the atmosphere always towards the fol- "lowing end of the spheroid. It will be further considered, that the long diameter of "this spheroid lies in the plane of the ecliptic, and being always a tangent to that part "of the earth's orbit where it then is, it will in the space of a year make one whole "revolution; so that when the earth is at D, a ray of light falling from the zenith (if

[o] P. 104.

[p] P. 6.

" perpendicular to the plane of the ecliptic) will be refracted towards T; when the earth " is at F, this ray will be refracted towards A; when the earth is at M, towards B." Hence it is concluded, that a star like γ Draconis, which is near the pole of the ecliptic, will be made to appear to describe nearly a circle round its true place; " but if the " observer hath an opportunity only to measure the variations of the place of this star " as to north and south, and this is all we can do by reason of the diurnal rotation of " the earth, then when the earth moves from D to F, the star will appear to him to " have gone southward by such a quantity, as is to the total refraction, as the sine of the " angle D S F is unto the radius." For any other star which is not near the pole of the ecliptic, " the rays falling from it respectively on parts of the curve of the upper region " of the atmosphere, whose tangents are differently inclined to the said rays, this will " disturb the above proportion;" but still upon the whole, he considers that the observations of Telescopica in Auriga and the star in Cap. Persei will agree in general with the hypothesis.

This paper is very curious on account of the hypothesis which it details; but still more for the advance which it shews that they had made in analyzing the motion which had been detected. It is dated June 18, 1726, and we clearly discover from it, that they already saw the effects to be such as might be produced by some cause which would make the stars appear to vary their places in the direction in which the earth was moving at the time of the observation. From hence it followed, as is particularly stated, that the apparent motion would not be merely north and south in the line of the meridian, but that it might make the heavenly bodies describe a curve about their true places. The curve was supposed to be circular for γ Draconis, which was situated at the centre of it: and this would probably have suggested that the variation in declination might be proportional to the sines of the distances from the place in which it was neither above nor below its true position. This law they had endeavoured to verify; for it is expressly mentioned, that " the quantities of the alteration of its (γ's) place were always proportional " to the sines of the sun's distance from the solstice." Bradley, taking his 0 from the extremities, states the differences from the maxima to be nearly as the versed sines of the distance from the equinox[q]. Mr. Delambre points out that this was the accidental consequence of the position of γ Draconis, and " n'était qu'un hazard heureux." It was certainly no small encouragement to the early investigation of the problem, that something was found to rest upon, which might be afterwards modified as circumstances should require. But if it facilitated the calculations, it must likewise be observed, that it operated on the other hand to lead them astray from the true cause of the phenomenon. The earliest speculations on the means by which the apparent motion might be produced, were directed in consequence to the earth's place in its orbit with reference to the sun, rather than with reference to the particular star. There were indeed some subsequent attempts, as we have just seen, to combine the two; but the fundamental position was always taken upon the sun's distance from the equinox or the solstice. It even suggested that parallax might not only enter into the cause of phenomenon, but might even be determined from it. This appears from a notice in one of the books which Bradley used for rough entries and calculations; it was probably written on the first, or at least before the seventh of October 1726,

[q] P. 4.

and is as follows: “ The star Caput Draconis has moved from north to south between “ March 10th and June 10th 20″,9; between June 10th and September 11th 17″,7; “ and if the difference is owing to the parallax of the earth's orbit, that parallax will “ conspire to make the star seem to move from south to north from March to June, and “ the contrary from June to December; so that the difference of its motion between “ March and June, and June and March[r], will be equal to the whole parallax of the “ earth's orbit. We may therefore from the observations conclude, that the diameter of “ the earth's orbit at Caput Draconis subtends an angle of 3″.”

Molyneux ends his paper on refraction by saying, “ I have wrote down this hypothe- “ sis this day, not as a thing which I look upon as certain and demonstrative, but barely “ as a probable conjecture, to be examined by further observations.” There is, however, another paper, in which reasons are detailed for abandoning the hypothesis. How soon this took place is not stated: the next date which can be ascertained is Sept. 28, when their attention was again directed to Telescopica in Auriga. It was not observed afterwards, till the 26th of November; but these two observations were at a “ different season “ of the year” from those which had been previously made, and confirmed them in the opinion which they had probably “ soon” begun to entertain with respect to the insufficiency of their views of nutation. The following curious entry occurs in Bradley's book which has just been referred to.

	Draco.	Telescopica.	Perseus.
“ Jan. 7.	10,2 S.	21,3 N.	7,5 N.
“ Feb. 12.	18,7 S.	26,1	
“ Sept. 27.	16,4 N.	8,9	
“ Nov. 26.	1,5 N.	15,5	15,2 N.

“ According to these numbers, the alteration in Caput Draconis is to that of the Tele- “ scopica in Auriga always as the latitude of the one and the other: and the same pro- “ portion holds likewise in Perseus, allowing for his alteration in declination.”

The memorandum which follows this entry is dated Dec. 2. We have therefore now arrived at the close of the first year's inquiry, in which much had been done. The existence of the motion had been established, and for γ Draconis the approximate law of its variation, as well as its maximum in declination, had been ascertained. No progress had indeed been made in discovering the true cause of the phenomenon, but the observations to which they had had recourse, as tests of the hypotheses which occurred to them, had given a clew to further investigation. It had been found that the quantity of the motion in declination was somehow connected with the latitude of the star. The precise nature of this connection does not yet appear to have been distinctly made out, but it was now clearly seen, that the few stars which passed within the narrow[s] range of Molyneux's instrument were insufficient for the complete examination of the question.

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[r] September?

[s] About 8′, P. 4, 98.

CHAP. III.

Bradley's zenith sector—The place where it was erected at Wansted—Construction of the instrument—Observations made with it—Laws of the apparent motion more particularly made out in 1727.

BRADLEY now determined on setting up another instrument for himself, and must have begun his arrangements for that purpose early in 1727. It may be seen[t] that he had got his object-glass, and had examined the centring of it in the latter end of March, and there is an entry made on the 11th of that month with respect to the azimuth of the chimneys against which the telescope was to be erected. His means at this time were very limited. There happens to be a memorandum, from which it appears that his professorship in 1724 brought him only 138*l.* 5*s.* 9*d.* per annum; and it is not known that he had any other source of income; but the "res angusta domi" were not suffered to be any impediment in the pursuit of his object. In the first rough draught of the paper on aberration, he complains that he had in several "journeys to Kew been successively hindered by clouds from making any observations" when he arrived there; he was anxious, therefore, to multiply his observations, as well as to increase the objects of them, by having the means always[u] at hand with which he might prosecute with more ease and certainty his inquiry into the laws of this new motion.

He tells us[x], that he had no convenient place in which he could make use of so long a telescope as Molyneux's. This, connected with the locality of Wansted, has suggested to M. Delambre[y] that it was to be used in his uncle's house. Lalande also alludes to Pound[z] upon this occasion; but Pound had now been dead more than two years, and the rectorial house had passed into the occupation of a stranger. Another more generally received opinion has been, that he was accommodated in the house of Mr. Wymondesold, whom he mentions, in the paper on nutation[a], as the friend by whose kindness his instrument remained for twenty years in the place where it was first erected. He speaks likewise of being deterred, after he went to reside at Oxford, from his observations, when they were to be made at such hours[b] of the night as would have incommoded the family of the house in which the instrument was fixed. This conjecture seems to be the most plausible which could be thought of in the absence of more direct evidence, and yet it is attended with several difficulties. Mr. Wymondesold's was an old mansion, built in 1690, by sir F. Dashwood[c]; and whatever objection there might be from other reasons, there could have been no want of a place in it which would have admitted as long a telescope as that at Kew. We shall hereafter see reason to understand more clearly the disturbance to which a family would have been subjected by his observations at night, but it will be necessary first to ascertain the precise place in which they were made.

After Pound's death, Bradley tells us that he observed at his "aunt Pound's house "in Wansted town." He began to do so in July[d] 1725, and he continued to reside with her for several years. The house must have been small, the ground room of it was only 7½ feet high[e], with what appears to have been a loft or garret over it. Among the observations there is an incidental notice in 1727 of his having meridian "marks "on the wall at the bottom of Mr. Wymondesold's garden." This must have been to the north: for May 1, 1729, the transit was adjusted "by two spots on the wall on "Mr. Will's house," which must have been near, and on the south, for "when the sun "passed the meridian instrument, Oct. 25, 1725, his centre appeared just above the wall "of Mrs. Will's house, part of the under limb being hid by it." From a memorandum made in May[f] 1731, it may be seen that another house had then been recently built up on the north side of Mrs. Pound's, and it is not improbable that a row may have been continued in that direction. The walls of the house were not exactly in the direction of the cardinal points[g] of the compass, for he says that "1725, Sept. 15th, 22h. 34′ app. time, "the sun was in the same azimuth as Wansted tower from my aunt's house; the sun's "azimuth was then 26° 8′ 10″." This must have referred to the church tower, for the building in the woods, on the hill to the east of Wansted, was not erected before 1730. Finally, there is fortunately a book of Bradley's preserved at Greenwich, in which he has entered this precise notice. "N. B. Wansted tower is 42 chains distant from the "house where my sector was placed, and lay 26° 8′ (in azimuth) east of the meridian, "as viewed from the house."

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[t] P. 194. [u] P. 4. [x] P. 4.

[y] Hist. de l'Ast. au xviii siècle, p. 415.

[z] Astronomie, §. 2822.

[a] P. 18. [b] P. 30.

[c] Morant's Hist. of Essex, vol. I. Becontree Hundred, p. 31.

[d] P. 356.

An actual measurement may now at any time determine the particular spot; but although unnecessary for such a determination, it may be permitted to preserve the memory of some other circumstances connected with so very interesting a subject. There can be no doubt that the place was near the west front of the house, which is now the property and in the occupation of the hon. Mrs. A. Rushout; and much of the little tradition, which can yet be collected, agrees in general with this situation. T. Barker, who had been many years parish clerk of Wansted, and who died in 1831, at the age of 93, reported that he had heard, when a boy, of astronomical observations having been made by Dr. Bradley in the house which is occupied by Joshua Knowles, Esq., and that the instrument was fixed in the cellar, from which the observations could be made at noon day through an opening in the roof. This house, which is on the south side of the entrance to Mrs. Rushout's, was a part of Mr. Wymondesold's estate, and certainly is as old as the beginning of the last century; but the rooms are too lofty, and the whole of it is much superior to that occupied by Mrs. Pound; it is not such a building as could ever have had its north end supported merely "by a timber partition[h]," and the houses to the south of it lie all open to the east, whereas Bradley's meridian lay on the west of Mr. Will's house; for 1726, Oct. 13, he says, "β of Capricorn is the lowest point "that can be observed by the middle thread, that star appearing from behind the wall "just as 'tis upon the thread."

By an examination of the old parish books, it was found that a person of the name of Wills, or Wells, certainly lived in this part of the town; and from the same source of information it was collected that Mrs. Pound's house was occupied about 1735 or 36, by a Mrs. Eliz. Williams, who was supposed by some old inhabitants to have lived in the clock house, at a short distance to the north-west of Mr. Knowles's. In this supposition, however, there must have been some mistake; it never, indeed, was Mr. Wymondesold's property; but that is not conclusive: what negatives the idea is, that the rooms are too high, that the whole building contains too many floors, that the chimneys will not answer Bradley's description, and that there are no traces of any habitation on the south of it.

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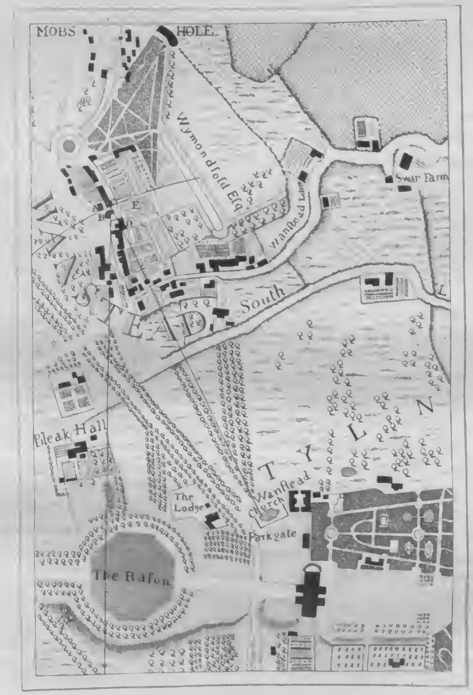
[e] P. 195. [f] P. 265. [g] P. 197. [h] P. 265.

There is no house at present existing which will answer in all points to what we are able to collect of Mrs. Pound's. An old contemporaneous map, however, enables us so clearly to see the situation of it, that a copy of what is requisite for this purpose is here annexed.



A. Clock House. B. Mr. Will's. C. Mrs. Pound's. D. J. Knowles, Esq. E. site of Mrs. Rushout's.

The original was published by Roque, from an actual survey for several miles round London, begun in 1741, and completed in 1745[i]. A circle has been drawn, whose centre is in the western end of the church, and its radius is equal to 42½ chains on the scale annexed to the map. When we consider the strain to which paper is subject in passing through the rolling press, and the inequality of contraction which will occur in its drying, this variation of half a chain will not be important. A meridian line has been drawn, where it makes an angle of 26° 8′ with the radius of this circle, and it will be seen that the lines meet at the southern extremity of a row of houses parallel to Mr. Wymondesold's garden wall: it will be seen that the eastern side of this row is in a direction, which, if continued, will pass near the church, which answers to the bearing of the side of Mrs. Pound's house, and that there is a building rather of a singular[k] shape immediately on the south. This last may now be clearly traced in the western extremity of Mrs. Rushout's offices, being a dwelling house of some age, (certainly above 100 years old,) adjoining to which her stables have been built. Mr. Wymondesold's mansion was in the northern part of the property; he sold the estate in 1756 to Humphrey Bowles, Esq., through whose grandaughter (the late lady Northwick) it descended to the hon. Mrs. Rushout, who pulled down the old house in 1823, and in the following year built her present residence in the centre of the western side of the grounds. No record is to be found of the existence of the row of houses, but they evidently were of very little value, and may have been pulled down even before Mr. Wymondesold parted with the property. At all events they may have belonged to him, for the present garden wall appears to stand very nearly on the site of them. So many circumstances, therefore, combine in this one position, that there can be no hesitation in fixing Bradley's station on the eastern side of the clock-house, and on the north of the dwelling in the western part of Mrs. Rushout's stables.

The description of Mrs. Pound's house will at once shew the limits within which it was necessary to keep the instrument. We find calculations of the length of the sine of a second on an arch whose radius is 13 feet, and mention of 13½ feet; but he finally took a somewhat shorter measure. The extent of the arc which the instrument should embrace, seems to have been the subject of more deliberation, for there is a catalogue of stars which passed within a degree of the zenith, another of those which passed within 3°, and, lastly, to take in Capella, the range was extended to 6°¼[l]. The instrument was contrived and completed under the direction[m] of Graham, who must have divided the arc, and probably himself executed or finished all those parts which required a nicer construction than the rest; but in a loose paper which is at Greenwich, and which will be referred to more fully hereafter, Bradley mentions that the apparatus for suspending the instrument was made by Hearne[n], who probably was likewise entrusted with other parts of the rougher work. With the addition of the arc, and of a combination of two plates to mark the number of revolutions, as well as the parts of a revolution which the micrometer screw may have made, this instrument appears to have been the precise counterpart of that which had been erected at Kew. In the construction, however, of that sector, the plumbline had been made to hang from a small notch[o] in the centre of the horizontal axis. This arrangement was retained in the Wansted instrument, and although it did not introduce any material error into Bradley's observations, Dr. Maskelyne[p] thought it liable to objection, and employed Bird in 1768 to apply another method of suspension by which the line should hang freely without being affected by the parts of the instrument, which might be put in motion. The very small alterations in direction, of which Molyneux's sector was capable, rendered this suspension a matter of no importance; but on an instrument of wider range, it is possible that the inaccuracy to which it might be liable was soon noticed, and when Graham made the sector for the measurement of the arc in Lapland he adopted a different[q] method of suspension, although the limb was not quite of half[r] the extent of Bradley's. It is not, indeed, improbable that the alteration was suggested by Bradley himself; for we find from Maupertuis' letter[s] that he gave his assistance in the construction of the instrument.

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[i] It will be remembered that Bradley's paper on nutation, in which he mentions his obligation to Mr. Wymondesold for allowing his instrument to remain so long in its original position, was printed in 1748.

[k] As a part of the house takes an oblique direction, the western extremity of the roof would be further distant from Bradley's observatory, and consequently would be at less apparent altitude than the eastern part. This will explain, what otherwise would be difficult to be understood, where he speaks of "the star appearing from behind the wall." P. xxiv

[l] P. 5.

[m] P. 4.

In the account of the measure of the degree between Paris and Amiens, (8vo. Paris 1740,) there is a description of the sector used by Maupertuis; and Bradley[t] refers to it as superseding the necessity of his giving any particular account of his own. The plates[u], indeed, illustrate not only the principle, but likewise the form of some of the most essential parts of it, but still they do not represent the precise original instrument to which so strong an interest is necessarily attached. Neither has any such representation[x] been supplied by subsequent publications. Mr. Taylor, therefore, who has so long been the valuable assistant to the Astronomer Royal at Greenwich, was good enough to make and communicate an accurate drawing of it, which has been carefully copied in plate A. fig. 3.

The length of Bradley's tube was about $12\frac{1}{2}$ feet[y], but he found that he could depend on the adjustment of it to $\frac{1}{4}$ of a second[z], and its weight being little more than half[a] of that which had been made for Molyneux, it was less liable to those disarrangements which occurred to the instrument used at Kew[b]. A shorter tube likewise could be made much more firm, and it was of importance to avoid all flexure, especially in the plane of the meridian, the telescope being supported when out of the perpendicular by a screw which pressed against the lower part of it. From the account of an accident which took place in 1736[c], it may be seen that the instrument was enclosed in a wooden case, probably like the trunk which was made to protect the telescope at Kew[d]. No inconveniences however like those which impeded the action of Molyneux's instrument[e] are mentioned as resulting from it. The length of the arc at Wansted would require the case to be of greater extent, and it may purposely have been made wider to prevent the cobwebs from accumulating in it.

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[n] "An excellent workman in Dogwell court, "White Friars, by Fleet-street." Smith's Optics, p. 304.

In the same manner the zenith sector, which the French astronomers took with them to Lapland in 1736, is described by Maupertuis to have been made in London, "sous les yeux de "M. Graham. Cet habile méchanicien" he goes on to say, "s'étoit appliqué a lui procurer tous "les avantages et toutes les commodités dont "nous pouvions avoir besoin: enfin il en avoit "divisé lui-meme le limbe." Mem. de l'Ac. R. 1737, p. 408.

[o] P. 95. [p] P. 198.

[q] Phil. Trans. vol. LII. p. 607.

[r] Mem. de l'Ac. Roy. des Sciences 1737, p. 441.

[s] P. 404. [t] P. 20.

[u] These have been copied for the Encyclopédie Méthodique (Astronomie pl. XXI.)

[x] Fig. 50. pl. V. of Vince's Practical Astronomy, and fig. 2. pl. XXVII. of Dr. Pearson's work on the same subject, are rather diagrams to illustrate what is essential in principle, than portraitures of the individual instrument.

[y] P. 5. [z] P. 202. [a] P. 98, 201.

Bradley's arc gave him the choice of more than 200 stars inserted in the British Catalogue, from which he selected 70[f]: of these there were nearly 20 of which he did not make many repeated observations, but on the other hand there were not less than 12[g] which were bright enough to be seen through all seasons, and even in full daylight.

In addition to his first great object, Bradley tells[h] us that he wished, by independent observations, to confirm the truth of those which had been made at Kew. Molyneux entered into his views, and assisted[i] in erecting the Wansted sector on the 19th of August, 1727. The very next time that he had an opportunity of observing at Kew, he entered in his journal[j], "Now Mr. Bradley's instrument is set up, and we go on "comparing notes from this time." But this was a pleasure which he was not destined to enjoy: he was only able to make six more observations in all the remaining months of the year; and on the 29th of December[k] was "the last observation made and "entered at Kew." The instrument indeed, in Feb.[l] 1727, was out of order, and there is no notice of its having been subsequently repaired. Graham was employed on the new sector, and possibly could not attend to it. Before[m] this was finished, Molyneux had been appointed one of the lords of the admiralty, and although it is clear from the entry just quoted that he did not on that account give up the hopes of being able to pursue the inquiry in which he was engaged; still before the year was out he must have found the continuance of it incompatible with his public business. What became of his instrument is not known; he died in the following April[n], and the house passed into other hands: besides, in the infancy of a discovery, the authors of it are themselves unable to estimate how far it may advance, and the memory of many things is lost, to which an interest, which could not be anticipated, is afterwards attached.

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[b] P. 186. [c] P. 273. [d] P. 94, 103.

[e] P. 102, 190, &c.

[f] These were
Andromedæ ξ, 3, 7, 8, 65.
Aurigæ α, δ, 9, 46.
Camelopard. 18, 27, 35.
Canum Ven. 3, 21.
Cassiop. α, β, θ, λ, π, σ, τ, 2υ, 34.
Cephei ζ, μ.
Cygni θ, ι, κ, 2ο, 1π, ψ, 3ω, 1f, g.
Draconis β, γ, ξ, c, d, 49, 51.
Herculis ι.
Lacertæ 3, 5, 7, 9.
Lyncis 13, 27, 35.
Persei α, γ, δ, θ, τ, φ, h, 4.
Ursæ Maj. β, γ, ε, ζ, η, θ, ι, φ, χ, ψ, f, g, 36.

[g] P. 5. [h] P. 4. [i] P. 201.

[j] P. 191. [k] P. 193. [l] P. 186.

[m] Aug. 2, 1727. It is on this account that Bradley gives him the title of Honourable, P. 1.

[n] Manning and Bray, Hist. of Surrey, vol. I. p. 446.

Molyneux probably was also failing in health, but still it seems remarkable that we find no notice of his visiting Wansted after the 19th of August. If he had gone there he most probably would have observed, and in that case we might expect to see his name recorded as well as those of lord[o] C. Cavendish, Dr. Hoadly[p], and Halley[q]. There is something characteristic in the entry of Halley's observation of Capella, Sept. 2, 1727, "he said that as it went out it appeared to be more southerly in reality than the thread, he therefore fancied from this observation that the direction of the thread was not right; but as all other stars at going out appear on the other side of the wire[r], this appearance must be owing to his not being able to bisect the star at the cross, because of its fluttering." In fact, Halley had no opinion of these fine observations: he expressly says[s], "ut verum fatear, minuta secunda vel etiam dena secunda instrumentis quantumvis affabre factis certo distinguere vix homini datum est," and he must therefore have thought it idle to attempt observing a portion of a second. Accordingly, he never believed a word of the doctrine of nutation, although he lived till 1742, when Bradley had already continued his observations for a series of many years, in which he had found the constant operation of this effect. Though Halley's was certainly a very powerful mind, it was not always free from error; for he not only had too much contempt for what appeared to him to be trifles, but he was often too rapid[t] in drawing his conclusions. The effects, however, of aberration, amounted to the greater part of a minute in each year, and formed a tangible quantity, the existence of which did not admit of a doubt: Bradley, therefore, when he announced the discovery in his letter to Halley must have felt secure, in this case, of "the approbation[u] of so great a judge."

Molyneux had placed[x] his observations at Bradley's disposal; but notwithstanding their intrinsic value, they were insufficient by themselves; and Bradley may have thought it better to confine himself to those which were made by the same eye, with the same instrument, and in the same place. He therefore deduces all his final conclusions from the Wansted observations. We find one of γ Draconis[z] on the 19th of August 1727, and several of other stars on subsequent days; but all the adjustments[a] of the instrument were hardly completed before the 20th of September[b]. He lost no time indeed in observing the stars which he considered most likely to answer his purpose, and he soon found that the idea of their being farthest north and south when the sun was in the equinoxes, would only apply to those which were near the solstitial colure. On the contrary, he perceived that the maxima of deviation in declination were connected with the times when the stars were on the meridian at six o'clock[c], and that they all moved southward while they passed by day, and northward while they passed by night. He says that he remarked this after he had continued his observations for a few months. This gives another epoch in the history of the investigation; for we must in consequence have now arrived at the end of 1727, or beginning of 1728.

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[o] P. 237.

[p] P. 254, 257. This was most probably Benjamin, the eldest son of Bishop Hoadly, Bradley's early patron. He was a physician.

[q] P. 208, 248.

[r] When the sector was first set up at Kew, it was observed that the star, which was bisected at the centre, was to the north of the horizontal wire, both on its entering and quitting the field of the telescope, P. 110, 111. Some obscurity which may be observed in P. 132, is entirely produced by additions and interlineations which have been introduced into the original entry of the observations of Dec. 18, in consequence of Bradley's having in this instance explained an appearance which Molyneux could not at first understand.

[s] Phil. Trans. vol. XXIX. p. 456.

[t] This was more particularly the case in subjects unconnected with his own line of studies. Hearne (N°. 94, p. 45) says, "Last night I was in company of Dr. Halley and Mr. Bradley, our two Savilian professors. Dr. Halley hath a strange odd notion that Stonehenge is as old, at least almost as old as Noah's flood.. . But when he is possessed of a notion, he very hardly quits it." And (N°. 118, p. 106,) he says, "Dr. Halley is excellent at writing short discourses in mathematicks; but when he dives into other subjects, he is whimsical, fanciful, and erroneous." If this estimate is objected to, because Hearne's natural talents were not cast in a giant mould like Halley's, the same will not be said of the author of the Analyst, and every one knows the unhappy errors which called down chastisement from him, who was possessed of "every virtue under heaven." Newton also found it necessary occasionally to check the great man, and sometimes even to say to him, "Mun! Mun! you talk without thinking."

[u] P. 15.

[x] P. 158.

CHAP. IV.

Discovery of the cause of aberration—Progressive motion of light—Demonstration of the rules for aberration—Examination of the observations—Maximum of aberration—Application of it to particular instances—Velocity of light—Paper for Royal Society.

BRADLEY began to observe the 35th Camelopard. on the 11th Sept. 1727, and he now found that the maximum of its apparent motion was to that of γ Draconis not in the ratio of their latitudes, as he had collected from the earlier observations, but as the sines of those quantities[d]. This led him to suspect that the same might hold good for other stars; but the hypothesis did not perfectly correspond with the observed motions of all, and he therefore determined upon abstaining from all further speculations till he had accumulated a sufficient number of observations to enable him completely to try the validity of them. When he had by this means pretty well satisfied himself as to the general laws of these apparent motions, he again endeavoured to find out their cause. From the details already given of the investigations which had been made at Kew, it is clear that nothing at first occurred which had not been previously found to be insufficient: but "[e]at last, when he despaired of being able to account for the phenomena "which he had observed, a satisfactory explanation of it occurred to him all at once, "when he was not in search of it. He accompanied a pleasure party in a sail upon the "river Thames. The boat in which they were was provided with a mast, which had a "vane at the top of it. It blew a moderate wind, and the party sailed up and down

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[z] P. 203.

[a] It may be remarked that Bradley examined his object-glass very carefully, and found that the optical centre of it did not coincide with its centre of magnitude: he therefore took care that the line joining these two points should be placed truly east and west. He likewise tried whether the apparent place of an object could be affected by the part of the glass through which it was seen (P. 194). The effect of varying the aperture of the instrument had been tried at Kew, (P. 106, 150,) and experiments had been made there to ascertain whether the deposition of moisture on the object-glass could affect the apparent place of the star (P. 151.)

[b] P. 216. [c] P. 5. [d] P. 6.

[e] Thomson's Hist. of the Royal Society, p. 346.

"the river for a considerable time. Dr. Bradley remarked, that every time the boat put "about, the vane at the top of the boat's mast shifted a little, as if there had been a "slight change in the direction of the wind. He observed this three or four times without speaking; at last he mentioned it to the sailors, and expressed his surprise that the "wind should shift so regularly every time they put about. The sailors told him that the "wind had not shifted, but that the apparent change was owing to the change in the "direction of the boat, and assured him that the same thing invariably happened in all "cases. This accidental observation led him to conclude, that the phenomenon which "had puzzled him so much was owing to the combined motion of light and of the "earth."

The ancients imagined that vision was performed by certain tentacula which were thrown out from the eye so as to reach the object, and through these they supposed the sensation to be instantaneously transmitted, in the same manner as Descartes[f] illustrated the effect by the impression which is immediately felt at one end of a stick, when the other presses against any substance which resists it. Galileo[g] wisely doubted of this theory, and with his usual sound judgment was desirous of reducing it to the test of an experiment. The Florentine academicians[h] tried it without finding any result which they could rest upon. Indeed, we can only wonder that the accuracy of their execution was sufficient to guard them against an exaggerated estimate of the really imperceptible interval which must have elapsed during the observation. It is impossible, indeed, to conceive any physical effect to take place without requiring some portion of time, however short, for its operation, and Bacon[i] therefore concluded, that "in visu liquet "requiri ad eum actuandum momenta certa temporis." But to measure the velocity of light required the radius of the earth's orbit for a standard, and nothing tangible was suggested with respect to it before 1676. Roëmer[k] then pointed out that it would account for the eclipses of Jupiter's first satellite being seen so much sooner when the planet was in opposition than when it was near its conjunction. This happy thought did not immediately meet with the universal concurrence which it deserved, but all subsequent observations have tended to confirm it, and none more powerfully than those of Bradley, for which it gave him the means of forming so beautiful an explanation.

Galileo made it a question, whether the sun may not really have reached the horizon before we see it there; and Bacon precisely points out that it might be doubted "utrum cœli sereni et stellati facies ad idem tempus cernatur quando vere existit, an "potius aliquanto post; et utrum non sit (quatenus ad visum cœlestium) non minus "tempus verum et tempus visum, quam locus verus et locus visus, qui notatur ab "astronomis in parallaxibus." Descartes likewise, in a letter which he wrote in 1634, distinctly mentions[l], as an immediate consequence from the progressive motion of light, "ut dum sol aspicitur, non appareat in loco in quo est revera, sed in quo fuit eo "instanti, quo lumen per quod videtur, ab eo prius egressum est. It must, however, be remarked, that the motion of light, thus considered, always makes the apparent behind the true place of the object which we are observing, and that this only occasionally takes place from the effects of aberration. There is another circumstance, likewise, which marks the two cases to be perfectly distinct: for the difference of place mentioned by Bacon and Descartes depends upon the actual velocity of the object, and therefore is obviously inapplicable to the aberration of the fixed stars discovered by Bradley.

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[f] Dioptrice, cap. I. §. 3.

[g] Mech. Dial. I. by Weston p. 63.

[h] Essayes made in the Ac. del. Cimento, p. 157.

[i] Nov. Organ. lib. II. §. 46.

[k] Lalande, Astr. §. 2930.

[l] Epist. pars II. p. 93. Delambre Hist. de l'Astronomie au xviii siecle, p. 418.

While Bradley's boat was at rest, the vane must have turned directly to the side opposite to that on which the wind blew; but when it was set in motion, it would meet the streams of air as they reached it, and their reaction on the vane would make it incline from its former direction towards the stern of the boat. The quantity of this alteration would depend upon the relative motions of the boat and of the wind at the moment when they met, and while these were the same, it could make no difference whether the current of air had previously passed over five hundred yards, or five thousand miles. The inclination given to the vane by the combination of these two forces would make the wind appear to come in some direction nearer than the truth, to the point towards which the boat was moving. So the earth in its revolution successively meeting the rays of light from any one of the heavenly bodies, modifies the direction in which they enter the eye, and the star (excepting under very peculiar circumstances) must appear to be at some distance from its true place. This deviation will always be on the side to which the earth's relative motion shall be at the particular moment directed, and the quantity of it will depend on the ratio which the velocity of that motion shall bear to the velocity of light—the same principle applies in this respect to the light from the moon, or from the most distant of the fixed stars[m].

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[m] Bradley's own explanation of the theory will be found at P. 7. Of all the familiar illustrations of the general effect, none possibly is more striking than that which Lalande gives in his Astronomie, §. 2831. "Je suppose que, dans un temps calme, la pluie tombe perpendiculairement, et qu'on soit dans une voiture ouverte sur le devant; si la voiture est en repos on ne reçoit pas la moindre goutte de pluie; si la voiture avance avec rapidité, la pluie entre sensiblement, comme si elle avoit pris une direction oblique."

According to the Newtonian theory of light, its velocity depends on the attractive power of the medium into which it penetrates. Mr. Melvill, therefore, in a letter[*] to Bradley, dated June 1753, suggests that aberration must in consequence be produced by the relative velocity of the earth in its orbit, when compared with that of light, not in the surrounding atmosphere, but in the denser humours of the eye. Mr. Melvill was the intimate friend[†] of Dr. Alexander Wilson, of Glasgow, whose son, Mr. Patrick Wilson, brought forward the same idea in the Phil. Trans. for 1782[‡]. Mr. Melvill died at Geneva in Dec. 1753[||], and although he was in correspondence with Dr. Wilson, it is very possible that he never communicated to him a theory which he seems to have rather submitted in general to Bradley's consideration, than to have followed out in all its bearings. Dr. Hutton informs[§] us, that the thought had struck Mr. Wilson as early as 1770, and that in 1772 another effect had occurred to him which it was necessary likewise to take into account. The same attractive power which increased the velocity of the light would also diminish the angle of refraction, and as these two effects, according to the received laws which regulate them, would counteract each other, the ray may be seen under the same angle of aberration if it passes through a tube filled with water, as it is under the common circumstances of its passage through a tube filled with air. Now the densities of the humours of the eye are not much greater than that of water, and on this ground we may

[*] P. 483. [†] Brewster's Edin. Journal of Science, 1829, vol. X. p. 5. [‡] P. 58. [||] Brewster, p. 7, 8. At the age of 27, Priestley's Hist. of Optics, p. 359. [§] Phil. Trans. Abr. vol. XV. p. 192.

There is no entry or memorandum from which the precise time can be fixed at which this happy thought at last occurred to Bradley, but it was most probably about September 1728. As it was after he had[n] completed the year which he had dedicated to the fundamental observations, it could not have been sooner than the end of August; and the story of the water party will not suit a later season than that of the autumn. While therefore we regret that Molyneux did not live to see the splendid result of the inquiry to which he had so essentially contributed, it is impossible to contemplate without admiration the great labour which Bradley must have undertaken between the time when the theory first occurred to him, and that when he presented the complete developement of it to the Royal Society.

It will be seen[o] that, with the exception of about a fortnight which Bradley spent at Oxford in the beginning of November, there was no relaxation of the observations at Wansted during the last four months of 1728. He must have traced out the consequences of his hypothesis before he could have devised those rules for its effects which he gives in his paper, and it is clear that they were the results of some regular demonstration. He observed only[p] the variations in declination, and he therefore does not enter at any length on the apparent motions in right ascension; but he must have been aware of their existence, as he gives the dimensions of[r] the ellipse in which the stars will appear to move. Simpson[s] states that Dr. Bevis was the first, as far as he knew, who experimentally proved that the phenomena in right ascension are universally conformable to Bradley's hypothesis. This, however, was probably as late [t]as 1739, and Eust. Manfredi was therefore much before him. His letter to Ant. Leprotti is dated in 1730, in which he says, that[u] although there were exceptions, he found his observations in general to indicate a motion in right ascension analogous to that which Bradley had detected in declination. Clockwork had not then arrived at sufficient perfection to enable astronomers to do more, and further time must have elapsed before the discovery could have been established, if the foundation had been laid in observations of right ascensions: whereas the twelve months employed with the zenith sector at Wansted established the precise quantity and direction of all the motions, whether they referred to the equator or the ecliptic.

No account has ever been given of the manner in which Bradley arrived at his rules for the variation in declination. Clairaut[x] seems to have had no acquaintance with it, and what is still more remarkable, Thomas Simpson[y] says, that before his time the subject had not been fully and demonstratively treated of by any author. There can however now be no doubt of Bradley's[z] having originally resolved the problem com-

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conclude that there fortunately is no necessity for an allowance which might differ for every individual observer. It is only astonishing that Mr. Wilson did not pursue his own principle to its full extent, and shew that the quantity of aberration will depend upon the velocity of light in free space; the effect, according to his view of the subject, would not be more altered by passing from thence into the air, than it would by passing subsequently from the air through water, or the human eye.

[n] P. 6. [o] P. 242—248. [p] P. 8.

[r] P. 9. [s] Essays 1740. p. 10. [t] P. 416.

[u] Comm. Acad. Bonon. (1748) vol. I. p. 634. At p. 618, Manfredi mentions that sir Thomas Dereham translated Bradley's paper on aberration into Italian, soon after it was published.

[x] Mem. de l'Ac. des Sciences 1737, p. 206.

[y] Essays, Preface, p. v. [z] P. 287—301.

pletely for himself, as it is printed in the present collection. There is a similarity between Simpson's Prop. II. and the [a]beginning of Bradley's investigation, but there are no grounds for supposing that the one was taken from the other, and it must be perfectly clear from a comparison of them, that if Bradley had altered the demonstration, he never could have done so (as he certainly would in the present instance) without improving it. The whole structure of the demonstration conveys the strong impression of its being the result of an inquiry which was new to the writer of it, and shews an evident unwillingness to omit any thing which might bear upon the subject. The writer, for example, saw that the star's true place was not exactly in the centre of the ellipse which it appeared to describe, and he was possibly not satisfied in the first instance of the extent of error which the neglect of this circumstance might produce. He has, therefore, kept this in view through the whole of his investigation; but if others, by omitting this particular, have simplified their calculations, it must be admitted, on the other hand, that Bradley's demonstration, though it may have been made somewhat more complicated, is also the more complete on this account.

There is nothing from which the precise time can be ascertained when this demonstration was written[b]. The only date which it contains[c], is that of the observation of η Ursæ Majoris, on the 21st March 1728. This might have been used at any subsequent period, but still it may be naturally supposed, that examples would not be taken from any remote occurrence, and at least there would be no improbability in assigning all to the same year. There are two or three circumstances which (though they unfortunately are not conclusive) are perfectly consistent with this supposition.

It will be seen[d] that A is used for the angle of position, and B for an auxiliary angle, which is exactly the same notation as that which is used in the paper on aberration[e]. Again, the maximum is taken at $40''\frac{1}{2}$[f], which is the quantity which Bradley first adopted[g], but which he afterwards considered as too large[h]. Likewise we find the word parallax still used, in its general sense, for the apparent change which had now been discovered, in the same manner as Eust. Manfredi used the word aberration[i] for the changes which might be produced in the relative situations of the fixed stars, from their being seen from different parts of the earth's orbit. How soon each of these words was confined to the specific sense, in which they are now used, is uncertain; there are no traces of it in Bradley's letter to Halley, and in the demonstration of the rules we possibly see the first endeavour to qualify the ambiguous use of them by speaking of "[k]an aberration, or a kind of "parallax[l]."

There is nothing to contradict the possibility of this demonstration having been actually the first step which Bradley made towards reducing his observations; in that case it was probably drawn up in September or October 1728, and it must have cost much thought and trouble. He drew other diagrams, and began his demonstration on three separate sheets, besides those from which it is now printed. The copy likewise which he completed has many insertions and corrections in it, not of a kind which could have been introduced after the whole was finished, as subsequent corrections, but where the pen had been drawn through whole sentences, and others are immediately added to express the same meaning in different words.

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[a] P. 288.

[b] There is no year in the watermark of the paper.

[c] P. 298, 300. [d] P. 296. [e] P. 9.

[f] P. 290. [g] P. 12. [h] P. 29.

[i] De annuis stellarum fixarum aberrationibus, 1729.

[k] P. 287.

[l] After the rules for aberration in declination, Bradley adds, (P. 9.) "It may be time enough "to enlarge more on this head, when I give a "description of the instruments, &c." It would be no way inconsistent with his well known and characteristic diffidence, to suppose that this very demonstration was included in the unassuming "&c."

When this was completed, he had to try the doctrine by the test of his observations. For this purpose he took the annual variation of eight stars, and calculated the maximum of aberration from each[m]. The present bishop of Cloyne took the trouble of recalculating these maxima[n], and his corrected deductions will be found in the last column of the following table.

	Ann. Var.	Max.	Max. Corr.	
	″	″	″	
γ Draconis	39	40,4	40,3	
β ———	39	40,2	40,2	
η Ursæ Maj.	36	40,4	40,4	
α Cass.	34	40,8	41,1	
τ Persei	25	41,0	41,4	
α ——	23	40,2	40,2	
35 Camel.	19	40,2	40,2	
Capella	16	40,0	39,7	
		40,40	40,44	mean.

Of these, the extremes may be seen to be in the maxima deduced from τ Persei and Capella. The first is a star of the 5th magnitude, which could not be observed on the meridian at the time when it was farthest[o] south; and the second varied less in declination than any of the rest. Now Bradley took the annual variations only to the nearest second, and any small fraction, which might have been neglected, would affect the result most, when it had the greatest ratio to the absolute quantity, from which the calculation was made. This, however, has very little effect on the mean of the whole, but Dr. Brinkley (whose authority on such a subject is the highest we can refer to) is of opinion that the result cannot[p] be depended upon to 0″¼. A second, however, in those days was considered as very little, and as the maximum was found from several other stars to be[q] about 40″ or 41″, Bradley assumed 40″,5 as the quantity which might be best taken as the mean of the whole. Mr. Herschell has collected[r] what Lindenau, Dr. Brinkley, and Struve have deduced from their own observations, and what Bessel has calculated from those which Bradley made at Greenwich. The maximum comes out from the mean of all their observations (4724 in number) to be 40″,89; and Mr. Richardson[s], from 2066 observations at Greenwich with Troughton's circle, found it to be 41″,01; and from 2053 observations with Jones's circle 41″004: so that the original estimate does not differ more than half a second from the nearest approximations to the truth, which have even now been made by the best calculators and ablest astronomers.

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[m] P. 10, 11, 12.

[n] Phil. Trans. 1818. p. 299.

[o] P. 12.

[p] Phil. Trans. 1818. p. 298.

[q] P. 12.

[r] Mem. of Astr. Soc. vol. I. p. 425.

[s] Mem. of Astr. Soc. vol. IV. p. 68.

Bradley illustrated the accuracy of the deductions by the application of his hypothesis to a number of observations. He gives fifteen of γ Draconis[t] in every different part of the year, from Oct. 20, 1727 to Sept. 6, 1728, and in no one of these did the observed differ from the calculated declination by more than $1''\frac{1}{2}$: he adds, that in above seventy which he had made of this star in a year, there was but one (and that was noted as very dubious on account of the clouds) which differed from his hypothesis more than 2″, and this did not differ 3″. It may not be superfluous to point out the scrupulous precision with which he expresses himself. If we take the twelve months to be those which ended with the 6th of Sept. 1728, it will be found that 72 observations of γ Draconis were made in this period, and that among these ":: very faint, hazy" is entered[u] against the observation of the 23d of Nov., which is the only one omitted[x] in the deductions that will hereafter be more particularly noticed. Again, fourteen observations of η Ursæ Maj. between Sept. 14, 1727 and Sept. 20, 1728 are[y] given, in which the differences between the observed and calculated declinations never exceed 1″; and it is added, that in about fifty made of it in a year, the agreement was found to be as exact as for γ Draconis. Now in this case we may take the twelve months ending with the 20th of Sept., and it will be found that the Wansted observations for that period contain 48 of the star in question.

It is always satisfactory when we find that independent investigations lead to the same conclusion. Roëmer considered[z] 22′ to be the time taken up by light in its motion across the magnus orbis; but Cassini, when he at first admitted the successive motion of light, thought that this time must be diminished[a] to 14′ 10″. Bradley shews[b], that according to his observations the time must be 16′ 24″, or 16′ 26″, which lie between the other two, and are much more correct than either. Indeed, M. Delambre mentions[c] that he calculated first 500, and afterwards 1000 eclipses of Jupiter's first satellite, and that the maximum of aberration which he deduced from them, agreed in a surprising manner with that which had been determined by Bradley. All these computations must of course be founded on the supposition of the light reaching us with the same uniform velocity from all the heavenly bodies. Bradley considered this to be established[d], and so did Clairaut[e]: but if a difference should be clearly ascertained in the maximum of aberration as derived from the motions of different stars, some[f] modification of this supposition will become necessary. This, however, is a question which requires all the resources of modern astronomy to determine it[g].

After all these computations, the materials were still to be arranged, and the account to be drawn up of the discovery: this to Bradley was a source of no small trouble, and probably was the most irksome part of the task. Besides several loose papers on the subject, we find, 1st, four 4to pages of a beginning, which he afterwards seems to have given up; 2d, the paper nearly completed in about 23 pages, in this he had loosely taken the maximum in general at 40″; but in the 3d, written on 26 pages, he gives it with more precision: this last is the copy mentioned[h] as having the date on it of Jan. 1, 1728-29, but it does not exactly agree with the text printed in the Phil. Trans. He may have written it out a fourth time, or he may have introduced alterations as the paper went through the press. This is fortunately a question of no importance, as it cannot now be settled: the Royal Society in those early times did not preserve the papers which were communicated to them.

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[t] P. 13. [u] P. 223. [x] P. 303. [y] P. 14.

[z] Mem. de l'Ac. des Sciences, vol. I. p. 214. vol. X. p. 577.

[a] Id. vol. VIII. P. 430—31.

[b] P. 11, 12.

[c] Hist. de l'Astron. au xviii siècle, p. 416.

[d] P. 11.

[e] Mem. de l'Astr. des Science 1739. p. 359.

[f] Phil. Trans. 1821. p. 330.

[g] The application of the theory of aberration to the apparent places of planets and comets is so very obvious, that it seemed superfluous to print some rules for that purpose which were found on a loose paper in Bradley's handwriting. They were of course essentially the same with those which are found in all writers who treat on this subject.

Such was the completion of the first of Bradley's great discoveries, which in M. Delambre's[i] opinion assure him "la place la plus distinguée après celle d'Hipparque et de "Kepler, et au dessus des plus grands astronomes de tous les ages, et tous les pays."

In estimating the merits of those great men, who stand foremost in the ranks of their several pursuits, it is invidious to set them in opposition to one another; each has his own distinctive excellence, combined with such varieties of times and circumstances as scarcely admit of a strictly just determination of their comparative merits. Nor will it derogate from the immortal fame of Hipparchus or Kepler, should a place be claimed for Bradley, which is not subordinate even to that high rank which the world has unanimously assigned to them.

CHAP. V.

Lectures in Experimental Philosophy—Bradley's undertaking them about 1729—Candidate for the office of keeper of the Ashmolean Museum—Came to reside at Oxford in 1732—Assisted in the trials of Hadley's sextant—Pendulum experiments in Jamaica—Maupertuis's arc in Lapland—Comets of 1737 and 1742.

IN 1729, Bradley undertook the lectures at Oxford in experimental philosophy. There was at that time no permanent foundation for them, and consequently there is no record of his appointment to the office. Desaguliers tells us[k], that Dr. John Keill at Oxford "was the first who publicly taught natural philosophy by experiments in a "mathematical manner[l]," and that he began to do so about 1704 or 1705. In 1709, Keill went to New England as treasurer to the Palatines, and then Desaguliers himself "began to teach experimental philosophy after the same method" in the university. This he began in 1710, and continued till he went to settle in London about 1713. Three or four years seem afterwards to have elapsed before any other person engaged in a similar undertaking; for there is a letter in the Bodleian from Dr. John Richardson to Dr. Charlett, (dated June 11, 1716,) in which he speaks with approbation of Mr. Whiteside's "designe of experimental philosophick lectures." It is probable, therefore, that the lectures were renewed in this or the following year, and Whiteside certainly continued them till 1728. Hearne[m] has a memorandum on Jan. 24, 1729, of his "having

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[h] P. xii.

[i] Hist. de l'Ast. au xviii. siècle, p. 420.

[k] Exp. Phil. vol. I. Preface.

[l] This has no reference to the lectures contained in the Introductio ad veram physicam, which were read several years earlier by Keill, for sir Thomas Millington, who was Sedleian Professor of Natural Philosophy.

[m] N°. 124. p. 121.

"sold his apparatus, the best that is known, for more than four hundred lib^s. to "Mr. Bradley," but he afterwards corrects his account of the price, for (1730, Feb. 13[n]) he says that Bradley, he was informed, "gave only an hundred and seventy pounds, "tho' I had been before told, that he gave four hundred lib^s. for them. Mr. Whiteside, several years agoe, valued them at five hundred lib^s. and at last at eight hundred "lib^s." Desaguliers read his lectures in Hart-hall, of which he was a member; Whiteside was a chaplain of Christ Church: he was also keeper of the Ashmolean Museum, and having divided the entrance room of it into two, it is said that when he had finished the delivery of his lecture in the one, the class removed into the other, where a servant exhibited to them the experiments which illustrated the subject on which they had been engaged. Bradley was accommodated with the rooms when he purchased the apparatus, and the indulgence was afterwards extended to his successors.

Bradley's character was established by the discovery of aberration[o], and he probably was successful from the beginning of his new engagement, but we have no document to give us information on this point at so early a period. There is, however, a book which contains the lists of all the persons who attended each of his courses of lectures, from April 1746 (when he began his 47th course) till April 1760, when he read for the 79th, and probably last time. From these accounts it appears, that till the end of 1749 he was in the habit of lecturing three times in the year; between Christmas and Easter, between Easter and the long vacation, and again in Michaelmas term. After 1749, however, he did not repeat his course in the autumn; about this time he procured the new instruments at Greenwich, and could less afford to be absent from his astronomical duties there. The 33 classes, of which we have the particulars, average 57 attendants at each, and as he received two guineas from every pupil for the first, and one guinea for the second attendance, his annual receipt must have been considerable. This afterwards received a small increase. Lord Crewe, bishop of Durham, died in 1721, and charged his estates with the payment of 200*l.* a year to the university, 30*l.* of which was set apart by decree of convocation in 1731 for "a reader of experimental philosophy." Lady Stawwell having a rent charge for her life on Lord Crewe's estates in Durham and Northumberland, he directed that his benefaction should not be payable till after her death, and it was not therefore till 1749 that Bradley derived any benefit from it.

In 1731, Jos. Andrews, a fellow of Magdalen college, became keeper of the Ashmolean Museum; Hearne[p] says, "it was expected at London, and by the most understanding men "of the university and much wished too, that Mr. Bradley, Savilian Professor of Astro-"nomy, should have had this post, he being a person every way qualified with respect to

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[n] N^o. 125. p. 9.

[o] Among the old apparatus belonging to the lectures in experimental philosophy at Oxford, there is a small machine to illustrate the doctrine of aberration. Bradley never wrote out his lectures, and in the notes from which he delivered them, no notice of it can be discovered; but still there is every reason to believe that it was his own contrivance. It consists of a box, the lid of which is made to slide laterally by a screw which is under it, and to the axis of the screw the two extremities of a silken thread are attached. By this construction one end is wound up on the cylinder while the other is unwound, and a mark on the thread representing a particle of light is thus made to move at right angles to the direction in which the lid is carried. The mark passes over the diagonal of a parallelogram, the sides of which represent the two motions, and shews the apparent direction in which the light would enter the eye.

[p] N^o. 129. p. 138.

"his skill in mathematics, (though he is no antiquary,) and being a man that performs "courses of experiments at the Museum in the great lower room:" but the junior proctor was the only one of the electors who was inclined to support him, and he therefore lost the election.

Bradley's engagements now called for longer residence in the university than had before been necessary: therefore in the beginning of May 1732[q] he removed there, and occupied the house in New College lane, to which he was entitled in right of his professorship. Lalande says[r] that he went at this time to Oxford, "pour remplacer Halley." Halley indeed resided very little there in the latter years of his life; but there is no reason to believe that Bradley ever gave the lectures in geometry for him. There is probably some mistake which may never now be cleared up, nor is it of much importance that it should; of the fact, and the real cause of it, there can be no doubt.

The removal to Oxford did not separate the family party, his aunt accompanied and lived there with him[s] for several years, she likewise carried with her two of her nephews, the sons of her brother Mr. Wymondesold. Bradley took most of the instruments which he had, with him; the transit[t] he fixed in one of the windows of the Museum; but where he used the 15f. telescope is uncertain; there was no place for it either in his house or in the little court before it, and the Museum afforded no accommodation, for all the windows on the south side open either from the staircase or very small rooms. The sector was certainly left at Wansted in the place where it was first[u] erected, and we are now able to clear up a difficulty which must obviously have occurred from the short notices which we formerly had respecting it. If the instrument had been in Mr. Wymondesold's house, as seemed probable from what is said in the paper on nutation, there was no obvious reason why the observations should have been impeded after Bradley went to reside at Oxford[x]. They had been continued unremittingly for nearly five years, and it must naturally be supposed that the telescope would have been originally fixed in that part of the house, in which it could be used at night with the least danger of incommoding the family. Changes might indeed have taken place in the arrangements of the household; but there is a simpler way of reconciling the account with the real state of the case. It appears from the old parish rate books at Wansted, that Mrs. Pound's house had become vacant in the course of 1732, and continued empty for the two following years, after which it was occupied, as has been before mentioned, by a Mrs. Elizabeth Williams. It likewise appears from Bradley's letter to Dr. Smith, that when he was at Wansted at Christmas 1732, he was living in Mr. Wymondesold's family[y]. It is probable, though it is not certain, that he continued to do so afterwards. While, therefore, he lived in one house and observed in another, it can be easily understood that there was a call on him not to do so "at such hours of the night as would have incommoded "the family of the house wherein the instrument was fixed[z]."

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[q] P. 22.

[r] §. 2891.

[s] Mrs. Pound continued to live with Bradley at Oxford till 1737; she died Sept. 10, 1740, and was buried at Wansted, according to the directions of her will, in which, after a few legacies, she gave the remainder of her property to Bradley and her brother.

[t] P. 365.

[u] P. 18.

[x] P. 30.

[y] P. 401. In accordance with what is mentioned in the note (P. 403.) it will be seen (P. 269) that Bradley was at Wansted on the 22d of Dec. 1732, but did not make any observations till the 30th.

[z] P. 30.

In 1731[a] Hadley published the account of his valuable instrument for measuring angular distances. Franklin and others have claimed the invention for Thomas Godfrey, of Philadelphia; but Hadley ought not to lose the honour which in this respect is justly due to him. Human knowledge is gradually progressive, and many an invention results from merely a happy combination of what had been previously known. Minds of a higher order occasionally advance a step which may yet be wanted; but even that step, in general, could not have been gained, if the ground had not been made good by their predecessors. For these reasons discoveries have been repeatedly made nearly at the same time by different individuals; and there is no argument to be drawn merely from their being simultaneous that the one was borrowed from the other. Hadley most probably knew nothing of what had been done by Godfrey, and if mere priority of time is to be considered, it is now clear that the principle, on which the instrument was constructed, had occurred many[b] years before to sir I. Newton. The lords of the Admiralty ordered Hadley's instrument to be tried at sea in 1732[c], and Bradley went out with him for that purpose in the Chatham yacht. The observations were chiefly directed to altitudes. Hadley himself speaks with doubt at the end of his first paper on the use, which might be made of his sextants in taking lunar distances, but Bradley in his letter to Hadley[d] expresses himself with confidence of the advantage to be derived from this means of finding the longitude at sea. It is remarkable that the very first observation which he made when off Gravesend, was on the distance of two of the heavenly bodies, which evidently shews that he immediately looked to the most important use which was to be derived from the new instrument.

In 1734, Bradley communicated a paper[e] to the Royal Society on the vibrations of the pendulum in different latitudes, and on the figure of the earth which might be deduced from them: it will be found at P. 62.

Colin Campbell, F. R. S. a gentleman who had been brought up under the care of his cousin Archibald Earl of Isla, (who was afterwards Duke of Argyle,) established an observatory at Black River in Jamaica. He furnished it with a valuable set[f] of instruments, among which he had a transit[g], and an excellent[h] mural arch of four feet radius, made by Sisson. He employed Graham likewise to make an astronomical clock for him, the rates of which were the foundation of the dissertation in question. The precise length of the pendulum swinging seconds, was a subject in which Graham through the whole of his life took very great interest[i]. Bradley likewise had paid particular attention to it, and probably had his share in suggesting and arranging what Graham in a[k] letter to him on the subject calls, " our experiment." The method which they adopted, was to ascertain the number of vibrations made by a pendulum, which was unvaried by any thing but the difference of temperature, at the two stations where it was made to swing. Graham's own account of his previous observations and experiments, mentioned in P. 64, is extant, and a memorandum is written on it and signed by Bradley, stating that it was delivered to him on the 9th of September 1731. The contents of P. 63, and of the first half of P. 64, are accurately detailed in it; from which it will be seen, that the number of vibrations made in twenty-four hours was reckoned by ascertaining the rate of the clock from the transits of α Aquilæ. The contrivance by which "its[l] pendulum "might be reduced to the same length whenever there should be occasion to remove the "clock," is stated in the MS. "The nut of the pendulum," Graham says, "which raises "or lowers the bob is divided into twenty-six equal parts, one of which corresponds to one "second a day. When the clock was observed by the star, the index at the lower end "of the pendulum pointed to the fifth division upon the nut: and there was a piece of "brass filed of an exact length to reach from the highest edge of the bob to the upper "edge of the piece of brass that goes through the bob, to determine at what turn of the "screw the nut stood, and the fifth division of the nut determines the part of the turn. "This piece of brass was sent with the clock, and lest any mistake should arise in applying the piece of brass, I cut a piece of wood into the form of this part of the pendulum, and tied on the piece of brass with some packthread, to shew in what manner it "was to be applied to the pendulum. Was the nut to be one turn too high or too low, "the piece of brass would reach above or fall short of the edge before mentioned, by "the space that is between one thread of the screw and the next, which is too great a "quantity to be overlooked, or not readily perceived."

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[a] Phil. Trans. vol. XXXVII. p. 147.

[b] Phil. Trans. vol. XLII. p. 155.

[c] Phil. Trans. vol. XXXVII. p. 341.

[d] P. 505.

[e] The original manuscript of it is at Oxford.

[f] P. 62. [g] P. 66. [h] Smith's Optics, §. 870.

[i] Abridgment of Phil. Trans. (1809) vol. VI. p. 537. [k] P. 395.

With these precautions the pendulum was set going in Jamaica, and the rate of the clock which was regulated by it was observed by Joseph Harris, whom Mr. Campbell took over with him to assist in[m] erecting his observatory. Observations of α and β Canis Majoris were continued for about a month[n], and they went to prove that the clock lost 2′ 6″½ in a sidereal day, in consequence of its removal to Jamaica[o]. The original memorandum of all these observations, and the copy[p] also which was made for Bradley, are still extant, and they contain very little more than the statement which is printed from them. Observations of Jan. 22, and Feb. 19 and 20, have been omitted: there is a notice likewise of the transit's having been disarranged on the 2d of Feb. by the unusual heat of the weather, and it is stated that the pendulum swung in different cases 1° 52′ and 1° 55′; 1° 50′ and 1° 53′ on each side of the 0 on the graduated arc by which the extent of the vibrations were measured. Graham was persuaded that a small difference in this respect could cause no great difference in the duration of the vibration[q]; and the mean extent is therefore only stated as being "about 1° 52″[r]."

Jos. Harris's health prevented his remaining in the West Indies, and there appears to be no record which can determine whether the inquiry was prosecuted any further. The original paper ends with a memorandum, stating, that on the 21st of February the pendulum was shortened 124 divisions, so as to make it vibrate sidereal seconds. The further observations of Mr. Campbell[s] would therefore have been principally useful in determining the effects of temperature, for which an allowance was made on rather indefinite[t] grounds. Nothing else seems to have been taken into consideration, and although there might have been reason at the time to esteem "Mr. Campbell's experiment[u] to be "the most accurate of all that had hitherto been made," still there were many things wanting to give precision to the results deduced from it. James Stirling, of Ball. coll.

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[l] P. 63.
[m] P. 64.
[n] P. 65.
[r] P. 66.
[s] P. 397.
[t] P. 397.
[o] P. 67.
[p] P. 395, 396.
[q] P. 64.
[u] P. 69

a very eminent mathematician, was employed at this time on the figure of the earth, and in 1735 he printed a paper on it in the [x] Phil. Trans. His letter to Bradley [y] was evidently connected with that inquiry. Bradley in his answer [z] communicated all the information he could, and suggested the probable variation of the effects of gravitation from different circumstances on the earth's surface, and particularly from the different degrees of density between earth and water, which it is curious to see adopted by Stirling in the latter part of his paper. The great advances which have of late been made, carry us far beyond these early disquisitions; but the first steps were slow, and the results of Mr. Campbell's experiment, though impugned by [a] Stirling, seems for a certain time to have been a basis for the calculations made with respect to this subject; and it was no exaggeration for Machin to have said, "[b] that sir Isaac Newton would have been much "pleased with it had he been living." Another similar to it was afterwards repeated. Among Bradley's papers there is a disquisition by And. Celsius, founded on the going of a clock made likewise by Graham for the observatory at Upsal, and he says, "ex "nostra observatione sequitur, gravitatem Londini ad gravitatem Upsaliæ eam servare "rationem, quam habet 10000 ad 10006."

As soon as the French academicians returned from the measurement of the degree in Lapland, Maupertuis hastened to communicate the result of the observations to the Royal Society. He did so (Sept. 1737) in a letter to Bradley, whose translation of it has been inserted in the following collection [c]. This was made most probably for Graham, and the letter [d] in which the translation was transmitted to that great artist is very curious. It is well known that the measurement had long been suspected of some inaccuracy, and that it was repeated in the beginning of the present century by M. Svanberg, who found the degree 223 toises less than the French academicians had calculated it to be. The source of the error was in a mistake of the latitude of Kittis. M. Delambre [e] is inclined to argue against the probability of its having amounted to the great extent which the later observations indicate, of 12″ in the amplitude of the arc. The only fault, he says, of which the French astronomers can be justly accused, is their having neglected to reverse their sector, and this is the very particular which Bradley [f] pointed out with regret as soon as he received the account of their labours. His clear mind at once detected the very point on which, after a lapse of more than sixty years, they were found to be deficient.

The comet of 1737 recalled Bradley's attention to the subject on which he first ventured to appear before the public. In the following year he communicated to the Royal Society [g] the observations which he had made, and likewise the elements of the parabola which he found to give the places of it without ever differing from the truth by 1′ of space. Indeed there is some reason to wonder at his having been able to approximate so well to the truth, for the comet was a small one, and its light (" even in the moon's "absence) so very weak, that it was difficult in some of the later observations to take "its place with any tolerable certainty [h]." He expresses himself [i] "sensible that a little "error, either in the observations themselves, or in the places of the fixed stars with

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[x] Vol. XXXIX. p. 98. [y] P. 398. [z] P. 400.
[a] P. 399. and Phil. Tr. vol. XXXIX. p. 104.
[b] P. 397. [c] P. 404. [d] P. 406.
[e] Astronomie, vol. III. p. 523, 4.
[f] P. 407. [g] P. 49. [h] P. 51. [i] P. 49.

"which the comet was compared, might occasion a considerable difference in the situation and magnitude, &c. of the orbit deduced from them;"...but still he was unwilling to keep back the account of his own observations which he had made at Oxford. Dr. Bevis, who then had an observatory at Newington, undertook to examine the places of his stars for him. This may be collected from a paper on which Bradley has noted in the margin, "The stars (observed by Dr. Bevis) with which the comet of 1737 was compared." These observations, however, were probably of little service; Dr. Bevis's transit, from the particulars which he mentions of it in another place[k], does not seem to have been an instrument which could be depended upon, and of sixteen stars, of which in this case he gave the right ascension, he committed no less than three mistakes; against two of them Bradley has written, "Not the right star," and to another there was added, "Qu.? is this the right star?" to which Bradley has answered, "No." There are other observations of the comet in the Phil. Transactions, and the Mem. of the R. Ac. of Paris; but Bradley does not appear to have made any use of them in endeavouring to give further precision to his elements. It happened that the comet was one of no great interest: Machin indeed pointed out some circumstances[l] in which he thought it had a similarity to that of 1556; if these had been confirmed by other subsequent observations, it would have been of importance to calculate its orbit more minutely; but discrepancies on the other hand were detected, which induced even the author of the suggestion to abandon[m] it.

In the beginning of 1742 another comet was first observed at the Cape of Good Hope, and afterwards in Europe[n]. The following elements of it have been found in Bradley's handwriting.

		°	′	″
Loc. ☊	6^s	5	27	30
Inclinat.		66	59	50
Temp. perih. Jan. 28. 4h. 22′				
Loc. perihel.	7	7	34	30
Perih. a ☊		31	57	0
Log. dist, perih. 9,88396				
Los. mot. diur. 0,134188				

There is a letter of Gael Morris, in which he gives the computed longitudes and latitudes of the comet from Feb. 21 to March 15, and he says in it that the nearest agreement he could make was by retaining this inclination, and taking 6^s 5° 36′ 50″ for the place of the node, with 31° 56′ 50″ for distance of it from the perihelion.

Only one[o] observation has been found which can be supposed to belong to this comet, which nevertheless is intimately connected with the history of our author. Lacaille says[p], "La comète de 1742 fût assez remarquable: elle réveilla l'attention des astronomes, qui pour la plûpart commençoient à suivre les principes de Newton. Halley étoit mort depuis peu, et M. Bradley étoit regardé comme le dépositaire de la méthode du calcul des comètes. Il trouva en effet tous les elemens de la théorie de celle-ci avant même qu'elle disparut[q]; il les communiqua de plus

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[k] P. 416.

[l] Phil. Trans. vol. XL. p. 123.

[m] Pingré Cometographie, vol. II. p. 45.

[n] Ibid. p. 47.

[o] P. 371.

[p] Introduction aux Ephemerides 1765-1775 p. xliii.

[q] G. Morris's letter is dated March 14, 1742.

" dans une lettre particuliere adressée à Paris dans le mois de Septembre de cette
" même année, avec la méthode qu'il employoit à ces sortes de recherches. Ce fût donc
" à l'occasion de cette cométe que l'on commença à écrire differens traités sur la théorie
" de ces astres, selon les principes de Newton. Un seul d'entre leur auteurs avoit eu
" communication du procédé de M. Bradley; mais les autres avoient fait de leur côte
" d'heureux efforts et s'étoient frayé chacun une route particuliere, de sorte qu'au mois
" de Juillet 1743, à l'occasion d'une petite cométe qui avoit paru au commencement de
" l'année, M. Maraldi lut à l'Académie des Sciences la première théorie de cométe qui
" ait été calculée en France."

Le Monnier published his Théorie des Cométes in 1743, and acknowledges[r] the obligation he was under to Bradley; he must therefore be the single individual above alluded to. Delambre[s] complains of the manner in which the explanations are given in that work, and though he at first rather connects the obscurity with Bradley's communication, he afterwards inclines to attribute it to the want of order and perspicuity in Le Monnier, who was no favourite among his countrymen. With whom the alleged fault may have originated is not worth discussing, but we must repel the ungenerous[t] conclusion, that Bradley (with Halley and Le Monnier) was mean enough to indulge the wish of keeping his method a secret from the world. Bradley had too much good sense to think that such a secret was to be kept. In the very first paper which he published, he distinctly stated[u] that he calculated his parabolas by sir I. Newton's method, and if some improvements may have occurred to him in practice, he knew that others who applied to the subject would most probably be able to acquire these same advantages for themselves, and even (as the event immediately proved) to devise new methods that might be more useful than that by which his calculations had been guided. We have not his letters to Le Monnier on the subject, but even if they were not so copious as the historian might have wished them to be, still the fact might be accounted for without recourse to any suspicion of dishonourable motives. Bradley disliked writing, and to a scientific correspondent he might be unwilling to enter into details which he would presume to be unnecessary. It is but just likewise to take into consideration, that the communication was probably made when his mind was occupied by the most important event of his life—when he was taking possession of the observatory at Greenwich, and preparing for the discharge of his duties as Astronomer Royal[x].

CHAP. VI.

Bradley succeeds Halley as Astronomer Royal—Created D. D.—State of the instruments at Greenwich—Observations made in 1742.

HALLEY had reached his grand climacteric when he succeeded Flamsteed in 1719 as Astronomer Royal; but depending on the great natural strength of his constitution, he did not hesitate to enter on a series of lunar observations which required a period of nearly twenty years for their completion. He seems to have been proud of his physical powers, and Hearne says[y], " Dr. Halley (now in the 72d year of his age) does not care

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[r] p. xlii.

[s] Hist. de l'Astr. au xviii siècle, p. 190.

[t] Ibid. p. 195.

[u] P. 46.

[x] This same circumstance may account for the fewness of the observations which he made of the comet.

[y] N°. 118, p. 106.

" to be thought old:" indeed he felt no indication of decay before 1737, when his right hand was attacked by paralysis. This, however, seems not to have prevented him for some time from active exertion, or even the enjoyments of life: but the disease increased upon him; and when he found himself failing, he was anxious to secure Bradley for his successor. It is said that he was even willing to have resigned in his favour, and that the letters[z] in which he urged these wishes were found among Bradley's papers. It is to be regretted that the search which was made for these proofs of Halley's kind feeling and zeal for the promotion of astronomy has been unsuccessful; it is to be regretted, likewise, that he had not the satisfaction of seeing so excellent a design carried into execution: he died on the 14th of January 1742[a], and Bradley's appointment as his successor did not take place till the following month.

Bradley's success has been generally (and there is every reason to believe justly) attributed to the [b]friendship and patronage of George earl of Macclesfield; but neither the warmth of that friendship, nor the manner in which it was exerted, have been properly known. The two following[c] letters, however, (the originals of which are at Shirburn Castle,) will shew the caution as well as the zeal with which it was made to bear on this great object. From them it appears that his lordship, at the time, was not acting in support of the government: it was not, therefore, his political interest which prevailed; it was the weight which was justly attributed to his opinion on scientific subjects, and which must have been supported by the united testimony of all who knew and were interested in the state of astronomy.

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[z] Hist. de l'Ac. des Sciences, 1762, p. 238. Supp. to Biog. Dict. p. 56.

[a] Biog. Brit. 1757. vol. IV. p. 1516.

[b] Hist. de l'Ac. R. des Sciences 1762, p. 238. Supp. to Biog. Dict. p. 56.

[c] Lord Macclesfield appears to have preserved no part of the correspondence which took place between him and Bradley, and these letters were only saved by circumstances in which the scientific world is much interested, and concerning which it is of some importance that the truth should be distinctly known.

Dr. Hutton says, (Dict. art. Jones,) that sir William Jones's father possessed the " best " mathematical library in England; scarcely " any book of that kind but what was there to be " found. He had collected also a great quantity " of manuscript papers and letters of former " mathematicians... After his death these were " dispersed, and fell into different hands," &c. Now Mr. Jones says in his will, " I bequeath " to the right hon. Geo. earl of Macclesfield my " study of books as they stand in my catalogue, " and any additions which I may hereafter " make thereunto." He also appointed lord M. to be one of his executors; and as his lordship was regularly admitted to act in that capacity, it is very improbable that he should not have taken possession of a legacy, which on every account must have been particularly valuable to him. We find, indeed, that a library of the earl of Macclesfield was brought to the hammer in 1765, but any one who would have taken the trouble of examining the sale catalogue would have found that no part of Mr. Jones's collection could have been contained in it: in truth, it was only the portion of books which happened to be in his lordship's house in St. James's square, and though it was considerable, the dispersion of it could have been attended with little loss to the family, and none to the world. In the library of the Royal Society there is a transcript of one of the Oxford MSS. of Pappus (Bib. Sav. N°. 3.) which was given by Mr. Jones. This and other presents which he made during his lifetime, may have led to the idea which Dr. Hutton entertained of the books having been dispersed. There seems to be no other foundation for the story. The invaluable collection, in the highest order, and in the best state of preservation, is now in the library of Shirburn Castle. Dr. Hutton's description of its contents is by no means overcharged. The printed books consist of the most valuable and some of the scarcest of the old writers on mathematics and science; and the manuscripts, which are very numerous, contain many of the greatest interest, and a collection of letters from Petty, Fermat, Pell, Brook Taylor, Newton, Barrow, Huygens, J. and D. Gregory, Hooke, Cotes, Halley, Flamsteed, Wallis, &c. &c. &c. It was among these manuscripts that the two letters were found which are printed in the text; they were fortunately preserved by Mr. Jones, and accidentally returned with his other treasures to the nobleman who did himself so much honour by writing them.

" TO WILLIAM JONES, ESQ.

" in Beaufort's Buildings in the Strand, London.

" Dear Sir,

" I received your's yesterday in the afternoon, and the shortness of the days, together " with my having company at dinner, prevented my writing by the last post, but I have " by this post written a long and pressing letter to the Chancellor[d] in behalf of Mr. " Bradley, whom you know how rejoiced I should be to have it in my power to serve. " But my way of thinking and voting is a great obstruction to having any interest at " court, nor can I think of any other way I can recommend Mr. Bradley to this professorship, than by means of the Chancellor and Archbishop[e]. I do not know upon what " footing my kinsman Mr. Justice Parker is with Lord Chief Justice Willes, it is certain " the latter has great interest with sir Robert, and it may be of advantage if cousin " Parker would state the thing in a proper light to Willes, and represent to him how " much it will be for sir Robert's discredit, that the only man in England fit to succeed " Halley at Greenwich, confessed so by all who know any thing of astronomy, should " be put by, and the finest instrument in the universe put into the hands of a person " unable to make a proper use of it. I dare say my cousin's friendship for me will make " him undertake this part, and endeavour to prevail upon Willes to interest himself for " Mr. Bradley, if there be no impropriety in the thing: I wish, therefore, you would " talk to him from me, and shew him both this and the enclosed copy of my letter to " the Chancellor, which will contribute possibly to make him master of the case, and " enable him to talk to Willes the better upon it. If your health should not permit " you to call upon Mr. Justice Parker yourself, I would have you send for my cousin " John, and explain the thing to him, and desire him in my name to go to the Judge, " and represent the thing to him, and desire his assistance in the affair so far as may " be consistent with the footing he may at present be upon with Willes, or, in short, so " far as he shall think convenient. Mr. Folkes might speak to the duke of Richmond, " you to sir Charles Wager, and lord Charles[f] to somebody or other, for I think nothing " should be left undone that can at present be done for our friend, this being the only " time for advancing him in the way he chooses.

" Lady Macclesfield and myself have both had colds, but both are better at present, " and hope to arrive in good health in town on Saturday about five o'clock, when if I " do not see you, I should be glad to hear by a line or two what prospect there may be " of Mr. Bradley's succeeding.

" I am, dear sir,
" Your affectionate friend, and humble servant,
" MACCLESFIELD."

Shirburn, Jan. 14,
1741-42.

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[d] Lord Hardwicke. [e] Dr. Potter. [f] Probably lord C. Cavendish.

My Lord,

"Yesterday I received notice that Dr. Halley could not hold out longer than a day "or two, and I hope your lordship will pardon my troubling you with this in behalf of "my friend Mr. Bradley, whom you formerly seemed inclined to serve whenever "Dr. Halley's death should make a vacancy at Greenwich.

"It is not the salary annexed to that professorship which makes me so desirous that "Mr. Bradley should succeed Dr. Halley in it, but there is a credit attending such a "professorship when possessed by a man of real merit; and it is a disappointment to, "and a sort of slight put upon, such a person, when upon a vacancy he is neglected, "and a person much inferior to him is preferred before him: and give me leave to say, "that must be Mr. Bradley's case, whosoever except himself succeed Dr. Halley; and "besides Mr. Bradley's abilities, he has so very great a liking to the practical part of "astronomy, the making observations, that on that score it would be extremely agreeable to him, and the science would have the greatest reason to expect to receive very "considerable improvements from his observations.

"But it is not only my friendship for Mr. Bradley that makes me so ardently wish "to see him possessed of the professorship, it is my real concern for the honour of the "nation with regard to science. For as our credit and reputation have hitherto not been "inconsiderable amongst the astronomical part of the world, I should be extremely "sorry we should forfeit it all at once by bestowing upon a man of inferior skill and "abilities, the most honourable, though not the most lucrative, post in the profession, "(a post which has been so well filled by Dr. Halley and his predecessor,) when at the "same time we have amongst us a man known by all the foreign, as well as our own "astronomers, not to be inferior to either of them, and one whom sir Isaac Newton was "pleased to call the best astronomer in Europe. This will, I flatter myself, plead my "excuse, if I should appear a little importunate in pressing your lordship to intercede "early and earnestly in favour of Mr. Bradley, nor can I apply on this occasion to a "more proper person than [your lordship]. For as this place has no relation to any department of the administration, but its sole business and view is the advancement and "improvement of the science that is of use to mankind in general, but more particularly "so to us, as a trading nation, and the chief of the maritime powers; this, I say, being "the nature of the place, to whom can the recommendation to it more properly belong "than to your lordship, who not only in private character, but by your public office likewise, are the patron of learning and learned men in general: it was upon this foot that "my father, when in the post which you now enjoy, took upon him[g] to recommend Dr. "Halley to the royal professorship at Greenwich, and Mr. Bradley to the Savilian at "Oxford, and succeeded in both his recommendations; and he always thought it for his

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[g] Whiston, in his Memoirs, vol. I. p. 252, mentions this very circumstance. He says, that (although, as he acknowledges, very unfit for the situation) he was persuaded by his friends to endeavour, after Flamsteed's death, to obtain the situation of Astronomer Royal; that he applied in consequence to "his very "valuable friend and patron, the then Lord "Chancellor Parker," who told him "that he "had spoken already to the king for Dr. "Halley."

At p. 272 of the same volume, Whiston speaks also of his "great friend and patron, "Sam. Molyneux, Esq." which leaves little doubt with respect to the conjecture which is made in P. 161, note [l].

" honour to have recommended two so able men. And I dare assure your lordship, that if " you shall be pleased to espouse Mr. Bradley's interest, you will have the satisfaction to " find your recommendation of him approved and applauded universally by those who " are versed in those studies, both at home and abroad. As Mr. Bradley's abilities in " astronomical learning are allowed and confessed by all, so his character in every " respect is so well established and so unblemished, that I may defy the worst of his " enemies (if so good and worthy a man have any) to make even the lowest or most " trifling objection to it. After all, it may be said, if Mr. Bradley's skill is so universally acknowledged, and his character so established, there is little danger of " opposition, since no competitor can entertain the least hope of success against him. " But, my lord, we live in an age when most men, how little soever their merit may " be, seem to think themselves fit for whatever they can get, and often meet with some " people, who by their recommendations of them appear to entertain the same opinion " of them, and it is for this reason that I am so pressing with your lordship not to lose " any time, as I am confident you would be sorry the professorship should be given to " a person unqualified for it, and the finest instrument perhaps in the universe put into " the hands of a person unable to make a proper use of it, and this to the prejudice of " the best qualified and most able astronomer, that not only this nation, but probably " all the world can at present shew.

" But I forgot how precious your lordship's time is, &c."

There is something highly gratifying in the merited success of such a recommendation. The unrivalled character which Bradley had established as an astronomer marked him as the fittest person for the situation; but the vacancy occurred just at the moment when the opposite party drove sir Robert Walpole from office, and in those times, and under such circumstances, merit could not be sure of its reward. But he was not deprived of it, or rather the world was not deprived of Bradley, and the minister ought not to lose the honour which he deserves for having acknowledged and rewarded his talents. It was on[h] the 2d of February 1742, that the administration was finally defeated in the house of commons, and on the following day an adjournment took place previous to sir Robert's resignation on the 11th. Now Bradley's appointment was dated on the 3d[i]. It appears, therefore, that one of the first things which Walpole looked to, as soon as he had determined to retire, was to make use of the power which yet remained in his hands, to secure the office of astronomer royal for the man who of all others was the most deserving of it.

Bradley had not proceeded further than to the degree of M. A. but the university now created him D. D. by diploma, a distinction which is never granted at Oxford but under very special circumstances; more particularly to resident members, who may be expected to proceed by regular advance in academical graduation. In the registers of the proceedings of convocation we find the consent of the chancellor (the earl of Arran) to the measure, in consideration of Bradley's extraordinary merit and his particular services to the university, and the form given to the instrument will best express the sentiments of those who were most intimately acquainted with him.

[h] Coxe's Memoirs of sir R. Walpole, vol. I. p. 695.

[i] Suppl. to Biog. Dict. p. 56.

“ Cancellarius magistri et scholares universitatis Oxoniensis omnibus, ad quos hoc præ- “ sens scriptum pervenerit, salutem in Domino sempiternam.

“ Cum eum in finem gradus academici a majoribus nostris prudenter instituti fuerint, “ ut viri de literis et artibus ingenuis optime meriti iisdem insignirentur, quorum in “ numerum vir egregius Jacobus Bradley, A.M. Astronomiæ Professor Savilianus summo “ jure ascribatur, qui in literis reconditioribus eruditionis suæ laudatores e quavis fere “ gente præstantissimos et paratos daret, si inter suos Oxonienses meritorum suorum lo- “ cupletes et certissimi deessent testes, quem doctorum Halleii laborum multos per annos “ socium fuisse et levamen novimus, ejusdem etiam amicitia ornatum, famæ æmulum; “ cumque talem tantumque virum scientiæ suæ, extincto Halleio, coryphæum tam bene “ lateque notum justissimis exterorum præconiis celebratum, regio demum favore ad pri- “ mum astronomiæ sedem evectum, omni modo academiam deceat sua etiam honoris “ tessera cohonestare.—Placuit almæ matri filio sui amantissimo de se optime merito, mul- “ tis nominibus ipsi caro, illustrissimum amoris et observantiæ testimonium palam perhi- “ bere, eundemque honestissimis insignibus decorare:—Sciatis igitur quod nos cancella- “ rius, magistri et scholares antedicti eundem doctissimum virum Jacobum Bradley die “ vicesimo secundo mensis Februarii anno Domini millesimo septingesimo, quadragesimo[k] “ primo, in solenni et frequentissimo doctorum magistrorum regentium et non regentium “ senatu, doctorem in sacra theologia renunciavimus et constituimus, eumque virtute “ præsentis diplomatis omnibus et singulis juribus privilegiis et honoribus ad gradum “ doctoris in sacra theologia quaqua pertinentibus frui et gaudere jussimus.”

The lectures in experimental philosophy probably detained Bradley in Oxford during the spring; for he did not go to reside at Greenwich till June[l]. When he went there, he was at first unable to do much from the wretched state in which he found the instruments. That, which lord Macclesfield speaks[m] of as perhaps the finest in the world, must have been Graham's; but the [n]roof of the room in which it was placed “ was so “ near the top of the stone wall, that at some seasons of the year the warping of the “ timber would cause the leaden weight, which balanced the telescope of the quadrant, to “ rub against the boards, so that in some positions it required a pretty considerable “ degree of force to move the telescope, which may be supposed the reason why the too “ great stiffness, with which it turned about the brass cylinder, was not duly attended to; “ otherwise the accident which happened of breaking the screws, with which that cylin- “ der was fastened to the centre plate, might have been prevented by putting a little oil “ to it.” As soon as this was discovered, the parts were taken off for Graham to make the necessary repairs; and it was found that “ the centre cylinder then adhered so firmly “ to the steel collar, that it required a considerable force to get it out.” There was likewise no apparatus for illuminating the wires of the telescope. Halley, indeed, seldom attempted to observe when this was necessary, and, when he did, he only placed a candle on the south end of the pier, so that its light might be reflected back from a shutter which slided perpendicularly[n]. Bradley introduced the lamp with an elliptical screen at the end of the telescope, such as was contrived and used at Kew in 1725[o].

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[k] The date is really Feb. 22, 1741-42, which answers to the latter year and not to 1741, according to the new style.

[l] P. 381.

[m] P. xlvi, xlviii.

[n] P. 381.

[o] P. 107.

Flamsteed provided his own instruments, and laid out more[q] than 120 pounds on the construction of that which Abraham Sharp divided for him. His executors removed them, and Halley was obliged to procure others. In 1721[r] his transit[s] was set up, it was 5 feet in length with an aperture of 1$\frac{3}{4}$ inch. It turned upon an axis of 3$\frac{1}{2}$ feet, but the telescope was heavier at the end in which the eyeglass was fixed, so that for any observation it required to be supported[t] at the proper altitude. The tube also was much nearer to one end of the axis than to the other, a construction which Dr. Hornsby[u] conjectures to have been occasioned by Halley's having been obliged to accommodate himself to the place in which the instrument was to be used: but the true cause was a wish to prevent the axis, if possible, from bending with the weight of the instrument. Pound's transit was made in the same manner, and it was the construction used by Roëmer[x] in the very first which was ever made. The principal inconvenience resulted from the impossibility of adjusting the line of collimation on the same meridian mark when the axis was inverted. This is the reason why Bradley was obliged to have more than one "spot[y]" at Wansted for that purpose, till Aug. 23, 1729, at which time we find that he made the following memorandum: "I altered my meridian telescope by placing " the tube exactly in the middle from each end of the axis, whereas it had been before 4,45 " inches nearer the east end than the west." To the middle of the longer arm of his axis Roëmer attached a cord, which passing over a pulley, and being stretched by a weight[z], remedied in some measure the flexure which otherwise might be produced by the telescope. For Halley's transit, a different contrivance had been adopted: four bars sprung from each extremity of the axis, two of which were fixed obliquely on each side of the tube of the telescope, and the others to a metal frame which was placed above and below the telescope; so that instead of the continuous surfaces of two cones, which we now have, there were here four sides of a conical superficies set in the four equidistant points of the base. Roëmer had an arm to his axis, which, extending parallel to the tube, enabled him to direct the telescope without touching any part of the apparatus. Halley's instrument seems to have had no provision of this kind, and yet it required some such precaution, for Bradley found that it was liable to be thrown out of adjustment by being touched although with the greatest[a] care, and that the same effect seemed likewise to be produced even by the heat of the body when any one approached the bars.

Halley, in 1725, obtained the valuable 8 feet quadrant which was made for the observatory by Graham. The radius of Flamsteed's mural arc was 6f. 7$\frac{1}{2}$ inch.[b] and the pier for it may have been built in a room proportioned to these dimensions. This might be the cause why the ceiling was too low for the later instrument to work without injury. It is to be regretted that an instrument, the finest of its kind which has been ever executed, should not have had more accommodation, especially when we consider the constant use which Halley made of it. He has been unjustly blamed[c] for having confined himself so much to it in his observations of the moon: and it has been attributed to his advancing age that he could not move quickly from one instrument to the other to take the transits, as well as the zenith distances. Hearne[d], indeed, speaks of him even in 1721 as being "somewhat lame," which would have increased the difficulty, but the value of each instrument for its respective observations was not then understood. It was not till a much later date that astronomers became convinced of the impossibility, which counteracted their endeavours to keep every part of their quadrants truly in the plane of the meridian, and it is not therefore extraordinary that he made comparatively little use of his transit, which was by far the inferior instrument. But even if there had been no extraneous difficulty, Halley had only one vertical wire in each of his telescopes, and it was therefore impossible for the meridian observations to be made with both, as he never had a regular assistant. In this respect Bradley made a different arrangement, and as soon as he went to Greenwich, he procured the appointment for John, the son of his eldest brother. It was not merely in the general description of the office that this change was of importance: the character of the first who enters on new duties often gives the direction in which they will afterwards be fulfilled, and in this respect John Bradley was an excellent pattern for his successors. Lalande[e] says, that he was born about 1728; he must therefore have been quite a lad when he entered on his new career; but he had the advantage of the very best instruction, and he executed his great master's plans with unwearied diligence.

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[q] Hist. Cœlestis 1725. Prolegomena, p. 108.

[r] Lalande Ast. §. 2388.

[s] It is still preserved at Greenwich.

[t] P. 382.

[u] Preface to Bradley's Greenwich Observations, p. ii.

[x] Horrebovii Opera, tom. III. p. 49. §. 101.

[y] See p. xxiv.

[z] Horreb. p. 51. §. 108.

[a] P. 383.

[b] Flamsteed, Hist. Cœl. Prol. p. 108.

[c] Monthly Review, 1786. vol. LXXIV. p. 185.

Bradley's first care was for the repair of his instruments. Graham did every thing which was necessary to his own quadrant, and several additions and improvements were made by Sisson[f] to the transit. The tube was balanced; a proper[g] apparatus was applied for illuminating the wires in every position; a new level was made for adjusting the axis, so as to make it truly horizontal; and, what was of the greatest importance, two vertical wires were added 15′ of a degree on each[h] side of the centre. There were three in the Wansted transit, and there are a few instances of the time's being taken at each of them; but this method of multiplying the observations does not appear to have been the principal advantage which was at first contemplated[i]. Bradley expressly says that the side wires were added “ [k]to be made use of for taking the transit of objects, in case clouds, “ &c. should hinder me doing it at the middle wire,” and trusting to the accuracy of the intervals, we find that he very frequently contented himself with a single observation, and that not always at the centre.

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[d] N°. 91. p. 154.

[e] Bibliographie Astronomique, p. 538; Lalande says, that he continued at Greenwich for fifteen years; but probably not so long, as he went to sea with Capt. Campbell, who obtained for him in 1767 the appointment of second mathematical master of the Royal Naval Academy at Portsmouth, with which he was allowed to hold the office of purser to a ship in ordinary. In this situation he continued till he died, in 1794, when he was succeeded by his son James, an able mathematician, who now resides at Portsea, and takes private pupils whom he prepares for the navy. John Bradley published the observation of an occultation of Venus by the moon (Phil. Trans. vol. XLVII. p. 201), which in the Index to the Phil. Trans., and in all lists of Dr. Bradley's writings, is by mistake attributed to him. He was sent by the Board of Longitude to the Lizard, in 1769, to observe the transit of Venus, &c. an account of which is given by Dr. Maskelyne in the preface to the Nautical Almanac for 1771; but no idea can be formed of his useful labours without an inspection of his uncle's papers, in which his progress may be traced from the unformed schoolboy to the indefatigable observer and industrious calculator.

[f] P. 382.

[g] Those who recollect the Greenwich transit in the time of Dr. Maskelyne, may remember the handle which was attached to the eastern pier, with which the lamp was elevated or depressed, so as to throw the proper quantity of light into the tube of the telescope. It is impossible not to recognise this very arrangement in the plan which Bradley recommended for the same purpose to Lord Macclesfield (see P. 420): it is most probable therefore that he used it from the time of his first going to Greenwich.

[h] In the observation books there is the following entry, 1744, Aug. 17; "It appears by "taking a mean of the transits of several stars

When all these mechanical repairs and improvements were completed, Bradley applied his first attention to the adjustments of every thing for the work which he had in hand, and he seems to have given up nearly the whole of the latter part of 1742 to this purpose. The entries in the regular observation-books begin with the following year; but the important work which was previously undertaken is still preserved on separate sheets, which enable us to trace out many interesting particulars.

The first transit observation was made on the 25th of July, and there were about 1500 entered between that time and the end of the year. On the 31st of July “ the “ instrument was set by the mark made by Dr. Halley on the park wall;” and by the observations of Capella above and below the pole in August, the error was estimated which there might be in the position of the mark: on the 3d of September the clock was fixed firmly against the brick wall, and its dial was made to face the N. W.: October 11th, Graham added a gridiron pendulum to it[l]: Dec. 4, there is an entry of “ Returned from Oxford;” and as all the preceding observations from Oct. 16, are entered in John Bradley's hand, it appears that by that time he had acquired sufficient knowledge of his business to be left in charge of the observatory while his uncle was called upon to be absent at the university.

The first observation with the quadrant was made on the 15th of June, and there are about twenty before the state of the brass cylinder and the fractures of the screws were discovered[m]. This took place in the beginning of July, and by an examination of the instrument on the 6th, Bradley found that it gave the zenith distances too small by 34″½, that being the quantity by which he considered its position to have been altered [n]since it was adjusted by Mr. Graham and himself in July 1726. He goes on to

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“ lately observed, that a star on the equator “ passes from the first to the middle wire in “ 59″,668, and from the middle to the last in “ 60, 286; the angle subtended between the 1st “ and 2d being 14′ 55″; and between the 2d “ and 3d 15′ 4″¼. By the transit of the pole-“ star taken in Dec. 1742, those angles were “ found to be 14′ 55″¾ and 15′ 1′.”

[i] Roëmer had ten silken threads set vertically in his transit, or rather five pairs of which the two lines in each were very near to each other. He seldom used any but the three nearest the centre, and for the same purposes as Bradley. The interval between each of his pairs was 24″ of time for an object on the equator. Horrebovii Opera, vol. III. p. 53. §. 117, 8. p. 158. §. 379.

[k] P. 382.

[l] Graham put a new and probably a compensation pendulum to the quadrant clock in the month of August, 1744.

[m] P. xlix.

[n] The following memorandum occurs in one of Bradley's books; “ 1726, Sept. 1, I went to “ Greenwich with Mr. Graham, and I adjusted “ the meridian telescope at right angles to the “ axis. Upon trial we found it varied from the “ truth about 1½ diameter of the thread, which “ amounted to not more than ⅛ of a minute.” . . “ We likewise set the mural instrument.” There are repeated notices of Bradley's attention, at other times, to the quadrant, but this is the only mention which has been met with, of his examining the transit for Halley.

mention an examination which he had made in July 1734, and another also which he had made with Machin and Graham in July 1735; from all which he concluded, that if the alteration was pretty regular, about 2″ per annum might be allowed for the variations from the time when it was first adjusted to the year 1742. On the 29th of August of that year Graham had the telescope taken to London, when he removed the vernier a little further from the centre, because in hot weather it was found hardly to reach the line of outer divisions, and he added two vertical wires at the same distance from the centre with those which had been inserted in the transit. The observations were regularly resumed on Sept. the 12th, and on that day we find the following memorandum: "By "comparing the zenith distances taken this day with those of Aug. 29th, it appears "that the line of collimation, as now rectified, corresponds well with what it was before "any alteration was made to the telescope. Hence we may conclude that the line of "collimation was not sensibly changed from its first rectification by Mr. Graham in 1726, "during the following sixteen years, and therefore in the later observations of Dr. Hal-"ley, the error of the zenith distances was about 30″, and about the year 1734 only 15″, "to be added to the observed zenith distances."

By noting the passages of different stars with both instruments, great pains were taken to detect and correct the deviations of the plane of the quadrant from the meridian; these are tabulated, and the general results may be seen in the note, P. 383 of this volume: above 800 more observations were made before the end of the year.

In addition to all this preparatory care, we find another precaution which Bradley took upon this occasion; for there are two large sheets on which he has written, "Ob-"servations made by lord Macclesfield at Shirburn Castle;" these are written out in his lordship's own hand, and extend from the 10th of September to the end of the year. There is also an additional leaf containing "Observations made at Shirburn in March 1743." The transits are taken at each of three vertical wires, the zenith distances at both the outer and inner divisions of the quadrant; and from the dates of them there can be little doubt of their having been obtained by Bradley, as a test to which he might refer his own observations at Greenwich.

CHAP. VII.

Greenwich observations from 1743 to 1749—Pendulum experiments at Greenwich—Comets of 1743, 1744, and 1748.

EVERY possible preparation having been carefully made in training his assistant, as well as in the examination and adjustment of his instruments, Bradley applied himself with all his best powers to the discharge of his duties. And the work of the first year alone is hardly to be credited. The transit observations occupy 177 folio pages: no less than 255 were taken on the 8th of August; and on more than one day the number exceeds 200. Most of these were only at one, and that not always the centre wire; but still the constant alterations in the direction of the instrument greatly increased the trouble, and makes these observations much more than equivalent in labour to the same number, if they had been severally taken at each of the wires for a smaller quantity of stars. The quadrant observations for this same year fill 148 pages, and these possibly exhibit the greater effort; the time was generally taken at this instrument for the star's passage as well as its altitude; the arc was usually read off only on the outer divisions of 96, but it was reduced to the sexagesimal degrees, minutes, &c.; still they were very numerous, and even on the 8th of August, which has been just mentioned, they did not amount to less than 181. The pages are each ruled for sixty entries, but sometimes they are in part taken up by the notices of examinations of the line of collimation, &c. &c. If, however, fifty-five observations are assumed as the average contents of each page, (and this does not appear by any means too large,) it will be found that nearly 18,000 were made by Bradley and his nephew in 1743.

Mr. Wollaston says[o] " it is certain, from papers I have seen, that Dr. Bradley had " the whole British Catalogue calculated to the year 1744; and there are traces therein " of his having examined almost every star in it. Indeed I have been well informed " that Dr. Bradley observed the British Catalogue twice through; first with the old " instruments of the Royal Observatory, previous to 1750; and afterwards with the " new ones." This catalogue, reduced to 1744, is probably the same which is now at Greenwich, and which may have escaped notice from its having, by mistake, been described as only containing " the southern stars[p]." If so, lord Macclesfield had most probably a share (and possibly a considerable share) in this useful work[q], for there is now at Oxford an exact copy of the same catalogue, which formerly belonged to his lordship's observatory at Shirburn Castle.

Flamsteed's catalogue appears without doubt to have been the basis on which Bradley worked. In the observation-book we find for example the following memorandum, 1745, Sept. 9, " From this place dotted in the catalogue under the column of the right " ascensions;" and in other parts of both the transit and quadrant books, there very frequently occur " Dotted off", " Dotted off thus far;" and there is a paper book on which is written, " About June 1st, 1746, a new semicircle was put to the transit in- " strument;—this book contains the stars that have been observed since the above arc " has been put up, that are not in Mr. Flamsteed's catalogue." This new arc was probably of a larger radius than that which had been originally attached to the instrument, so as to enable them to mark the approximate zenith distances of undescribed stars. It appears, therefore, that Mr. Wollaston, though he seems to have imagined that he had formed a high estimate of Bradley's labours, in fact was giving a description which was much short of the truth.

In the transit observations the passage of the wires was generally taken to the nearest second: where there was a perceptible interval, it was most commonly marked + or — ; and sometimes the $\frac{1}{2}$ or $\frac{1}{3}$ of a second is estimated. Dr. Maskelyne, in the earliest of his Greenwich observations, introduced the division of the second into eight parts, and in Sept. 1772 he first used the decimal notation which is now universally adopted.

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[o] Specimen of a general Astronomical Catalogue, preface, p. xi.

[p] Cat. of MSS. in the library of the R. Obs. Greenwich, p. 3.

[q] It is not known by whom the reductions were made, and the catalogues transcribed; but in the Savilian library at Oxford there is a very interesting document. It is the right ascensions and declinations of all the stars in the British Catalogue, as they stand in the first edition of the Historia Cœlestis, the R. A. having been reduced to time—the whole written out by the joint labour of Bradley and of his noble friend.

Bradley had considerable obstacles at first to overcome in this respect: his wires were thick compared with what have been subsequently introduced; his instrument of course was not achromatic, and its power was small. On the 1st Aug. 1743, we find the following memorandum: "This day I put a shorter eyeglass into the transit telescope, its focus is about 1½ inch, so that the telescope magnifies now about forty times." Every thing likewise which is new requires time before it can be used to the greatest advantage, and Bradley was the first who introduced the method of observing by "[r]noting the proportional distance of the star from the wire at the two beats immediately preceding and following the transit across the wire."

He likewise made improvements in the method of observing with the quadrant. Halley's well-known maxim was, "Secure your minute." The divisions on the vernier for the outer arc of 96 were equivalent to 13″,18, and he sometimes read off by estimate to half this quantity; his observations were therefore not to be depended upon as nearer to the truth than about 7″. In the same manner Bradley at first only read off to the nearest of these divisions, annexing +, −, and sometimes ½ to them; but July 18, 1745, a new micrometer screw was applied to the instrument, "the threads of which," he says, "are much finer than those of the former, viz. 39¼ threads in one inch.........so that one revolution of it will alter the inclination of the telescope 53 seconds." After this, a fourth column was added to the other three, in which the quantities for the outer arc were recorded, and we have the observations taken to the seconds as near as they were before to the divisions of the vernier. About the middle of August in the same year, he "fitted a shorter eyeglass into the quadrant telescope, (viz. one of two inches focus, the old one being about one inch longer,) and likewise put in a smaller horizontal wire, that is only $\frac{1}{440}$th part of an inch in diameter, or subtending an angle of 5″ only; the former, that was of the same size with the perpendicular wires, seeming too gross for small stars." But the greatest difficulty seems to have been from the limb. The pains have been mentioned[s] which he took from the beginning to reduce it to the plane of the meridian; but in 1746 a new obstacle occurred. For about that time he made the following memorandum: "When the telescope is moved about the centre of the quadrant, it appears that the circles or arcs on which the divisions are made are not every where at the same distance from the centre of the cylinder upon which the telescope turns; for if a small piece of brass be fixed to the nonius plate, and a point or stroke, made upon it, be brought to correspond with any of the concentric circles, (as, for example, that wherein the points for the outer division are,) it is found that the arc near the beginning of the divisions or the 0 point seems equidistant from the centre of the arc at the end of the divisions, but the arc of 45°, or thereabouts, appears sensibly farther from the centre. This makes it probable that the defect of 15″¾ in the whole arc of the quadrant is occasioned by the cylinder's being placed (at least at present) about $\frac{1}{191}$ part of an inch from the true centre of the arcs, in a line passing through it at 45° on the arch; for such a position of the cylinder will occasion such a defect in the quadrantal arc as was found in Sept. 1745." The examination of the instrument in this respect was repeated in Feb. 1747; and he was then induced to believe that the excentricity did not exceed $\frac{1}{400}$ of an inch. He had found that the length of the quadrantal arc had for several years been gradually diminishing, and this, combined with the circumstance now mentioned, seems evidently to have proceeded from the same alteration in the figure of the instrument. All this was a great check to his exertions; we no longer find those efforts of assiduity with which he began his career. He had found that his instruments were inadequate to the accuracy which he was desirous of giving to his observations, and which he succeeded in attaining when the proper means were afterwards furnished for him. This, however, was not completed before 1750; and in the interval there were several other objects, of no small importance, which occupied Bradley's attention.

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[r] Maskelyne's Observations vol. III. p. 339.

[s] P. liii.

The astronomical labours of 1743 would have afforded sufficient occupation to any common man; but notwithstanding all that he did in the observatory, Bradley found time, in this same year, to begin a course of experiments on the length of the seconds' pendulum. The subject was not new to him; before he drew up the account of Mr.[t] Campbell's observations in Jamaica, he had paid considerable attention to it. There is a paper book, on the first page of which we find, " 1719. Observations of the vibrations " of the great ball, weighing 19lb. 6¼oz., being in diameter 4,22 inches, hanging by a " wire and axis weighing 5oz. 14¼dwts. The distance of the point of suspension from the " surface of the ball 73,73 inches." There are papers also on the same subject in Pound's handwriting, which indicate, that, like other scientific labours, it was pursued by them in common. Their apparatus was similar to that which was used by the earlier French[u] philosophers, and which is still employed by[x] those of the present day. The experiments were most probably carried on at Wansted; and it appears, from the papers just alluded to, that the length of the pendulum swinging seconds at that place was found to be between 39,14 and 39,168 inches. We know nothing of the standard by which these measures were taken, or the temperature at which they were determined, and they are therefore only valuable as the first approximations by which Bradley's mind became familiarized to the inquiry.

On the 13th of Sept. 1743, he returned to the subject at Greenwich, with an apparatus constructed on the same principle as before, but finished in a more complete manner for him by Graham. The experiments were continued and repeated till the 13th of Oct. about which time he went[y] to Oxford: they were resumed at Christmas; and again, after he had spent his spring in the university, in July 1744; but the results which he seems most to have depended on were those which he arrived at in 1745 and 1749. The details of each experiment are preserved amongst his papers, but it seemed to be sufficient to print the synopsis, which will be found at P. 384 of the present collection. Capt. Kater's elegant application of Huygen's theorem has superseded all other methods of resolving the problem, and it would be impossible to recompute Bradley's results. We do not know his estimates for the general effects of temperature, &c. and the principal experiments were made with a sphere screwed on to a brass wire[z], which was supported on knife-edges. Now this mass of matter appears from the drawing (Pl. III. at the end of the volume) to have had such a ratio to the whole as must have affected the conclusion: there is however no indication of any allowance having been made for it, nor is there any memorandum of its dimensions, or of its specific gravity. Neither can these particulars be now recovered, for it is not known what became of the apparatus. It is not at Greenwich, nor can any thing of the kind be found in the collection of the Royal Society.

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[t] P. 62.

[u] Mem. de l'Ac. R. des Science 1735.

[x] Base du System. Metrique, vol. III. p. 338. Biot. et Arago Recueil d'observations, p. 441.

[y] P. lii.

[z] This wire was 0,1 inch in diameter: and the rigidity which it gave to the apparatus was a manifest improvement. Prof. Airy has pointed

Bradley found the lengths to be[a] 39,13472 inches at a temperature of 68°, and 39,149 at 32°, which give 39,13710 inches for a temperature of 62°. In this no allowance is made for the resistance of the air; but we know from Bessel's experiments, that we are not yet in full possession of the effects of this element in the calculation.

From a mean of twelve observations in Portland-place, Captain Kater[b] found the length under the same circumstances to be 39,13284. This was by Sir George Shuckburgh's scale, and answers to 39,13147 by that of the Royal Society, which by Bird's[c] data may be considered as equal to Graham's; Bradley's length therefore has an excess of 0,00563. The difference of the stations would not make a variation of $\frac{1}{1000}$ of an inch; but the knife-edges may have more effect, and as they were above the axis of suspension, they would have tended to increase the distance of the centre of oscillation. If an allowance could be made for this circumstance, it would therefore bring Bradley's results still nearer to the most accurate of which we are in possession, and shew how much superior they were to any which had been obtained before Captain Kater's determination of the question. Graham's estimate was 39,126[d]; and Whitehurst was still farther from the truth, for he considered the length of the second's pendulum to be[e] 39,1196 inches.

The finest comet of the eighteenth century was that of 1744[f]: Loÿs de Cheseaux[g] gives a splendid account of it; on the 1st of February, he says that its light surpassed that of all the stars of the first magnitude, even of Sirius; on the 8th it was equal to Jupiter, when in opposition; and on the 18th it even exceeded Venus in size, though not in brilliancy. Its light even increased beyond this, so as to make it visible by daylight. It was seen by Klinkenberg at Haerlem, on the 9th of December 1743[h], but more than three weeks elapsed before any thing was known in England of its appearance. This deficiency in scientific information seems in the present day to be most extraordinary; yet we find Lacaille regretting in a letter[i] which he wrote to Bradley that more than three years had passed without any intelligence having reached Paris of what was going on among astronomers in London. This letter was written in 1748, before the peace was concluded at Aix la Chapelle; and as England was engaged in the contest on the continent as early as 1743, even in the latter part of that year the same obstacles

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out an allowance which must be made for the length of the pendulum, when it is deduced from the vibration of a heavy body attached to a flexible string. Mem. of the Phil. Soc. of Camb. vol. iii. p. 355.

[a] P. 387.

[b] Phil. Trans. 1818, p. 87.

[c] P. 387.

[d] Mem. de l'Ac. Roy. des Sciences, 1735, p. 512.

[e] Attempt towards obtaining invariable measures, 1787, p. 17.

[f] Lalande §. 3209.

[g] Traité de la comète de 1743,4. p. 138.

[h] Pingré Cométographie, vol. II. p. 52.

[i] P. 438.

may have prevented the accounts of observations made abroad from reaching England.

The comet was not discovered in England till the evening of the 23d of Dec. 1743 O.S. It was then seen in London by Dr. Bevis; and the account which he sent of it to Bradley will be found at P. 425. There is a paper in Lord Macclesfield's handwriting, in which he discusses this account, and says, that there was no other way of reconciling it with his own observations, than by "supposing the Doctor mistaken in the stars" with which he compared the comet, and that it was probably ζ Andromedæ which he observed, when he refers to η. His lordship was particularly interested about the circumstance, because the comet was discovered on the same night of 23d of Dec. at Shirburn Castle, and there is a letter which he immediately wrote, probably to Bliss, giving him an account of it. It was written while there was yet some uncertainty; for he says towards the end, "if it prove to be a comet, I will send notice of it to Mr. Graham and "Dr. Bradley." A postscript adds that the weather afterwards cleared up, and left no longer any doubt on the subject, and he communicated all the particulars to Bradley on the following[k] day. It was seen at Greenwich on the 26th[l], and observed from that time till the 17th of Feb. There is indeed an ephemeris of it from Feb. 21 to March 11, calculated by Gael Morris, and probably deduced from elements with which Bradley had supplied him. What these were does not appear; but there is a letter at Shirburn Castle, dated Feb. 12, 1743-4, in which Bliss tells Lord Macclesfield, that "the comet "appeared so very bright last night, equalling the light of Venus, that Dr. Bradley "agrees that it may be seen on the meridian, and being engaged himself has desired "me to request your lordship to try to observe it. The elements which he left at Shirburn appear to our last night's and former observations to give the place true within "2′ of longitude and latitude." That he reduced them afterward to a greater degree of exactness is most probable; for there is a loose sheet, which contains what was evidently intended for the introduction to a paper on the subject, which was to be addressed to Lord Macclesfield. It is as follows: "The comet which appeared lately "with such uncommon lustre, having engaged the attention of most persons, more "particularly of astronomers, its apparent course has no doubt been accurately "observed as well in other parts of the world as in England. Some of the observations "made in foreign parts being of a more early date than any made here, might upon "that account be more likely to enable us to settle its true course; but as those are not "yet transmitted to us, I have made use of the best and earliest of our own, in order to "determine the elements of a parabolic trajectory. The first as well as the last exact "observation of the comet made in England, I had the honour of receiving from your "lordship, as they were taken in the observatory erected by your lordship at Shirburn "Castle. With what great advantage to their science your lordship has built it, the "astronomers of future ages as well as those of the present will judge, when they have "the opportunity of examining your lordship's observations."........It cannot now be ascertained why the design was not executed; but it may have been given up from motives of kindness. Betts was a young man who had just taken his master's degree at

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[k] P. 423. [l] P. 372.

University college, and his anxiety about calculating the orbit of a comet is described in rather an amusing manner by Bliss[m]. Bradley may therefore have been inclined to give him the opportunity of exercising himself in this way, which he so far succeeded in as to obtain his elements sufficiently near to represent eighteen observations of this comet without any error, which exceeded 31″ of longitude or 37″ of latitude[n].

The first comet which appeared in 1748 occupied Bradley's attention, and his observations of it are the more valuable, because Struyck has expressed doubts of Maraldi's calculations respecting it. Pingré[o] indeed has pointed out the errors which might have been occasioned by defects in the solar tables, and they particularly affect the conclusions at the time of the comet's passing between the earth and the sun, but still it is of importance to have some independent observations by which the elements may be examined. The transits have been long before the public[p], and they are now completed by the addition[q] of those for declination[r].

Additions and corrections to the preceding chapters collected from the Journals of the Royal Society.

P. iii. The first notice which has been found of any communication from Bradley to the Royal Society, is in March 1716. On the 6th Halley and Pound observed an Aurora; on the 22d of that month mention is made of it, and of "a letter from Mr. J. Bradley "from Oxford, curiously describing the same, wherein he remarks, that the rays or beams "which ascended towards the zenith, passed on the southward thereof, and seemed all to "tend to the middle between Castor and Pollux, about 25 degrees from the zenith." The "J." was inserted in his name after the minutes had been written, to distinguish him from Richard[s] Bradley, whose name was at that time more familiar to the members of the society. Among Bradley's papers there are several rough notes of the Aurora, but they are all subsequent to that which is here alluded to.

P. iv. Oct. 23, 1718. "Dr. Pound presented the society with several curious astro-"nomical observations lately made by himself and Mr. Bradley, at Wansted in Essex, "viz. of the eclipse of the moon, Aug. 29...." There is no notice of this observation in Pound's observation-book, either in his own or Bradley's hand, but all the particulars of it were published in the Phil. Trans. vol. XXX. p. 855. The other observations, of which mention is made, are the appulse of Aldebaran to the moon, (see P. 342,) "this "being the first observation of the application of the moon to a fixed star in the day-

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[m] P. 426.

[n] Phil. Trans. vol. XLIII. p. 97.

[o] Cométographie, vol. II. p. 59.

[p] Bradley's Greenwich Observations, vol. II. p. 425.

[q] P. 375.

[r] At P. 444, there is a letter of Bonaventura Suarez from Paraguay, giving some observations of this comet of 1748. In the same year there was a remarkable eclipse of the sun. At P. 447, there is an account which Ferguson sent to Bradley of the observations made by Mr. Irvine at Elgin, where it was annular. He records a curious circumstance, for "before "the joining of the cusps of the sun, and at "the breaking of the annulus, a tremulous mo-"tion was observed, and irregular bright spots "of the sun 'twixt the cusps." An appearance, very similar to this, was observed in the eclipse of Sept. 1820. See Mem. of the Ast. Society, vol. I. p. 142.

[s] See P. vii. note [x].

" time," and the conjunctions of Jupiter with Venus and Cor Leonis; both of which are printed in P. 343 of the present volume.

P. iv. Nov. 16, 1721. " Dr. Halley produced a paper of observations he had lately " received from Mr. James Bradley, made in October last, being some very curious " observations on the planet Mars," from which he found the sun's parallax rather less than 10″. It is added, that " the same has since made others not less curious, which he " will be pleased to communicate in time."

P. v. July 2, 1719. A paper of Mr. Bradley was read, being " an account of certain irregularities observed in the motions of Jupiter's satellites, in part accounted for " by the new tables lately computed and corrected from the observations made by the " Rev. Mr. Pound and himself at Wansted." From the particulars mentioned in the minutes, it is perfectly clear that this paper must have been identical with the remarks inserted in Halley's tables, and reprinted at P. 81 of the present volume.

P. vi. Oct. 23, 1718. " Dr. Halley proposed Mr. James Bradley, nephew to Dr. " Pound, as a person very well qualified to be a member of this society, which was " referred to the next council." At a meeting of the council on the 6th of November, Sir Isaac Newton being present as President, this recommendation was approved, and on the same day Bradley was in consequence elected a fellow. It happened that Saunderson, who was afterwards Lucasian professor at Cambridge, was elected at the same time.

P. ix. No entry was found of the time when the Huygenian glass was lent to Pound, but it is referred to (April 6, 1721,) as if the society had engaged him to make use of it, rather than that he had made application, and borrowed it from them. For in speaking of Hadley's reflector, it is said that " several of these observations were never made before " in England, until the society set up their long telescope of above 120 feet at Wansted." There is no observation made with it by Bradley after Pound's death in 1724, although he kept it by him for three or four years, as we find by the following minute: June 20, 1728. " The Rev. Mr. Bradley, Savilian professor of astronomy at Oxford, delivered " to the society the glass and old furniture of Mr. Huygens's large telescope, which had " been reposited for some years in the hands of his uncle, the late Mr. Pound, for making celestial observations. At the same time he acquainted the society, that there being " no conveniency for his using it since the pole upon which the glass was erected has " been broken, he thought fit to return it into the hands of the society, and withal " desired the society to accept of such new additions and improvements which his uncle " had made to the furniture and apparatus, whilst he was using it; viz. a curious " micrometer contrived and made by Mr. Graham, a new eyeglass, a new director to " the sight, and a new tin tube to carry the object-glass."

P. x. Jan. 7, 1725. " The Rev. Mr. Burnett sent the society, by the hands of Mr. Professor Bradley, a present of the glasses belonging to the longest of all Mr. Huygens's " telescopes, being that of 210 feet. The object-glass of this telescope with that of another " for 170 feet (since bought by Sir I. Newton) are mentioned by Mr. Huygens in his " Cosmotheoros, with a particular encomium, as being not only the largest of the kind, " but also the most perfect as to the exactness of figure, of any that had ever yet been " contrived. And as such they were presented to his brother Constantine Huygens, " secretary to king William, of whose executors Mr. Burnett purchased this, which was

" now presented to the society, for the sum of 100*l*." Huygens's name is written with a diamond on the glass, with the date of July 23, 1686, which refers to the manufacture of the glass, and not to the time at which it was given to the society; it having been presented in 1725 by a friend of Bradley, is still more strongly in favour of its being the same glass which was used by him in 1722. The focal length of 210 feet is probably according to the Dutch standard, which is longer than ours, and may account for Bradley's calling it 212¼.

P. xxxiii. The time, within which the cause of aberration occurred to Bradley, is still further narrowed by the following extracts from the minutes of 1728, Nov. 14: " Dr. " Halley took occasion to speak concerning the late improvements in astronomy made " from the new discovery of an annual motion of the fixed stars." He gives an account of what had been done at Kew, and then adds, that " his colleague, the Rev. Mr. Brad- " ley, resolved to fix up another and more accurate instrument......and after fifteen " months' almost daily observations on fifteen different stars, has at length discovered " not only the laws of the motions, but also the true and manifest cause of them." The instrument having been set up at Wansted on the 17th Aug. 1727, the fifteen months would only have been expiring, and from the expressions which are used, it is clear that the completion of the discovery was then quite recent. Halley, at the end of his report, " desired that a proper notice might be taken of this new discovery of Mr. Bradley, to " prevent any other person from laying claim to it before he had sufficient time to pre- " pare and adjust his observations and reflexions on this subject for the public." After the communication was finished, Halley concludes his report by saying, that Bradley was sufficiently convinced of his having discovered the true cause of the phenomena, since he was " able to foretell at any time, the situation of a star being given, how much " the variation of it will amount unto, and that with so much exactness, that there does " not remain any sensible part unaccounted for, which can be supposed to arise from " parallax.—The President proposed that thanks might be returned to Mr. Bradley for " the great care and pains which he has taken in his application to this subject, and " likewise that it would be proper to advise Mr. Bradley, and hasten him, to the publi- " cation of his thoughts, as soon as conveniently may be."

P. xxxiii. note [u]. It appears that in 1727, and several of the following years, Sir Thomas Dereham was in Italy, and in constant correspondence with the Royal Society. Jan. 29, 1730, we find mention of a letter to him from Eust. Manfredi, dated at Bologna, Oct. 12, 1729, in which great approbation is expressed of " the theory of Mr. Bradley, " for explaining the new discovered motion of the fixed stars." He speaks likewise of his transit observations, which he designed to examine at his first leisure, and compare with the theory of aberration. But May 7, 1730, a letter was read from Mr. Atwell, dated Rome, April 14, 1730, in which he describes the manner in which the doctrine had been received in Italy, and says, " they do not at present seem ready to give into " it, and Signor Manfredi of Bologna is said to be writing an answer to it." Atwell then states how he had exerted himself for its vindication; that he had sent an explanation to Manfredi through cardinal Doria, which had (as he heard) had the effect of removing part of the objections; but what the difficulties were which still remained he had not been informed. The earth's motion in its orbit was so closely connected with the theory of aberration, that ecclesiastical prejudices unfortunately threw impediments in the way of its admission among rigid Roman catholics, and more especially in the papal dominions.

P. xliii. Feb. 25, 1742. " Mr. Bradley said that the first account he had of this " comet was from an observation made on Monday the 20th of this month, at Melk- " sham in Wiltshire, since which he has found it, and made observations on its distance " from the stars."

⁂ The first seven chapters of these Memoirs were drawn up and sent to the press at a time when academical business made it inconvenient to be absent from Oxford. If any thing occurred which was likely to admit of elucidation from the records of the Royal Society, application for that purpose was made by letter, which was always attended to in a very obliging manner by the assistant secretary. The work, however, having advanced so far that more was expected to be found than at an earlier period, I took the opportunity myself of referring to the Journals in the summer vacation of 1831. As they were more full than I had by any means anticipated, I thought it right to look through the whole, which belonged to the time of Bradley's life when there was any probability of his being mentioned. Besides the particulars for which I had been more particularly in search, the additions which are here inserted were in consequence collected. It is to be regretted that there was occasion found for correction also: but the date sent with the account of the long Huygenian object-glass was connected by a mistake with the gift of it, (P. x.) when it was found that it probably belongs to the time of its being executed, as will be seen in P. lxi. It is to be regretted also that these earlier notices were not collected in time to be distributed in the several places to which they belong; but the journals and minutes of the Royal Society from 1714 to 1762 (the period during which the examination was to be made) extend over more than sixteen folio volumes; and to look through all these for my particular object was a task to be undertaken by no one but myself. S. P. R.

CHAP. VIII.

Nutation—History of its discovery—Quantity of it—Reduction of the Wansted observations—Copley medal assigned to Bradley—His connection with different scientific bodies.

IN September 1747[t], Bradley completed the series of observations, which established his second great discovery, the nutation of the earth's axis. Like Halley, he required a whole revolution of the moon's node to do all that was necessary for completing the object which he had in view, and no small debt of gratitude is due from society to those, who will dedicate the labour of nearly twenty years to the investigation of important truths. If Halley's advanced age makes us think with greater admiration of his energy, it was no less fortunate for the cause of science that Bradley's life was spared, as well as his, for the full attainment of so great an undertaking.

In stating the result of the first year's observations at Kew on γ Draconis, Bradley says[u], that " it arrived in December to the same situation it was in at that time twelve " months, allowing for the difference of declination on account of the precession of the " equinox." A minute excess at that time might have been attributed to error of observation, but when he repeated his observations, and increased at Wansted the number of stars to which they were extended, he found that this conclusion required some modification. For "[x]the stars near the equinoctial colure changed their declination 1½″ or 2″ in a year more than they would do if the precession was only "50″[y]," while those "near the solstitial colure......altered their declination less than "they ought, if the precession" was precisely of that quantity. At first it appeared to him that the variations in the phenomena might not be produced by the same cause[z], and to determine the effects of aberration, he was obliged to content himself with making allowance, according to the annual difference in excess or defect, which his observations had detected in the motions of each particular star. The quantity was small, but that was not a reason[a] for his neglecting it, and having satisfied himself of the accuracy[b] of his instrument, and settled the laws of aberration, he continued "his observations "of the same stars, hoping that, by a regular and longer series of them, he might at "length be enabled to discover the true cause of such apparent inconsistencies."

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[t] P. 284.

[u] P. 3.

Bradley continued to reside chiefly at Wansted till May 1732[c], during which time he found the effects accumulate. The following is a copy of a memorandum found on a loose paper, and which from the dates on it was probably made about this time:

"4 9,5
4 23,7

14,2 Capella has gone north in 4 years.
21,6 by the common precession it ought to move.

7,4 Capella has gone south in 4 years.

1 33,3 γ Draconis in 1731.
2 3,3 γ Draconis in 1727.

4,0 γ Drac. gone 4″ north in 4 years.
3,3 precession ought to be 3″,3 south.

7,3 γ Draconis gone north in 4 years.

4 30,3 β Draconis in 1727.
4 24,3 β Draconis in 1731.

6,0 β Draconis went south in 4 years.
12,¼ the precession south.

6,¼ β Draconis gone north in 4 years."

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[x] P. 10.

[y] For calculating his first tables of nutation, he assumed the mean quantity of precession to be 1° in seventy-one years and a half, (P. 28.) or 50″,3 in each year; and a loose paper has been found, dated May 1749, on which he says, "the motion of the precession is not precisely "known, but is undoubtedly more than 50″ per "ann., and yet not so much as some moderns "as well as former astronomers have made "it, who reckon it after the rate of 51″." He then considered it as amounting to 1° 24′ in 100 years, or 50,″4 annually.

[z] P. 10.

[a] P. 18.

[b] P. 22, 19.

[c] P. 22.

A vague idea of nutation was familiar to the minds of astronomers. Newton had even shewn that there must be a very small semi-annual nutation depending on the sun; Flamsteed[d] refers to such an effect, and in 1693 Roëmer had detected a certain variation in the declination of the stars which could not be accounted for either by refraction or parallax, and which he says, "[e]sine dubio ad vacillationem aliquam "poli terrestris referendam, cujus me verisimilem dare posse theoriam observationibus "munitam spero;" it was one of the first causes which occurred to Molyneux and Bradley[f] for accounting for the phenomena, which afterwards were found to be produced by aberration; and the same steady appeal to facts which proved to Bradley that it was not[g] the cause of the greater alteration of the apparent places of the stars, now proved to him that it did operate to produce the variation in it. He might have been influenced in extending the arc of his instrument so as to take in Capella, not only by the magnitude of the star, but likewise by its situation in the opposite part of the heavens to γ and β Draconis. We find him accordingly on the preceding paper contrasting the variations which he found in the alteration of their places; but this, after all, does not appear to have been sufficiently precise for him; he therefore again compared the 35th Camelop. with γ Draconis, and found that the direction in the difference of their motions was perfectly consistent with a nutation of the earth's axis. The cause which produced this nutation did not escape him. In 1727[h], the moon's ascending node was near the beginning of Aries, and consequently her orbit was as much inclined to the equator as it could be. At that time [i]the stars near the equinoctial colure changed their declination $1\frac{1}{2}''$ or $2''$ in a year more than they would have done, if the precession was only $50''$. But when in 1732 the motion of the node had carried it back towards the beginning of Capricorn, [k]the phenomena were no longer the same, and as the inclination of the plane of the moon's orbit with respect to the equator was at a mean, the stars near the equinoctial colure now changed their declination no more than the mean precession required, while some of those near the solstitial colure altered theirs above $2''$ in a year less than they ought. The quantities as well as the directions of this motion during the intermediate time all agreed with the effects of the same cause, and shewed that in consequence of the moon's action on the spheroidal figure of the earth, the obliquity of the ecliptic would sometimes exceed, and sometimes fall short of its mean quantity, [l]according to the various situations of the nodes of the moon's orbit. It will be recollected that γ Draconis is near the solstitial colure, and this position, combined with that of the moon's node at the time of Bradley's beginning his observations has been cited[m] among the circumstances which tended to facilitate his discoveries; but it may be justly doubted whether this was really so. It made the effects of nutation more apparent, but exactly in the same proportion it increased the difficulty of determining the true quantity of aberration.

At the end of nine years, he naturally looked forward to such an analogy for the remaining half of the revolution of the moon's node as would confirm his hypothesis, which encouraged him to continue his observations. He simply [n]mentions this period, but there is more connected with the epoch than would be suspected from barely reading his paper on nutation. From his cautious and retiring habits, it is very probable that he had no intention of giving publicity to his discovery till the whole eighteen years of observation had perfectly established the truth concerning it; but Maupertuis, when he returned from Lapland, consulted him on the quantities which had been allowed for aberration in reducing his observations, and requested Bradley to inform him whether he had himself observed δ and α Draconis[o]. Neither of these stars was within the range of the Wansted sector; but he was too liberal to meet the inquiry with a repulsive answer; and we find him therefore consulting [p]his friend whether it would not be right in him to apprise the French astronomers of the other irregularities, which he had "observed in the situation of the earth's axis to the plane of the ecliptic, and of the "unequal precession of the equinoctial points." Now this was in 1737, ten years after the sector was erected at Wansted, and in his letter to Maupertuis he[q] accordingly communicated a clear outline of what he had observed, with the results of his half period, and stated the confirmation, which they had derived from the observations of an additional year. Maupertuis acknowledged[r] the information which he had received on this point, and probably was the first to give a public notification of this important discovery. Le Monnier alludes also to it[s] in a memoir on the height of the pole at Paris: to this there could be no objection: but in another respect he was guilty of a very unfair return to Bradley's candour. In the letter to Maupertuis[t] there was an invitation to use the sector, which Graham constructed for him, in making observations on the motions of the fixed stars. Le Monnier undertook it, and found them conformable to what Bradley's observations had determined. These results he sent to Greenwich[u]; and if he had done nothing more, all would have been well; but, too eager in putting himself forward, he gave, in 1745, [x]his own account of the doctrine of nutation, and of the part which he had taken in settling the question. This was not handsome, after the freedom[y] of communication which he had enjoyed, and Bradley, when he had made so fine a discovery, and had pursued the inquiry for so many years, ought to have been allowed in common courtesy (to say nothing of justice) the privilege of being the first to publish the details of it to the world[z]. His name, however, was too high to be affected by such a circumstance, and he probably paid little attention to it[a]. He continued his observations as long as he thought right, and when they were completed he drew up the account of their results in a letter to the Earl of Macclesfield, which was read before the Royal Society on the 7th and 14th of January 1748, and printed in the XLVth vol. of the Phil. Trans.

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[d] Hist. Cœl. prol. p. 113. Wallisii Opera, vol. III. p. 706.

[e] Horrebovii Opera, vol. III. p. 65.

[f] P. 3. [g] P. 4. [h] P. 23.

[i] P. 10. [k] P. 24. [l] P. 23.

[m] Delambre, Hist. de l'Astr. au xviii. siècle, p. 419.

[n] P. 25.

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[o] P. 404—406.

[p] P. 408.

[q] P. 409.

[r] Mem. de l'Ac. R. des Sciences, 1737, p. 411. It was probably this circumstance which led De Fouchy to speak inaccurately of Bradley's publishing his discovery in 1737. (Hist. de l'Ac. 1762, p. 238.

[s] Ibid. 1738, p. 220.

[t] P. 409, 410.

[u] Mem. de l'Ac. R. des Sciences, 1745, p. 522.

[x] Ibid. p. 512.

[y] See P. xliv. 434.

[z] The view which La Caille (see P. 440) took of this was exactly what might have been expected from a man of right feelings. Le Monnier was ready to accuse La Caille of plagiarism for having introduced into his tables an equation for the unequal precession of the equinoctial points; (Delambre, Hist. de l'Ast. xviii siècle p. 209;) but La Caille himself, instead of affecting to take up and develope Bradley's ideas, honestly applies to him for the information which he wanted on the subject. See also de l'Isle's letter, p. 434.

[a] See P. 473.

[b] Machin suggested, in the course of the inquiry, that tables representing the several effects might be calculated, by supposing that the moon's attraction caused the pole of the earth to describe a small circle around its mean place; and to determine the magnitude of this circle was one of the great objects of Bradley's inquiry. The diameter at first was taken as equal to 18″; but Bradley considered that there would be a better agreement with some of his observations, if the curve[c] was supposed to be elliptical with its major and minor axes equal to 18″ and 16″. He seems at one time to have been inclined to a larger number for his maximum, and there is a paper on which he has set down the results of a number of observations, and at the bottom of it he says,

"Hence γ Draconis gives the greatest nutation...9″,6
"and β gives the greatest ....9″,45
"and the mean of both will be about..............$9''\frac{1}{2}$"

It is to be regretted that he took the round number of 9″, instead of that which these calculations pointed out to him, and which was more accurate. Dr. Maskelyne[d] says, that he framed his tables "from a scrupulous calculation of all Dr. Bradley's observations "of γ Draconis, which he was pleased to lend me;" and from which it followed "that "the nutation of the earth's axis is performed in an ellipse, whose greater axis lying in the "solstitial colure is 19″,1 and lesser lying in the equinoctial colure 14″,2[e]; namely, in "proportion to one another as the cosine of the obliquity of the ecliptic to the cosine of "double the obliquity, as given by theory." But the theory was not completely made out when Bradley wrote. Newton indeed had shewn the principles on which it depended, and Machin's suggestion is described[f] in connection with his having been employed on the theory of gravity; but his artifice was not new in supposing the pole of the earth to revolve about its mean place, and Bradley, though no mean mathematician, found it necessary to invite the attention of those to the problem, who had devoted their attention more especially to the various effects of the power of gravitation. In this he acted wisely, since he confined himself to those exertions in which he was most eminent, and the call, which he made on others for their assistance in promoting the search after truth, was not in vain. Dalembert[g] answered it in 1749: in 1752, a paper on the subject was communicated to the Royal Society by Silvabelle, a translation of which, by Dr. Bevis, was printed in the 48th volume of the Phil. Trans.: considerable doubts seem to have been entertained at the time about the merits of this solution, but we are indebted to it for a dissertation[h] which Thos. Simpson, in consequence, composed on the subject—

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[b] P. 25, 26. [c] P. 36.

[d] Greenwich Obs. vol. I. Explanation and Use of tables, p. viii.

[e] η Ursæ was one of those stars, (P. 36.) the observations of which suggested to Bradley that the variations would be better represented by supposing the pole to move in an ellipse than in a circle. Dr. Maskelyne therefore paid particular attention to it, and says, "from a "like examination of his observations of η Ursæ "Majoris, I found the lesser axis of the ellipsis "of nutation to be 14″,1, or only 0″,1 less than "what it should be from the observations of "γ Draconis. But the result from the observa-"tions of γ Draconis are most to be depended "on." The Bishop of Cloyne, from 378 observations which he made on ten different stars, (1808—1814,) and 1240 more which he made on the same objects, (1818—1820,) found the greatest coefficient of the nutation for the obliquity of the ecliptic to be 9″,25. (Phil. Trans. 1821, p. 347.)

[f] P. 25.

[g] Recherches sur la précession des équinoxes et sur la nutation de l'axe de la terre.

[h] Phil. Trans. vol. L. p. 416. Simpson's Mis-

"qui nullum quod tetigit non ornavit." Euler and many other mathematicians, both foreigners and of our country, have applied themselves to this recondite problem, but the enumeration of their respective labours would lead us too far from our immediate subject.

The quantity having been fixed upon for the radius of nutation, Bradley's next care was to examine the truth of it by very extensive calculations. He seems to have had a double object in the stars which he selected for this purpose from his observation book; and it will be seen by the following arrangement of them, with the right ascensions annexed to each, that they were not only taken from different parts of the heavens, but were so situated as to admit in several instances of being formed into pairs, which consisted of stars nearly in opposition to each other. They were

α Cass. 6°	Capella 74°	35 Camelop. 86°	γ Ursæ 175°
ε Ursæ 191	β Drac. 261	γ Drac. 268	β Cass. 359

To these were added τ Persei 39°, α Persei 46°, and η Ursæ 204°, which do not group so well with one another, but yet were so far apart as to make the identity of direction from nutation very apparent in them. It may be seen[i] that he reduced almost all the observations which he had of these eleven stars, and that when he allowed for the effects of aberration and nutation, the alterations produced by the mean quantity of precession brought their places all very nearly back to what they were at the epochs from which he sat out. These must, to a certain extent, be deficient in the accuracy, which might now be given to similar reductions by the improvements in astronomical tables, and the stricter investigation even of his own discoveries. He calculated his nutation from a circular instead of an elliptical motion in the pole, and the maximum was rather too small. He diminished his maximum[k] of aberration, when he really was nearer the truth in the quantity which he had originally taken. As his stars passed near the zenith, and he did not want their absolute places, he paid no attention to the minute effects of refraction. He likewise made no allowance for a solar nutation: "motus iste "nutandi perexiguus esse debet et vix aut ne vix quidem sensibilis[l]:" it was a quantity too small to be determined, in the first instance, by observation, and its amount was therefore unknown to him. All these circumstances will require some allowance to be made for occasional differences, which they may either create or increase in particular instances, from the mean quantities. Still, however, it was thought right to print the whole[m], not only as a monument of his industry, but to enable any one to see how he arrived at his several conclusions.

In making these reductions it will be seen that he took the distance of the star's right ascension from the place of the moon's ascending node, which he entered in a column which he called "the argument of nutation," and 9″ × sine of this quantity gave him the number which he entered in the column "nutation." The sun's place being found at the time when the particular star had no aberration in declination, the distance of it from the sun's[n] equated place at the time of the observation, gave the quantity which

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cellaneous Tracts, p. 1. Walmesley paper on the precession in the Phil. Trans. 1755, vol. XLIX. p. 700, was transmitted to the Royal Society through Bradley, P. 498.

[i] P. 302—338.

[k] P. 29.

[l] Principia, lib. III. prop. 21.

[m] P. 302—338.

[n] P. 290.

he entered in the column of "argument of aberration:" and to find the aberration for each observation, he multiplied the maximum by the sine[o] of this quantity.

Bradley, like Newton, did not "deal in conjectures," he remembered that no physical causes were to be admitted, unless "et veræ sint et phænomenis explicandis sufficiant." In the same manner therefore as he shewed the truth of his doctrine of aberration by the application of it to particular instances, he gave a number of examples in his paper on nutation[p], in which he shewed the existence of a motion corresponding with that which would be produced by the cause which he suggested. These he took from his observations of γ Draconis, 35 Camel., α Cass., τ and α Persei, and η Ursæ Majoris, and it appears that he calculated them all over again for this purpose. There are two rough copies at Oxford of the paper on nutation[q], but these tables are not inserted in either of them: there are however a number of papers, from which it appears that he varied at different times both the particular observations which he selected of each star, and the degree of precision with which he should give the results. He had calculated the precession, aberration, and nutation, at one time, to hundredth of seconds, to which he had added the data, which are contained in the four last columns of his more extensive reductions; but the size of the page, on which the Philosophical Transactions were then printed, could not have conveniently taken in so much, and he probably thought it useless to give his quantities with this degree of minuteness. From a wish likewise to abridge the extent of what he was printing, he omits the particular considerations of β Drac. and β Cass.; but there was no exaggeration in the account which he gives of the extent and results of these as well as of the rest of his stars; he mentions having compared[r] "above 300" of γ Draconis with his hypothesis, and it will be found that he calculated 314; he mentions "about 250" of β Draconis, and there will be found 247; and in the same manner the facts in each case will strictly bear out his assertions respecting them. There were some irregularities in γ and ε Ursæ Majoris, so that he could not argue from them: he makes no reference also to Capella; for it may be seen that some of the results[s] for this star differ widely from the mean, and its distance from the zenith at the extremity of the range of the sector may have made him suspicious of his observations of it. From the other stars his conclusions come out as near as, under all the circumstances of the case, could possibly have been expected[t]; much nearer certainly than could have been found, but for Bradley's most extraordinary talents for observation[u].

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[o] P. 297.

[p] P. 31—36.

[q] The more generic term of deviation was sometimes used, which was probably adopted by Bradley before he had satisfied himself of the true cause of the motion.—La Caille retained it, P. 457, and Maskelyne used it in a confined sense. Expl. of Tables, p. iii.

[r] P. 31.

[s] It is hardly necessary to point out that 192,6 in the last line of P. 320 is a press error for 162,6.

[t] Lemonnier, who affected to connect himself with Bradley's particular pursuits, had a sector, in the construction of which Graham derived hints from what might have been objected to in that which he made for Wansted (P. xxvii.); he had likewise all the advantages of the information which Bradley communicated to him, and yet he got only to a loose estimate of the maximum of nutation. See Mem. de l'Ac. R. des Sciences 1745, p. 522.

[u] When La Caille went to the Cape of Good Hope, he endeavoured to make arrangements for simultaneous observations to be made in Europe, which, compared with his, might give the means of ascertaining the parallax of the sun

That the moon, in its successive revolutions round the earth, must have a difference in declination produced by the retrogradation of the node, was sufficiently evident; but no one had considered the varying effects, which this would have on the position of the earth's axis till Bradley detected the fact, and explained its cause. He was not, however, aware of the excellence of his own observations, nor did he feel confident that, in a single revolution of the node, they could have brought him so very near the truth as was deduced from them by Dr. Maskelyne's scrupulous examination. He knew, likewise, that he was not in possession of those particulars of the theory by which exact conclusions were to be obtained. He satisfied himself, therefore, with establishing the general truths, and was contented with pointing out the great use[x] which might be made of them, when the exact quantities should be more precisely determined, by which these causes affect the relative situations of particular stars. His hesitation likewise was increased by the motions which he found proper and peculiar to many stars. He mentions the instance of Arcturus[y], and there are many loose papers on which we find him to have worked in comparing the places of this and other fixed stars, as they were determined from the observations of Tycho or Flamsteed, as well as from his own. He notices the difficulty in determining whether this effect may be produced by an actual motion in the stars or in our own system, a subject which was afterwards more fully discussed by Sir W. Herschell[z]; but it is probable that both must be combined, which increases the difficulty of the problem; and such a complication will require a very long series of observations to allow of any determination of the relative quantities of motion, which must be apportioned to each particular body.

As soon as the paper had reached the continent in the summer of 1748, La Caille borrowed[a] Cassini's copy, and made[b] a translation of it, from which he read extracts at a meeting of the Royal Academy of Sciences. These he likewise published in the Journal de Trevoux[c], with an account of the manner in which the effect of nutation was to be calculated. Lalande mentions an oversight which had been committed in drawing up this communication; but the very haste by which it was probably occasioned, shews the zeal with which Bradley's account of his discovery was received by those, who were best able to make a right estimate of it.

When the discovery of aberration had been communicated to the Royal Society, they were not contented with the thanks which were of course voted for it, but in a council held on the 14th of Dec. 1730, "[d]a question was proposed whether a compliment shall

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and moon, (P. 464.) De l'Isle apprised Bradley of these particulars, and requested him to send him what observations he had been able to make, that they might be added to those which had been supplied by the astronomers of Sweden, France, and Italy. When de l'Isle afterwards made the necessary calculation, he acknowledges that although the mean came out nearly the same from all, yet he did not find the different observations of other astronomers to agree so well among themselves as Bradley's, (P. 481.)

[x] P. 40.

[y] P. 39. There is a paper on which he has taken out the difference of zenith distance between Alcore and ζ Ursæ Maj. on those nights, between 1728 and 1732, on which he had observed them both: this may have been to discover if there was any proper motion which was apparent in either, or it may have been with reference to a suggestion of Wallis, in a curious paper on parallax which was printed in the Phil. Trans. vol. XVII. p. 844.

[z] Phil. Trans. vol. LXXIII. p. 247.

[a] P. 457. [b] P. 454.

[c] Nov. 1748. See Lalande Bibliographie Ast. p. 435. See also de l'Isle, p. 457.

" be paid or not to the Rev. James Bradley, in discharging him from future payments, " and in giving him liberty to take up his bond gratis, in consideration of his useful and " curious discoveries and inventions in astronomy, which redounded greatly to the " honour of the society—which being put to the ballot passed nem. contr. in the affirm- " ative." This, at the time when it was voted, was the most advantageous way to Bradley, in which the society could have distinguished him; but in 1748, when he was above the world, there was equal consideration and right feeling in giving him the Copley medal for his paper on nutation[e]. His early friend, Martin Foulkes, was now President, who had introduced[f] the practice of delivering an appropriate discourse on the presentation of this highest mark, which the society then had to bestow, of its gratitude and approbation. There were ample topics for him on this occasion; those, however, which he used of a public nature, add nothing to the facts with which we are already acquainted. Sir Hans Sloane presided when the council, in 1730, cancelled the bond, and it is mentioned that, as the last surviving executor of Sir Godfrey Copley, he had the pleasure of assigning, at the President's suggestion, the medal on this occasion to the author of the paper on nutation. The distinction must have been a subject of great gratification to Bradley, and it is with some regret that we find he was unable to receive the honour in person. His business in Oxford prevented his being at the meeting on St. Andrew's day, when the presentation took place.

Bradley may easily be conceived to have had a very intimate connection with the Royal Society, and there can be no doubt of his having been esteemed in it as he deserved; and yet the commonly received account is rather at variance with such an idea. It is said[g], " he was elected member of the Royal Society and one of the council, Jan. " 25, 1752, in the room of Dr. Cromwell Mortimer." From this, it has sometimes been understood that he was not even a fellow before that time, and M. Delambre expresses[h] the astonishment which must necessarily have been felt if this fact had been true; but it is no more so than the impression, which the latter part of the sentence is calculated to give us, of his having been elected for the first time into the council on the day which is there stated. On the contrary, he was selected for that honour in 1725, 1726, 1728,

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[d] Minutes of the council of the Royal Society.

[e] When the paper was read, the vote of thanks was not worded in the common formal manner. They were offered to Lord Macclesfield, who had communicated the paper, and they " were likewise ordered to Dr. Brad- " ley, for his great pains and unwearied dili- " gence in carrying on, through a course of so " many years, so noble a series of the most " accurate observations; which, joined to his " great knowledge and sagacity, have enabled " him to make such discoveries as will be a " lasting honour to himself, to his country, and " the present age."

[f] Kippis's Life of Sir J. Pringle, prefixed to his Six Discourses, p. xxxvi.

[g] Sup. to the Biog. Dict. p. 57.

[h] Hist. de l'Ast. an. xviii siècle p. 421. All the papers from 1723 to 1748, which Bradley printed in the Philosophical Transactions, will be found to have F. R. S. annexed to his name; but this may easily have escaped M. Delambre's notice. He was overhasty, however, not only in blaming the society for their supposed neglect of Bradley, but also for what he says on the same subject with respect to La Caille. After an enumeration of the academies of which he was a member, (p. 504,) it is added, that no notice was anywhere found of his having been of the Royal Society of London. Now the fact is, that his certificate was sent over from Paris in 1759, signed by La Condamine, Clairaut, and Doutours de Mairan; it was read on the 21st of June, and he was elected on the 17th of January 1760.

1730, 1742; and it was after having thus repeatedly served the office, that his name was again put forward, on Dr. Mortimer's death, in Jan. 1752. The majority likewise did not betray any feeling of neglect for him, since the votes were, for Dr. James Bradley 95, for Dr. James Parsons 7, and for Jos. Andrews, esq. 1.

It was not, however, among his own countrymen alone that Bradley was esteemed and honoured. His merits were acknowledged also by foreigners. Euler himself wrote[i], in July 1746[k], to announce that he had been received a member of the Royal Academy of Berlin. In July 1748, he became one of the foreign associates of the Royal Academy of Sciences at Paris. The correspondence in this volume contains the congratulations of Grischow[l] and of La Caille[m] upon that occasion, as well as the letter which Bradley wrote himself in French[n] to the Comte de Maurepas, to acknowledge the honour which had been done him. In 1750[o] he became a corresponding member of the Imperial Academy of Sciences at St. Petersburgh, and had the opportunity of rendering material service to that learned body by superintending the quadrant which Bird made for them in 1752[p]. This may have added to the inducements, derived from his reputation, in procuring him the honour of being admitted as one of their members in 1754[q]; and in June 1757 he was chosen a member of the Institute of Bologna[r]. There was no scientific society in Europe, as De Fouchy remarks, which was not proud of attaching him to its establishment. But amidst all these distinctions, wide as the range of modern science, and permanent as its history, there was one which probably came nearer to his heart, and was still more gratifying to his feelings, than all the long list which we have enumerated. Lowth, (afterwards Bishop of London,) a popular man, an elegant scholar, and possessed of considerable eloquence, had, in 1751, to make his last speech in the Theatre at Oxford, as Professor of Poetry. In recording the benefits for which the university was indebted to its benefactors, he mentioned the names of those whom Sir Henry Savile's foundation had established there: "Qui viri Academici! quanta in Mathesi nomina! "Savilio debemus Briggium, Wallisium, Halleium; eidem Savilio Greavium, Wardum, "Wrennum, Gregorium, Keilum, ne nominem, quem posteri nunquam tacebunt[s]." Bradley was himself present: there was no one in the crowded assembly on whom the allusion was lost, or who did not feel the truth and justice of it: all eyes were turned to him, while the walls rung with shouts of heartfelt affection and admiration:—it was like the triumph of Themistocles at the Olympic games.

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[i] To C. Wetstein. Journals of the Royal Society, Oct. 23, 1746.

[k] This by mistake has been said to have taken place in 1747. Hist. de l'Acad. R. des Sciences, 1762, p. 240.

[l] P. 451.

[m] P. 454.

[n] P. 455.

[o] P. 465.

[p] P. 466, 476.

[q] Hist. de l'Acad. Roy. des Sciences, 1762, p. 241. Supp. to Biog. Dict. p. 57.

[r] Ibid. Ibid. in the Supp. where the name of the place by mistake is printed Bulogne.

[s] See the Creweian oration, printed at the end of Lowth de Sacra Poesi Prælectiones, 4to. 1753, p. 363.

CHAP. IX.

Instruments provided for the Observatory at Greenwich in 1725—Grant to Bradley for new ones in 1749—The zenith sector—Quadrants—Transit—Short's telescope—Openings in the building for observation, and other arrangements.

IT has been already mentioned[t] that Halley owed his appointment as Astronomer Royal principally to the good offices of Lord Chancellor Macclesfield, and he acknowledges the gratitude which he owed to him in the dedication of the 30th vol. of the Phil. Trans., published in 1720. Science is likewise indebted to this same nobleman for the Greenwich observatory's being first furnished with instruments as a public foundation. There is, among the papers of the late Dr. Hornsby at Oxford, a manuscript life of Halley and there are some circumstances which seem to indicate that it was drawn up by Israel Lyons, for the collection which it is well known that he once intended to make of that great man's most numerous publications. It mentions, that Lord Macclesfield's recommendation to the office was supported by that of the Earl of Sunderland, who was secretary of state, and that the instruments for the observatory were procured by means of " a sum of money obtained from his majesty's treasury for that " purpose, chiefly through the favour and assistance of the noble lord first men- " tioned, and who was pleased to add to his other great qualifications that of being the " greatest promoter of science and patron of men of learning of his time."

From the minutes of the council of the Royal Society it appears, that on the 12th of May, 1726, Halley reported, that the money advanced by the treasury had been expended, and that more was required; on which a committee, consisting of B. Taylor, Stukeley, Halley, Graham, and Foulkes, were appointed to examine what had been done.

On the 26th of the same month it was stated, that the committee had been on the preceding Monday at Greenwich, had seen the instruments and the vouchers for the money which had been expended; and the following report was then submitted to the council:

" 1st. As to the state of the observatory.

" That all the instruments now lodged or erected in the royal observatory, and be- " longing to it, were procured by the present professor. Those which were used by his " predecessor being carried off and claimed by his executors.

" That there is a room adjoining to the west end of the house newly erected, which " serves as an observatory for taking the transits of objects on the meridian: it being " furnished with a curious telescopic instrument, of five feet radius, fitted to an axis, " and adjusted with screws to revolve in the plane of the meridian, and a plain week " clock standing by it for making the observations.

" That the great room in the observatory is furnished with a plain month clock and " three very good telescopes, one of nine feet, another of sixteen, and a third of twenty- " four feet in length, and also two very good micrometers of different forms.

" That in the garden, from off the south-east corner of the house, there is erected

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[t] P. xlvii.

"another building, being a room designed to hold two large mural quadrants, of eight "feet radius, for observing the meridional altitudes of objects; one of which quadrants "is to command that part of the meridian which lies to the south, and the other that "part which lies to the north. And in the middle of the said room is erected a firm "stone wall, lying north and south, being 11 feet high, 9 feet long, and 2 feet thick, "consisting of nine large stones cemented. To the east face of which wall is affixed the "large quadrant, which is taking the observations to the south, being entirely finished "and fitted up for use[u].

"That they are informed, the materials for making the other quadrant are procured, "and many of its parts formed; as the brass limb, the iron bars, and the tube for the "telescope, with some others.

"2dly, Next as to the bills and vouchers for the monies expended in making these "works and instruments.

"The said report sets forth that they have examined the several particulars in the "said bills, which, in the total, amount to the sum of 435*l.* 10*s.* 4*d.*; and have had the "opinion of able judges, that the several particulars are reasonably rated, and are fully "persuaded that the several sums have been truly laid out with great frugality and "good husbandry.

"That it appears by the vouchers produced to them, that out of the sum of 500*l.*, "which was issued from the treasury for fitting up his majesty's observatory at Green-"wich, the sum of 40*l.* was paid for taxes and fees; so that there remains only the sum "of 24*l.* 9*s.* 8*d.* balance in the hands of his majesty's observer.

"3dly, They offer it as their opinion that the observatory cannot be accounted suffi-"ciently furnished with instruments for serving all the purposes required, unless there "be one large and substantial instrument, which is not confined to the plane of the "meridian, but fitted for making observations in any other vertical; for the want of "which, the Professor is at present obliged to make use of one which belongs to this "society.

"They further say, that they have formed a calculation, as near as they are able, of "the expenses requisite for finishing the quadrant already begun, and also of the "charges of an instrument of suitable bigness to make observations out of the meri-"dian; and in the whole are of opinion that the expense will amount to the sum of "200*l.* more than the balance now remaining."

This report having been adopted, Mr. Conduit was commissioned to wait on the duke of Argyle, as master-general of the ordnance, in hopes of inducing him to interest himself in obtaining the further funds which were required; but none were granted: and it was not till 1748, that the means were procured of completing the establishment of instruments. Bradley having found that all his attempts were vain to go on with those already in the observatory, represented the state of them to the members of the Royal Society, who attended as [x]visitors in the summer of that year; and in the following August, the council[y] took into consideration a petition to the lords of the admiralty, which he had drawn up on the subject. It was signed by himself, and having been approved of, was also signed by the President and the rest of the members of the council then present. Some alterations having been made, it was not presented before October; and on the 9th of November, the President acquainted the council that he and Dr. Bradley "had attended the lords of the admiralty, and that their lordships had "promised their assistance in forwarding the petition." There was a schedule annexed to it, containing a list of what was considered to be necessary for completing the observatory, and an account of the sum required for that purpose, which was estimated at 1000*l.* This was immediately granted by King George the Second, and the money was directed, by an order under the sign manual, to be paid to Dr. Bradley [z], in order to repair the old instruments of the observatory, and to provide the new ones which were required. In the original estimates, 100*l.* were reserved for the former purpose; but as it will be much more satisfactory to see how the money was actually laid out, than how it was intended to be disposed of, the following particulars are copied from the statement which was afterwards given in to the council of the Royal Society.

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[u] The position of the instruments shews the impossibility of the transit and altitude of the same object being taken with them by the same observer. See P. li.

[x] Hist. de l'Ac. Roy. des Sciences 1762. p. 239.

[y] Minutes of the Council of the Royal Society.

		£.	*s.*	*d.*
1742, Dec. 8.	By Mr. Sisson's bill for altering the transit	19	8	0
—— Aug. 10.	By a pair of globes of Mr. Senex	8	9	0
—— Feb. 9.	By Mr. Graham's bill for a pendulum	15	13	0
1744, Jan. 3.	By Mr. Graham for a pendulum	10	0	0
1745, Oct. 1.	By a micrometer for the mural quadrant, by Mr. Sisson	[a]3	3	6
1746, Feb. 11.	By an apparatus for trying the line of collimation, by Mr. Bird	2	12	6
—— May 29.	By an arch for the transit instrument, by Mr. Bird	8	13	0
1747, July 9.	By a level by Mr. Bird	1	11	6
1748, Oct. 12.	By alterations in the transit instrument, &c. by Mr. Bird	5	15	6
1743—1748.	By alterations made in the quadrant and transit rooms by smiths and carpenters, their bill	10	0	0
	By glasses and a perspective of Mr. Mann	3	18	0
	By a chamber-alarum by Mr. Graham	3	3	0
	By a parallactic sector 12½ feet radius	45	0	0
	By a diurnal sector, with a telescope of 30 inches	35	0	0
	By an apparatus for frequently observing the variation and inclination, or dip of the magnetic needle	20	0	0
	By a 15 feet refracting telescope and a micrometer	20	0	0
	By a clock pʳ Mr. Graham	39	0	0
	By a brass mural quadrant by Mr. Bird	300	0	0
	By a moveable quadrant pʳ ditto	200	0	0
	By a transit instrument ditto	73	13	6
	By a 20 feet refracting telescope pʳ ditto	7	10	0
	By a barometer pʳ ditto	2	12	6
	By a thermometer pʳ ditto	1	15	0
	By alterations to the old mural quadrant pʳ ditto	32	10	0
	By fees at the treasury	2	2	0
	By Mr. Short in part for a six feet reflecting telescope	100	0	0
		972	0	0
		28	0	0
		1000	0	0

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[z] Suppl. to the Biog. Dict. p. 56.

[a] This probably should be 3*l.* 13*s.* 6*d.* since 10*s.* are wanted in some one of the items to make up the sum total of 972*l.*

As the buildings were also erected in which the quadrants and transit were afterwards placed, a considerable sum in addition to this must have been laid out, probably by the Board of Ordnance[b]. The place at the west[c] of the dwelling house, in which Halley had his transit, must have been small, and very inconveniently situated. There is a drawing likewise, which seems to have been made from the plan of the room, in which the old quadrant was originally put up; and if it is so, the length of it was nearly 15 feet, with a width of a little more than 10. The sides of the pier must in this case have stood at an angle of 13° with the walls, which can only be accounted for by the supposition of its having been erected in some building[d] which was standing on the premises, and was found convenient for the purpose. This must have been taken down in 1749, when the new building was accommodated to the improved state of the observatory.

Most of the smaller particulars in the account have been already taken notice of[e], and the first article of importance is the parallactic sector, with respect to which the following memorandum[f] was found, in Bradley's own hand, among some papers in the library at Greenwich. "In the year 1749, 1000*l*. was given by his majesty, to be paid by the " treasurer of the navy out of money arising from the old stores of the navy, (upon the " representation of the lords of the admiralty, and principally upon Lord Anson's re- " commendation,) to buy some astronomical instruments for the use of the Royal Ob- " servatory; when it was proposed by Mr. Foulkes, Mr. Graham, and Mr. Robins, " who were consulted with on that occasion, that in the catalogue of instruments to be " purchased a parallactic sector should be inserted, as very useful for observing stars " near the zenith; and the sector which I had formerly hung up at Wansted, in 1727, " (with which I afterwards discovered the laws of the aberration of the fixed stars, as " also the nutation of the earth's axis,) being judged by them worthy of a place at the " Royal Observatory, I removed it from Wansted in July 1749, and procured a new " apparatus for suspending it, (made by Mr. Hearn, as the old one was;) and I likewise " took care, while the rooms of the new observatory were building, that there might be " made convenient places for hanging the sector, both in the new quadrant room and in " the transit room: my view (in providing for its suspension in either room) was, to " render it useful for settling the true zenith distances of such stars as come within its " reach, (or within 6¼° on either side of the zenith,) whereby errors of the line of colli- " mation of the telescopes of the mural quadrants may be found with great ease and " certainty." This provision for two separate places of suspension was with a view to the advantage of reversing the instrument, the face of the sector[g] being turned to the east when it hung in the quadrant room, and to the west in that of the transit. The removal, however, from one room to another, was attended with risk, as well as inconvenience; and in 1769, when some alterations were made in the observatory[h], Dr. Maskelyne had a second support put up in the quadrant room, on the opposite side to that which had originally been fitted up in it. The great advantage from reversing the sector is in determining the absolute distance from the zenith. Now Graham originally constructed Bradley's for a different object, and all the divisions were "pretty nearly equal to one "another," but their dimensions were too large by 2″,6 in a degree[i]. The limb also was of brass[k], and the arc in consequence followed a different rate of expansion from the tube; Dr. Maskelyne, therefore, had a steel arc[l] substituted on it, with divisions to every 5′ of zenith distance, upon gold pins; and he mentions, in 1785, that this had been done by the late Mr. Sisson. It appears that Bradley was allowed 45*l.* for the instrument. In the estimate attached to the petition which was sent in to the admiralty there is inserted " a parallactic sector of 12 or 13 feet radius, for observing near the zenith, 45*l.*" It is possible, therefore, that this was the sum which would have been charged for a new one; and if so, we have a guide to what Bradley's originally cost him.

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[b] Maskelyne's Observ. vol. II. p. 160. The ten pounds to the smiths and carpenters was evidently for subsequent and less extensive work.

[c] P. lxxii.

[d] In the report of what was done for Halley, (P. lxxii.) the building in the garden appears at first sight to be described as if it had been erected on purpose for the old quadrant. But it may be remarked, that it is not said, like the room at the west end of the house, to have been "newly" erected; and it is hardly possible to conceive, that if it had been so, sufficient height would not have been given, to avoid the evil which Bradley found to have taken place, when he went to Greenwich in 1742. See P. 381.

[e] P. li. lii. liv.

[f] This is a paper referred to, P. xxvi. xxvii.

[g] Bradley's Greenwich Observations, vol. I. Obs. with Zen. Sect. p. 91.

The diurnal sector with a telescope of 30 inches is evidently the small equatorial which Dr. Maskelyne [m]mentions as having been made by Graham for Bradley.

The magnetic apparatus was probably a suggestion of his own; for magnetism was a subject in which he was interested. His friend Graham, as early as 1722[n], had made observations on the diurnal variation; and a certain number have been found which Bradley himself made on the same subject at Wansted in 1729.

The clock is put to Graham's account, but Dr. Maskelyne[o] states that it was made by Shelton, under his directions. Graham was born in 1675, and was now too old to work himself[p]; Shelton, who had been his assistant, was succeeding to his business: and in Bird he saw another artist rising to emulate his fame, and advance the improvements which he had made in the division of astronomical instruments.

We have seen[q] that it was originally intended to have furnished Halley with a second quadrant, and that even a portion of it was completed, when the funds being exhausted the execution of this part of the plan became impracticable. Bradley, in his paper on nutation[r], alludes to this deficiency; and the first article in the schedule of estimates is " a " brass quadrant, for the west side of the wall, of the same radius as that which is now " fixed on the east side." A full account of all the mechanism of this noble instrument has been given by the [s]maker, who used his utmost skill in giving it stability as well as accuracy; and as far as human art could contribute to his design he certainly succeeded. Roëmer indeed, in writing to Leibnitz, says[t], that he differs much from those who would fit up an observatory for show rather than use, and then adds, "nec nihil abeo ab alio-"rum opinione in ordinandis instrumentis cum quadrantum et sextantum usum plane "abrogandum censeam, magis fidens integro circulo quatuor pedum, quam quadranti "circuli decempedalis." Ramsden finished the circle of five feet for Piazzi in 1789; he arranged, though he did not live to complete, the Dublin circle: and Mr. Pond, with a circular instrument of Troughton's at Westbury, destroyed[u] the illusion in which Bird indulged himself, of its being possible for his quadrant to last many [x]ages without change of figure, or any diminution of its value. Indeed during Bradley's lifetime there is no reason to suspect that any alteration had taken place. On the 16th of Feb. 1750[y], it was suspended on the west side of the pier in the observatory, and in the June following it was ready for observation. It was placed there for the observations of the pole and other circumpolar stars, from which Bradley had to determine his latitude, and to collect data for his calculations of refraction. In 1753 it was taken to the west side, and Graham's removed to the place which it had previously occupied. Before this was done, the arc was carefully examined, in January, and was found by Dr. Bradley to be 2″, and by Bird only 1½″ less than a quadrant; it suffered no injury in being moved; for in July 1759 the examination having been repeated, no excess or deficiency was found by either of them. And the [z]plane was capable of being so well adjusted to the meridian, that the greatest difference at any part of the limb did not exceed 6″ in time.

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[h] Maskelyne's Greenwich Trans. Obs. vol. II. p. 160.

[i] P. 20; and Maskelyne's Obs. vol. I. Pref. p. x.

[k] Ibid. p. ix.

[l] Ibid. vol. II. Obs. with Zen. Sect. p. 25.

[m] Ibid. vol. I. Præf. p. ii.

[n] Phil. Trans. vol. XXXIII. p. 96.

[o] Green. Obs. vol. I. Pref. p. ii. This clock was afterwards successively improved by Arnold and Earnshaw, and its pendulum was hung from a pier detached from the clockwork. Vol. III. p. 339.

[p] He died in 1751.

[q] P. lxxiii.

[r] P. 19.

[s] The Method of constructing Mural Quadrants, by John Bird, 1768.

[t] Misc. Berol. Contin. II. p. 277.

Besides the mural instruments, Bird executed a moveable quadrant; it was proposed at first to be four feet in radius, which being inconveniently large was [a]reduced to 40 inches: and Bird completed this part of the apparatus by repairing the quadrant which had been made for Halley. He tells us[b], that when he understood that he should be engaged to work for the observatory, he made himself fully acquainted with the general construction of the old quadrant, and found the plan of it to be such as to afford a lasting testimony of Graham's great skill in mechanics; its greatest defect, and the cause which he considered to have contributed most to the alteration of its figure, was the use in several parts of iron, which could not be forged so advantageously as brass could be cast into the form which was required. It was to this instrument that the double division of the quadrant was first applied[c]. This was a severe test of the execution; but the repeated bisection, of which the number 96 admitted, was so advantageous, that if there should be any discrepancy, Bird recommended it to the greater[d] confidence. In 1753, when Graham's quadrant was removed from the place which it had then occupied for nearly 30 years, a new[e] set of divisions was put on it by Bird. With these Bradley examined the arch of it by means of his micrometer[f], and found the difference in it from a quadrant to be the same as he had before ascertained in a different manner[g].

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[u] Phil. Trans. 1806, p. 420.

[x] Bird, p. 26. [y] Ibid. p. 23, 24.

[z] Maskelyne, vol. I. Preface p. viii.

[a] Journ. of R. S. 1754, Oct. 10.

[b] Bird, p. 1.

[c] Bird's Method of dividing Astronomical Instruments, p. 5. Bird was fond of making all his work very solid, and the plate was originally entire which clamped the telescope to the limb of his quadrant. Dr. Maskelyne had the central part cut away. He likewise perforated the friction-wheels to give more light to the vernier. Bradley was "very desirous of having the tele-"scope to perform its office without bracing;" Bird "therefore hammered the tube extremely "hard, and made the trial, but without suc-"cess; it was therefore braced in imitation of "the telescope of the old quadrant." (Method of constructing Mural Quadrants, p. 15.)

[d] P. 27. It is curious to trace the gradual improvement which use suggests. Graham seems originally to have fixed the horizontal wire in the field of view of his quadrant, without making a provision for correcting any inaccuracy which might be found in it. Bradley has made the following memorandum, with reference to the repairs in 1742. "The horizon-"tal wire (before the telescope was altered)

There was yet another important instrument for which Bradley was indebted to the skill of Bird, and that was the transit. Pound's and Halley's were among the first which were made, and it is very probable that they were supported by the walls of the room, or on some stand which was used for that purpose; for the schedule describes "a transit "instrument with an axis of four feet, and a telescope of eight feet in length, to be sup"ported by two substantial stone pillars." The length was constructed according to these dimensions, but the axis was made ½ a foot longer[h]; and to diminish the effect of friction on the ends of it, the weight of the instrument was in part taken[i] off by the application of a counterpoise to each extremity of it. The aperture was 2,7 inches in diameter[k], which was however reduced to 1½ inch by the elliptical screen for illuminating the wires. It at first had a single eyeglass[l], but in this construction, the field of view, particularly if it is large, will be indistinct towards the edges; to remedy which inconvenience a double eyeglass was applied to it in January 1753. The lenses in it seem to have been fixed, for Dr. Maskelyne at the first entrance on[m] his office at the observatory, employed Bird to make the nearest eyeglass draw out, so that the distance of it from the wires might be accommodated to the powers of the eye for each particular observer. It was not till August 1772 that the sliding eyepiece was applied by Dr. Maskelyne to the telescope, which enabled him to bring the axis of the glass directly on the image of the object as it passed across the field, and by that means not only the oblique view of the side wires was avoided, but the occasion for the compound eyeglass was removed.

In the new building, the room intended for Bird's transit was connected by a short passage with that in which the quadrants were fixed. This was a great advantage, but still (as might naturally be expected) some of the first arrangements admitted of considerable improvements. Bradley's transit clock was fixed against the south wall on the eastern

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"inclined so that stars going out appeared be"low the wire, if it was brought upon them as "they entered the glass, and the difference "amounted to about 15″. Mr. Graham has "now made provision for altering the situation "of the wires in this respect; since they were "renewed, I find that stars now appear to move "a contrary way to what they did before; viz. "if they appear upon the wire at the entrance, "they appear a little above it going out, but "the difference (though sensible) is less than "'twas before. However, 'twill be necessary "to reduce them as truly horizontal as may be, "which should be done before I examine again "the line of collimation, which I purpose to do "when I have leisure;" he then has added, "the diameter of the wires in the focus of the "telescope is $\frac{1}{247}$ of an inch, which therefore subtends 8″½." The horizontal wire was afterwards diminished.—See P. lv.

[e] Bradley, Gr. Obs. vol. I. Zen. distances to the southward, p. 57.

[f] P. 78. [g] P. 77, 478.

[h] Bradley's Gr. Obs. vol. I. Pref. p. iii.

[i] Ibid. p. iv. The following memorandum occurs in a paper-book which was found in the library at Greenwich, "Jan. 4, 1755, I "placed the weights which support the new "transit at such distances that the pressure "of each cylinder on the brass notch was just "three pounds." In another part of the same book he says, "the new transit instrument "weighs 55 pounds."

[k] Maskelyne's Gr. Obs. vol. I. Pref. p. v.

[l] Bradley's Gr. Obs. vol. I. Pref. p. iii. iv. v.

[m] Pref. p. iv.

side of the meridian-openings, which was too far from the instrument; but he, with good reason, was very anxious to keep his clock as much as possible from any external motion: in 1742, he particularly mentions that "the clock by the transit instrument was taken down Sept. 3, and fixed up again very firmly by means of three pieces of oak plank, about 2½ inches thick and 10 broad, which were wedged fast into the brick wall." From the improvements which Dr. Maskelyne made in 1779, we may also see the difficulties which had previously been encountered[n]. "An indistinctness was produced in the appearance of the heavenly bodies, greater or less at different times, by the narrowness of the windows and openings of the roofs. This inconvenience was principally felt in the observations made with the transit instrument and mural quadrants, where the meridian openings being only six inches wide, generally occasioned a fluttering and scintillation of the fixed stars, and an indistinctness in the disc and tremulous or undulating motion in the limbs of the sun, moon, and planets." To remedy this, the windows of the great room were turned into glass doors, so that the whole, to the width of four feet, might be thrown open if occasion should require it: all the openings of the roofs were likewise enlarged, especially those in the meridian, which were increased in width to three feet; to prevent the heat which was produced by them, all the windows of the transit and quadrant rooms, excepting those to the north, were bricked up. The enlargement, however, of the openings made a precaution necessary, for which their original narrow dimensions might have been intended to provide; and in June 1784, "mahogany covers were put over the ends of the axis of the transit instrument, to defend it from the sun's rays[o]," when observations were taken with it at noon, or at short intervals from it.

In addition to all these instruments, there was a want of a reflecting telescope, and it was proposed to have a Newtonian of about 6 feet in length. It was estimated that this would require 115*l*. The 100*l*. brought into account as paid in part to Mr. Short for this instrument had been advanced to him for this purpose, and it is mentioned in the minutes of the council of the Royal Society for June 5, 1755. There was some discussion at that time about his taking back a Gregorian reflector, which he had probably provided for the observatory while he was at work on that which he had undertaken to make. This, however, he very soon agreed to, "[p]without any further demand on Dr. Bradley or others on account of it;" and at the same time he undertook to deliver the Newtonian in four months. As the money remained on account at the time of Bradley's death, in 1762, it is probable that, like many other eminent workmen, he promised more than he found himself able to perform. Short died at no very advanced age, in 1768; but he completed the instrument, and Maskelyne mentions it in his preface[q] as part of the apparatus of the observatory. He had previously contributed his services in another manner, for in the Journals of the Royal Society[r] there is notice of a letter which he had written to Mr. Davall on his method[s] of making large lenses. It is known that the mirrors which he first made for reflecting telescopes were of glass, and he describes his method of producing a spherical figure to be so good, that in the year 1749, at Dr. Bradley's desire, he ground some object glasses for the new instruments in the Royal Observatory at Greenwich, which were found to excel all those that were compared with them, in consequence whereof the use of them was adopted.

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[n] Greenw. Obs. vol. II. p. 160.

[o] Ibid. p. 325.

[p] Minutes of the council of the Royal Society, June 12, 1755.

[q] P. ii.

[r] April 30, 1752.

[s] He delivered "a full account" of it sealed up, to be preserved with the papers of the society, and not to be opened till he gave leave for its publication. He appears to have made no communication on the subject during his

CHAP. X.

Reformation of the calendar—Scientific pursuits of George, Earl of Macclesfield—Bradley's first use of the new quadrant at Greenwich—Refraction—Latitude of the observatory.

BEFORE Bradley was well settled in the enjoyment and full use of his new instruments, he was called upon to take a part in the great national question of the alteration of the style. It was not indeed of that nature, which could have been attended with any doubts, or long and perplexing considerations with respect to the suggestions which he had to make on the subject. The difficulty did not rest with him who had to direct what ought to be done—that was clear—but with those who had to bring about the reformation which was required.

When Pope Gregory XIII, in 1581, issued his bull for the reformation of the calendar, he established an improvement which ought to have been universally received without hesitation. But the alteration came from Rome, which was in itself sufficient to raise obstacles to its reception in protestant countries. In spite of these prejudices it worked its way on the continent; but even in the middle of the eighteenth century it had not been admitted in our own country.

The Earl of Chesterfield is said to have been the original[t] promoter of the assimilation of the British calendar to that of other countries, being chiefly influenced by the chronological inaccuracies which the difference tended to produce. The Duke of Newcastle " was too deeply impressed with the favourite maxim of Sir Robert Walpole and his " royal master, 'tranquilla non movere,' to relish a proposal, which was likely to " shock the civil and religious prejudices of the people;" but Mr. Pelham and Lord Chancellor Hardwick supported the measure; and it was finally carried in 1751. The bill was brought in by Lord Chesterfield on the 25th of February, and the next reading took place on the 18th of March, when he was seconded by George Earl of Macclesfield; some alterations were made in the committees, but it passed through both houses apparently without any serious opposition, and received the royal assent on the 22d of May. The principal public support of this great measure was divided between the two noblemen who spoke in favour of it on the second reading. Lord Chesterfield was persuaded of the convenience to be derived from uniformity with our neighbours; but in a flippant letter[u] to his son he acknowledges that he knew nothing of the other merits of the case, and boasts of having imposed upon his noble hearers by rounded sentences and amusing relations. Maty[x] mentions a valuable com-

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lifetime, but after his death it was opened by the council, and printed in the Phil. Trans. (vol. LXIX. p. 507,) under the title of " A me- " thod of working object glasses of refracting " telescopes truly spherical."

[t] Coxe's Memoirs of Pelham's Administration, vol. II. p. 178.

[u] 4to. vol. II. letter XXVII.

[x] Memoirs of Earl of Chesterfield, as quoted in Hansard's Parliamentary Debates, vol. XIV. p. 979, 980.

munication, which he received on the subject from Chancellor Daguesseau, but it probably was only of a political nature; and the letter, to which we have just alluded, does the justice of avowing that Lord Macclesfield had the care of the provisions of the bill, and supplied all the science necessary for the arrangements of it.

Lord Chesterfield took pains in the periodical[y] journals of the day to prepare the minds of the public for the change; but he found it much easier to prevail with the legislature, than to reconcile the great mass of the people to the abandonment of their inveterate habits. When Lord Macclesfield's eldest son stood the great contested election for Oxfordshire, in 1754, one of the most vehement cries raised by the mob against him was, "Give us back the eleven days we have been robbed of:" and even several years after, when Bradley, worn down by his labours in the cause of science, was sinking under the disease which closed his mortal career, many of the common people attributed his sufferings to a judgment from heaven for his having been instrumental in what they considered to be so impious an undertaking.

Lord Macclesfield's task, if it required more powers of abstract reasoning, brought him into contact with those, who were more ready to admit the force of his arguments. It was evidently with a view to this measure that he drew up the paper, which was printed in the Phil. Trans.[z], containing remarks on the solar and lunar years, and the method of finding Easter as it was then observed in most parts of Europe. He likewise printed the speech which he made in support of the bill: he says, in the advertisement prefixed to it, that he did so in compliance with the pressing instances[a] of many of his hearers, who must have felt that it required the quiet of the closet to be rightly comprehended.

It is very possible that the weight of Bradley's opinion had much more to do with this great measure than is generally understood. Lady Katherine, the wife of Mr. Pelham, was made ranger of Greenwich park in 1745[b], and he in consequence used occasionally to[c] reside there. This produced an intimacy with Bradley, whom the minister always treated with great consideration and friendship. There is mention repeatedly made of a "mark on Lord Chesterfield's house," which proves his Lordship's residence in the neighbourhood: and although his own habits were very different from those of the Astronomer Royal, a certain degree of intercourse probably took place between them, and Bradley's sentiments, from a personal knowledge, might have had the greater influence on him. Of their estimation in the opinion of Lord Macclesfield there can be no doubt, and his Lordship in his speech[d] refers particularly to them.

In the calendar prefixed to the liturgy in the early part of the last century, we find only two tables which follow the enumeration of "certain solemn days;" the one is, that of the moveable feasts for a number of years in advance, and the other is for finding Easter by the Sunday letter and golden number, which now stands second in our prayer-books: all the rest were added in 1751. Lord Macclesfield tells us that the bill was "drawn, and most of the tables prepared, by Mr. Davall, a barrister of the "Middle Temple, whose skill in astronomy, as well as in his profession, rendered him "extremely capable of accurately performing that work." He was secretary to the Royal Society from 1747 to 1759, and Nicholls[e] mentions that he became afterwards a master in chancery, of which court he was accomptant-general at the time of his death in 1763. The whole, his Lordship adds, was carefully examined and approved of by Folkes, who was at that time President of the Royal Society, and by Bradley[f]. This of course could hardly have been intended to apply to any of the legal enactments, and Bradley is particularly mentioned[g] as having composed the three general tables at the end. We have[h] his letter, in which he informed Lord Macclesfield that Basket had printed off all the tables before the alterations could be introduced which his Lordship proposed, and this part of the bill has been found among the papers at Oxford, with the emendations in his own hand. He made some corrections in the "rules to know "when the moveable feasts and holy-days begin," and he introduced the words "kept "in memory" into the enumeration of the days "for which particular services are "appointed." Table I.[i] with a few verbal emendations, is found as it now stands in our prayer-books. In Tables II. and III. between the second and third columns there were others inserted for the epacts, which were afterwards omitted, probably for the reasons stated in Lord Macclesfield's speech[k]: and some alterations were made in the directions. Among Bradley's manuscripts there is likewise the second old table for finding Easter accommodated to the new style.

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[y] Coxe, p. 179.

[z] Vol. XLVI. for 1749-50, p. 417.

[a] It was reprinted in the same year (1751) by Faulkner at Dublin, which shews that it attracted attention.

[b] Gent. Mag. vol. XV. p. 109.

[c] Lyson's Environs of London, vol. IV. p. 453.

[d] p. 22.

This was not the only instance in which Bradley's labours were joined with those of the Earl of Macclesfield. His Lordship was born in 1697[l]; he was therefore the younger of the two by a few years, and the patronage of the Lord Chancellor, which Bradley so gratefully acknowledges[m], was a means of bringing them much together. Whether his Lordship's fondness for astronomy was the cause or consequence of their intimacy cannot now be determined, but it certainly began very early. A transcript has been mentioned[n], made jointly by Bradley and himself, of the British Catalogue, which must have been previous to 1725, when the second edition was published of the Historia Cœlestis. Having become a fellow of the Royal Society in 1722, he continued to take great interest in its proceedings, and in 1752 was elected President, which office he held during the remainder of his life.

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[e] Literary Anecdotes, vol. II. p. 372.

[f] There were probably others, who contributed their assistance, although their names are not recorded. Hutton, in his account of Walmesley, says, that he "was the last survivor "of those eminent mathematicians who were "concerned in regulating the chronological "style in England, which produced the change "of style in this country in the year 1752." Phil. Trans. Abr. vol. XI. p. 17.

[g] Lord Macclesfield's Speech, p. 22.

[h] P. 461.

[i] To the rule for this table it might possibly have been useful to add, that the dominical letter which it determines, is the second of the two when the year is bissextile.

[k] p. 20.

[l] None of the printed accounts, or the obituaries in the magazines, give the year of his birth; but in one of the Shirburn observation-books the following memorandum is entered against the 17th of March, 1764: "This morn-"ing died the Earl of Macclesfield, aged 67." For the general account of him, see Park's edition of Lord Orford's Royal and Noble Authors, vol. IV. p. 272.

[m] P. 41.

[n] P. liv. note q.

The Earl of Macclesfield succeeded to his father's title and estate in 1732, the same[o] year in which Bradley removed to Oxford; and Shirburn Castle being at a distance of only seventeen miles, he frequently visited a mansion in which he was so kindly received. This intercourse contributed, no doubt, to the determination of erecting an observatory there. The exact period when this took place is uncertain, but it was not later than 1739. In 1738, there are records of the time deduced from observations of the sun's altitudes; but these were not taken on the meridian, nor with Sisson's large quadrant, for the error of its limb (in 1745) was 5″, whereas the error allowed for at this time was 30″, which was the quantity[p] found by Bradley for some smaller instrument. The earliest meridian observations which have been found among those made at Shirburn are in Dec. 1739. They are taken at the transit almost entirely by Bradley, and probably with a view of bringing the instrument into proper adjustment. This will agree well with the whole tenor of his letter in October of that year[q]. On the 4th of June, 1740, Lord Macclesfield began the regular series of his own observations, which were continued till the end of his life; and entries in Bradley's hand occasionally appear interspersed amidst his Lordship's. In the winter of 1740-41, they were both engaged in observing ζ Ursæ and Alcore[r].

The observatory stood about 100 yards south from the gate of the Castle, and a little to the west of it. It was not erected for mere amusement. It consisted of three rooms, one was fitted up for his Lordship's accommodation as a bed-chamber, which he occasionally occupied when he passed the night in his observations. Bliss says[s] incidentally, in a letter to Bradley, "My Lord lay two nights last week at the observatory." A second contained the transit, and the third a mural quadrant, (both of which instruments were made by Sisson,) and in this third room there was a bed for the assistant. His Lordship at first, with the occasional aid of Bradley, seems to have made the observations for himself; but in January, 1742, he admitted an old servant of the family, Thomas Phelps, to work under him. John Bartlett afterwards had a similar trust reposed in him; and there is an engraving of them, in which the latter is represented as preparing to write down the observation which his elder coadjutor is taking at the transit. This instrument was five[t] feet in length, and seems from the drawing to have been very similar in proportion to Halley's telescope; but the arms of the axis are conical, equal to each other in length, and supported on two stone pillars. It had originally three wires, which were afterwards increased to five, probably by Dollond, who certainly provided the instruments at one time with achromatic object-glasses.

The quadrant had Graham's division of 96 on the limb, as well as that of 90°. In the summer of 1745, a new set of divisions was made on it by Bird, and when it lay upon a plain table made to receive it, the error of Sisson's quadrant was found to be 10″ in defect[u]. After it was put up, its figure was examined with one of Graham's levels, and "it appeared, upon a medium of several trials made by his Lordship, Dr.

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[o] P. 24. [p] P. 421, 422. [q] P. 419.

[r] See P. lxix. n. [y]. [s] P. 423.

[t] This is the length given in the observation-books: Hornsby (Ph. Trans. vol. LXIII. p. 103.) calls it $5\frac{1}{4}$ feet. The difference probably consists in one measure's being referred to the distance of the object and eye glasses, the other measure to the length of the tube. In this last particular Halley's was 5f. $6\frac{1}{2}$ inch. though the instrument is usually described as being only five feet in length.

[u] Shirburn Obs. June 1745.

" Bradley, and Mr. Bliss, several different days[x]," that the arc was enlarged 5″ by being placed in a vertical position. The first operation of Bird was to determine the 96 parts, and to subdivide each of them into 8. Six years afterwards he bisected each of these subdivisions[y], and then the beam compass, being adjusted to the length of 60°, was applied to a number of points on the limb, and none were found to vary from their true position by more than 1″. The same trial being made on the radius of the instrument, its true length appeared to be 64,4173 inches; but the middle of the arc was found to be a little too near to the centre of the quadrant[z]. The books in which the observations were entered, as they were made with these instruments, are preserved in the Savilian library at Oxford. They are in a regular series for the transit from June 1740 to June 1787, and for the quadrant from January 1743 to March 1793; the portraits of Phelps[a] and Bartlett were drawn in 1776; after which time the name of Redding occurs as of one employed in the observatory: the adjustments being occasionally examined by Bradley, Bliss, and Hornsby, who all in their turns enjoyed the hospitality and joined in the scientific pursuits of the noble owners of Shirburn Castle.

The quadrant clock was made by Tompion, and that, which was used for the transit, by Graham; both appear to have been provided with mercurial pendulums. In the back ground of the print that has been just mentioned, there is the drawing of a telescope on a stand, which has much the appearance of one of Dollond's achromatics. There is in the Savilian library a volume of Tables which are chiefly written with his Lordship's own hand, and among them are two for the values of his micrometer when applied to telescopes in length 14f. 4 inch. and 6f. Besides these, he had the use of the Huygenian glasses of 210 and 120 feet, which were lent[b] to him by the Royal Society.

Such were the advantages to which it was Bradley's good fortune to have access. A friend and patron like Lord Macclesfield justly deserved the compliment of being made the channel through[c] which the discovery of nutation was afterwards communicated to the Royal Society. In that paper Bradley particularly mentions his esteem and the use[d] which he had made of the Shirburn observations. The quadrant admitted of being removed to the opposite sides of the pier upon which it was hung, and this gave it a great advantage: it was constructed with every care, and there were parts of it which Grischow particularly noticed[e] as desirable to be introduced into the large instrument which Bird was to make for the academy of St. Petersburgh. The transit likewise had advantages over Halley's, and there is a letter at Shirburn Castle from Bliss to the Earl of Macclesfield, (dated Feb. 19, 1743-4,) in which he says, " Dr. Bradley is " much pleased that the comet has been seen on the meridian at Shirburn, because his " nephew could not find it at Greenwich; the reason of which he supposes is, because " they have no tube on the end of the telescope to screen the object-glass from the sun." All these circumstances must have made Bradley the more weary of his old instruments, and not only have suggested many things which would be advantageous in the construction of his new ones, but have made him better prepared for the use of them.

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[x] P. 478. Shirburn Obs. Oct. 1745.

[y] Shirburn Observations, July 1751.

[z] Hornsby says the radius was 5 French feet, and that in 1745 the body of the instrument was strengthened by screwing a large and broad plate of brass on the cross bars. Phil. Trans. vol. LXIII. p. 110. His description gives the impression of Bird's having, in 1745, divided each 96th part into 16, so that the bisections in 1751 were into half subdivisions. Hornsby himself was familiarly acquainted with the instrument. It may be right therefore to give Lord Macclesfield's own words: " Mr. Bird " having, in the year 1745, made a new set of " divisions on the mural quadrant to correspond " with every other (1536) division, came down " again to Shirburn the 22d of July 1751, and " inserted between those already made, other " points to correspond to the intermediate di- " visions which were before omitted."

[a] Phelps was likewise employed in conducting chemical experiments. Lord Macclesfield was much interested in this department of science, and built a very large and complete laboratory about 50 yards from the mansion, supported on arches: it was furnished with furnaces and every species of apparatus.

[b] Aug. 29, 1748. One of these (if not both) was certainly erected at Shirburn.

[c] P. 17. [d] P. 19, 41. [e] P. 466.

Bradley during the latter part of his life used generally to reside about three months every year in the university, and he had now ample occupation for the rest of his time at Greenwich. Before this period he had been able to observe his meridian altitudes only[h] to the south; but he was anxious to make a series of observations which would enable him to determine his latitude, and the amount of refraction at different altitudes. For this purpose, therefore, he had Bird's new quadrant directed at first to the north, notwithstanding the risk which attended the change of its place when his object should be completed: and he dedicated three years, from Aug. 10, 1750, to July 31, 1753, to the observation with it of α Ursæ Min. and circumpolar stars.

Refraction is of so much importance in the determination of the altitudes of objects, that we cannot wonder at Bradley's having attended to it even before he had the means of satisfactorily determining the question. In the volume of astronomical tables in the Savilian library, which belonged to Lord Macclesfield's observatory, there are several for refraction, and one in particular, which is entitled, " Dr. Bradley's table of refrac- " tions collected from the observations at Shirburn." The title proves that these calculations must have been made after the diploma was given him in 1742, and they probably contain some of the elements for which he acknowledges his obligations to his Lordship's " most accurate observations[i]." The table is not carried below 80° of zenith distance; but the following comparison will shew how near the quantities, that it contains[k], had been brought to the results, at which he finally arrived:

Zenith dist.	10°	20°	30°	40°	50°	60°	70°	80°
Shirburn.........................	9″½	20″	31″	45″	1′ 5″	1′ 35″	2′ 32″	5′ 14″
Maskelyne's Tab.[l] XXII.	10	20,7	32,9	47,8	1 7,8	1 38,4	2 35,5	5 14,8

Dr. Maskelyne had the advantage of having been " apprised[m] by Dr. Bradley him- " self of several particulars of moment relative to his observations, and particularly of " the method which he used for settling his latitude and refractions." We cannot therefore follow a better guide in this part of our inquiry. He tells us, that " [n]by " the observations of the pole star and other circumpolar stars above and below the " pole, Dr. Bradley got the apparent zenith distance of the pole; by the apparent and " equal distances of the sun at the two equinoxes, having at the same time opposite

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[h] P. 19.

[i] P. 19.

[k] These are not exactly what Dr. Maskelyne calls Lord Macclesfield's refractions, of which he tells us that he had a copy written out by Bradley, " being what he used " to correct his observations by before he had " been able to determine the refractions by the " new mural arc." See Phil. Tr. vol. LXXVII. p. 172.

[l] p. 15.

[m] Phil. Trans. vol. LXXVII. p. 153.

[n] Ibid. p. 154. See also p. 75.

" right ascensions," he " found the apparent zenith distance of the equator, which " lessened by parallax and added to the apparent zenith distance of the pole, gave a " sum less than 90°, by the sum of the two refractions belonging to the pole and meridian zenith distance of the equator.......The sum and difference of refractions answering to the pole and equator being thus given, the refractions themselves are given; " but he afterwards, from the consideration that the refractions at the pole and equator " may be taken without sensible error as the tangents of the zenith distances, according " to Mr. Thomas Simpson's theory of refraction in his Mathematical Dissertations[o], divided more accurately the sum of the refractions at the pole and the equator into the " just parts answering to each zenith distance, and thereby found the latitude with more " exactness. In this manner he found the latitude of the Royal Observatory to be " 51° 28′ 39″½, and the mean refraction at 45° 3′ to be[p] 57″, the barometer standing at " 29,6 inches, and the thermometer of Fahrenheit's scale at 50°."

Dr. Maskelyne then confirms his account by a passage[q] which he gives in Bradley's own words. The extract was made from the paper-book which has been[r] before referred to, and contained all which was necessary for his immediate purpose; but as there are other particulars annexed, which are connected with Bradley's researches, it will be better to copy it more fully from the original. It is as follows:

	°	′	″
" The apparent zenith distance of the equator by the mean of 20 " observations in 1746, 1747, was	51	27	28
" The mean apparent distance of the pole by the observations made " between 1750—1752	38	30	35
	89	58	3
" Sum of refractions		1	57
" Polar refraction			45½
" Equat. refraction		1	11½
" Latitude ....=	51	28	39½
" Co-Lat. ....=	38	31	20½

" The equatorial refraction is 2″ or 3″ greater in March than in September, and the " right ascension differs 5″ when the zenith distance is the same, for which an allowance " ought to be made in settling the stars' right ascension or the equinoctial points."

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[o] p. 46.

[p] In the Explanation and Use of his Tables (p. v.) Dr. Maskelyne observes that Bradley supposed the sun's parallax to be 10″⅓, and that had he made use of the true parallax of 8″,8 or 8″¾, he would have found the refraction at 45° to be 56″½ instead of 57″, and the latitude of the observatory to be exactly 51° 28′ 40″ instead of 51° 28′ 39″½. On the other hand most writers have found the refraction at 45° more than 57″, and Bessel does so from Bradley's own observations.—See Delambre, Ast. vol. I. chap. XIII. p. 325, and Trans. of the R. Irish Ac. vol. XII. p. 80. But the quantity possibly varies for different situations in a manner for which we have at present no instruments to afford us the proper measures. Dr. Wollaston found the hygrometer inefficient, (Phil. Trans. 1803, p. 8.) and so did Delambre, (Astr. vol. I. p. 320.) yet there can be no doubt of refractions being affected by vapour. The vicinity therefore of the Thames might be expected to have increased it, and yet Mr. Groombridge, on Blackheath, made the amount of it greater than Bradley found it at Greenwich, (P. T. 1810, p. 201.)

[q] Phil. Trans. vol. LXXVII. p. 156.

[r] P. xxiv.

A difference like that which has just been pointed out would be consistent with the effect of temperature, which must generally be lower at the vernal than at the autumnal equinox. If r is the refraction at any zenith distance, b the height of the barometer in English inches, and t the number of degrees on Fahrenheit's scale at which the thermometer may be standing, it is known that Bradley's formula gives $57'' \text{ tang. } (\text{Z.D.} - 3r) \frac{b}{29{,}6} \times \frac{400}{t+350} = r$. This was first published in 1763 by Dr. Maskelyne, in his British Mariner's Guide[u], and afterwards in the Phil. Trans[x]. &c., and several curious notices respecting it have been found among the Oxford manuscripts. The coefficient of r was not settled 'till after many trials. There is a paper, on which is written, in John Bradley's hand, "From the mean zenith distance subtract 4 times the refraction to find the "apparent refraction by the tangent." This was probably soon found to be too large, and possibly the next step may be traced in the following memorandum which Bradley has entered in the Greenwich book: "The refractions in all altitudes above 5° are nearly "as the tangents of the apparent distances, lessened by about three times the refraction; $\begin{cases} \text{in summer } 3\frac{1}{2}, \\ \text{in winter } 2\frac{1}{2} \end{cases}$." There are no means of ascertaining precisely the time when this was drawn up; in the beginning of[y] 1753, he certainly had not satisfied himself on the subject, but in writing to La Caille about 1757, he speaks with more confidence when he[z] refers to the same fundamental quantities. Below 5° indeed of altitude, there are still great difficulties in all attempts to reduce the refraction to precise rules[a]; but there is another remarkable circumstance, for Bradley evidently considered at first that the coefficient might be variable which was to be applied to the quantity of refraction subtracted from the apparent zenith distances. It is possible, however, that this might have been occasioned by an attempt to introduce the correction for temperature into this part of his formula, which he afterwards applied to the whole quantity.

The mean refraction at 45° being nearly 1′, Bradley took his constant of 57″ from 45° 3′; because at that apparent altitude the tangent of (Z. D. $-3r$) might be considered as unity, and it then remained for him to settle the corrections which must be applied to the changes in the state of the atmosphere.

He seems from the beginning to have considered the refraction as proportional to the height of the mercury in the barometer, and to have found no reason for altering his estimate of this ratio. In settling his coefficient for heat and cold, we find nothing which can determine whether Bradley began by instituting any original experiments on the subject. He seems to have set out with an estimate of the air's being contracted one part in 450[b], by a variation of temperature equivalent to 1° of Fahrenheit's scale, and afterwards to have introduced such modifications as would best accommodate the quantity of elasticity to the effects which he detected in his observations.

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[u] p. 120.

[x] Vol. LIV. p. 265. See also vol. LXXVII. p. 157.

[y] See P. 483. Mr. Howe, from whom the letter is written, is described by Dr. Maskelyne (Expl. and Use of Tables, p. ix.) as "an indefatigable and accurate calculator, who had "made a variety of computations upon observations, ancient and modern, and inferred "many deductions from Dr. Bradley's observa- "tions in particular."

[z] P. 496.

[a] Delambre, Astronomie, vol. I. p. 318, 320.

[b] Greenwich MS.

The following numbers which were found on a loose piece of paper will illustrate the method by which he proceeded.

Th.		50 Th.
“ 39½	2′ 46″,4	2′ 41″,8
42½	2 44,7	41,7
51	2 40,5	40,9
48	2 39,5	38,7
50½	2 41,7	41,5
69	2 30,5	38,1
70	2 30,5	38,5
32	2 43,6	36,4
22	2 50,5	39,4
32	2 49,8	42,6
28	2 47,2	38,5
32	2 46,8	39,6
34	2 46,0	39,6
44	2 40,6	38,2
66½	2 31,2	37,7
69	2 32,7	40,2
59	2 37,2	40,8
61½	2 36,6	41,2
24	2 48,5	38,2
		2 40 mean

°	″
69	30,5
70	30,5
66	31,2
69	32,7
62	36,6
	11,5
67,2	2 32,3

°	″
32	43,6
22	50,5
32	49,8
28	47,2
32	46,8
34	46,0
24	48,5
	52,4
29	2 47,5

67,2	2 32,3
29,0	2 47,5

These give $y=354$

$350+t : B :: (77''\frac{1}{2}$ or$)\ 77'',6 : m$

Suppose it $350+t : B :: 78'' : m$ and $n=3\frac{1}{3}$.”

There is a paper book, on which is written, “Observed refractions;” from which it appears that the quantities in the second column were adapted to 70° of zenith distance, and were deduced from observations of stars, which passed under the pole nearly in that situation. With the exception of the last, they were all made between Oct. 25, 1750, and July 24, 1753, and consequently with Bird's quadrant. The latest date in the book is Feb. 7, 1754, which at least marks the period before which the calculations were not completed. It would have been tedious, and in this case superfluous, to make distinct computations[c] for each observation. Bradley contented himself with taking the two extremes of temperature when the differences in each case did not exceed 10°, and the numbers by the side give the means of the greatest and least heights of the thermometer, and of the refractions which belong to them. The quantity was thus found for 67°,2 to be 2′ 32″,3

[c] P. 418.

and for 29,0° to be $2' \, 47'',5$, which fundamental determinations having been made, it followed from $y+29 : y+67,2 :: 152'',3 : 167'',5$ that y would be equal to 354[d]. $77''\frac{1}{2}$ $77'',6$ or $78''$ was the result of multiplying $\frac{400}{296}$ into the number of seconds which were equal to the refraction at 45° 3′ at the standard heights of the thermometer and barometer. For B is the height of the barometer in tenths of inches; and, according to the laws assumed for the variation, we have $(350+t) \times 296 : 400 \times B ::$ the mean refraction at $45^\circ \, 3' : m$, or the real quantity for t and b. Hence $350+t : B ::$ refraction at $45^\circ \, 3' \times \frac{400}{296} : m$. It appears, therefore, that Bradley was now hesitating whether for the mean quantity he should adopt $57'',35$ or $57'',42$ or $57'',72$.

At the Oxford observatory there are a number of manuscript books and papers of Charles Mason, as well as many of his letters to the late Dr. Hornsby. In one of these, dated July 27, 1774, he says, " in computing the refraction, Dr. Bradley made use of " the mean of the thermometers within and without, (∵ taking half the sum;) and then " the general rule followed was, as 350 + the thermometer : the barometer :: the constant " number 77″ to the refraction at 45° 3′, then as rad. to that at 45° 3′ :: tang. zenith " dist. lessened by three times the refraction : the refraction required[e]. This rule was, I " believe, partly deduced from observations, and partly from Mr. Simpson's (late of " Woolwich) theory. But I very well remember Dr. Bradley's paying Mr. Simpson a " visit in the year 59 or 60, however it was after the lunar computations were nearly " gone through, that the Dr. at his return told me they had had some discourse on the " subject of refraction, and that Mr. Simpson rather supposed that the thermometer in " the open air only should be used. But I believe (though am not sure) the Dr. con- " tinued his computations as before." Mason in a note on the rule says, that it " is " supposed to hold good to 75° from the zenith ;" and he goes on in his letter to say, " Dr. Bradley took no notice of any inequalities, besides those of refraction and parallax, " in reducing the sun's true zenith distances about the time of the equinoxes, that I " can remember: but I have not one operation by me, though I made many hun- " dreds."

Bradley, for the ease of calculation, endeavoured to avoid fractional quantities, more, possibly, than the nature of his subject would always admit, and his coefficients have therefore been the subject of subsequent revision; but the simplicity and compactness of his formula, combined with its near approximation to the truth, have preserved its use, even when more elaborate investigations have been applied to the parts of the problem that are of greater intricacy[f].

Bradley did not always enter the heights of the barometer and thermometer with the regularity with which they are inserted in the registers of modern observations;

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[d] From this value of y the third column is calculated; $354+50 : 354+39,5 :: 2' \, 46'',4 : 2' \, 41'',8$ &c. &c.

[e] This is very nearly the same as given by Dr. Maskelyne in the Phil. Trans. vol. LXXVII. p. 156, 157.

[f] The rule seems to require that the refraction should in the first instance be known, in order to subtract the required quantity from the apparent zenith distance; but if for this purpose the value is taken at first from any table, the result will come out so near the truth, that by subtracting the triple of it and repeating the operation, the precise value is found by a quick approximation.

and before 1750 we never find them noticed at all for any of the fixed stars. They are always given with the sun's transit, but it should seem without any intention of applying them to the present object. Their being annexed to those very observations, which are independent of refraction, would raise this suspicion, but there is another circumstance which confirms it. When comparisons of the time are registered between the quadrant and transit clocks, the heights of the thermometer and barometer are entered in the same manner as at the sun's transit; it seems probable, therefore, that the primary object was to judge of the rate of the clock and of the effects which the changes in the temperature and density of the atmosphere might have upon the pendulum[g].

In the Greenwich manuscript there is the following entry: "Memor. ½ the sum of the "observed zenith distances of the pole star will differ but $\frac{1}{10}$ of a second from the app. "z. distance of the pole,

$$\begin{array}{r} '' \\ 50,98 \\ 44,13 \\ \hline 95,11 \\ 47,56 \\ 47,46 \\ \hline 0,1 \end{array}$$"

When therefore the refractions were settled, there could be little difficulty in Bradley's deducing the latitude of the place from the series of his observations on the pole star. In a paper[h] which Dr. Hornsby communicated to Dr. Maskelyne, he has shewn that this quantity by the new[i] quadrant came out 51° 28′ 38″½, and by the old quadrant with the new divisions 51° 28′ 41″½, the mean of which is 51° 28′ 40″, only ½ a second more than he had found it by another[k] method, and exactly what it would have been if in his calculations by that method he had used the true[l] quantity of the sun's parallax.

M. Cassini being desirous that the coasts of England and France should be united by trigonometrical measurements, had ventured to state that the latitude of Greenwich was uncertain to[m] 15″. Dr. Maskelyne drew up a valuable paper in reply to his assertion, and we are indebted to it for much of our information on the present subject. He adds[n] the results of 246 observations of his own, made between 1765 and 1771, from the mean of which he brought out the latitude of the observatory 51° 28′ 41″,3; and by correcting Dr. Bradley's for the sun's parallax as determined from the transit of Venus, he makes it 51° 28′ 40″,7; so that the uncertainty may be conceived to lie within narrow limits. Bessel, in his invaluable work[o], after a laborious application of all the resources of modern analysis to Bradley's meridian observations, concludes by saying, "quamobrem fidenter ponere licet Grenovici elevationem poli=51° 28′ 39″,56;" and Mr. Pond, from 720 observations in which he had combined the action of his two mural circles, concludes it to be [p]51° 28′ 39″,0. This is only half a second more than Bradley's determination from his observations with Bird's quadrant.

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[g] The same practice of annexing the heights of the barometer and thermometer to the sun's transits, was continued in Bradley's later observations, though they are not inserted there in the printed books. The Shirburn observations contain the same.

[h] P. 78, and Phil. Trans. vol. LXXVII. p. 160. The original ought to be at Oxford, for it appears that only a transcript was communicated of it; search therefore was particularly made for it, but without success.

[i] This is said to have been "after the new "quadrant was balanced, Nov. 24, 1750." Bird mentions the counterpoise in his first construction of the instrument, to have been so arranged that the telescope pressed at different elevations with unequal weight on the centre, the defect "was observed by Dr. Bradley, (whose vigi-"lance scarce any thing escaped,") and was then remedied. (Bird's Const. of M. Q. p. 18.) This would seem to answer to the balancing here alluded to, but the expression used in the observation-book at the time creates a difficulty; it is that "the telescope of the quadrant was "balanced, so that the plumbline seemed to be "the same in all positions of it;" whereas the plumbline was attached to the strong frame of the instrument, and was unconnected with the telescope.

[k] P. lxxxvi. and 76. [l] P. lxxxvi. note p.

[m] Phil. Trans. vol. LXXVII. p. 151.

CHAP. XI.

Pension granted from the crown of 250*l.* per ann.—Greenwich observations—Comets of 1757 and 1759—Directions for use of the micrometer—Lunar tables—Longitude by lunar distances —Resignation of the Readership in Experimental Philosophy—Transit of Venus—Termination of Bradley's scientific labours.

UPON the accession of George II, in 1727, Queen Caroline visited the observatory at Greenwich, and was much gratified with all she saw. She reminded Halley of his having formerly borne the king's commission, and procured him, for the remainder of his life, the enjoyment of half-pay as a [q]commander in the navy. The stipend, however, of Astronomer Royal received no increase, and Bradley when he accepted the office in 1742, became entitled to no more than the 100*l.* per annum, which had been originally assigned to Flamsteed, and which was considerably reduced by the fees deducted from it at the public offices. It seems to have been on this account, as well as out of personal regard, that Mr. Pelham offered him the vicarage of Greenwich. His refusal of this offer has been already [r]mentioned, and the sacrifice was made up to him by a pension[s] from the crown. This he most probably owed to the good offices of the same kind friend, who would have procured him the living; for that had become vacant in the month of [t]May 1751, and the warrant for the 250*l.* per ann. to be paid to him quarterly, during his majesty's pleasure, was given under the privy seal on the 15th of February 1752, and was to reckon from the preceding Christmas. In the instrument it was stated that the king was pleased to grant this pension "in consideration of the great skill and knowledge of "our trusty and well-beloved James Bradley, doctor in divinity, in the several branches "of astronomy, and the other parts of mathematics which have proved so useful to the "trade and navigation of the kingdom[u]."

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[n] Phil. Trans. vol. LXXVII. p. 165.

[o] Fundamenta Astronomiæ, p. 25.

[p] Mem. of the Astr. Society, vol. II. p. 529.

[q] Biog. Brit. 1757, vol. IV. p. 2515.

[r] P. viii.

[s] Suppl. to the Biographical Dict. p. 57.

[t] Lond. Mag. 1751, p. 236.

[u] In the XIIth volume of the Mémoires de la Classe des Sciences Math. et Phys. de l'Inst. de France, année 1811, there is an account of the life and works of Maskelyne, in which we find, at p. lxx. the following passage: "On raconte "que la reine d'Angleterre, frappée de la modicité du traitement alloué à l'astronome royal "pour un travail si pénible, avait offert de "l'augmenter. Bradley s'y opposa, dans la

This addition to his income[x] was well deserved by Bradley's previous merits, and was justified by his subsequent continued services. Even if he had only left us the Greenwich observations, his fame might have rested on the care and skill which they exhibit. Delambre seems much[y] displeased at Bradley's having been called "vir incomparabilis" with any reference to them. Amongst other objections, he says, "Qui sait d'ailleurs si toutes ces observations sont de Bradley? nous n'avons pas ce "doute en lisant celles de La Caille; il n'avait point d'assistant." This is all true, and it is impossible that the doubt can ever now be removed, for the observations were not of necessity copied into the books by the same person who made them, and the difference of handwriting will lead to no conclusion. But the excellence of the system which Bradley introduced, consisted much in taking distinct observations with those instruments which admitted of the best adjustments either for right ascension or altitude; and, both for the continuance of labour and the accuracy in the performance of it, a great advantage is always derived from the same individual's not being hurried by different objects, which interfere with each other. If the meridian observations were to be carried on simultaneously at the transit and the quadrant, it became necessary for the astronomer Royal to employ an assistant. But the assistants were trained by himself, and acted under his control; he was answerable for the performance of their duties, and he therefore had a right to connect their labours with his own.

The Greenwich observations for twelve years (1750—1762) have been published, and occupy 931 large folio pages: their number cannot be less than 60,000. Before they were printed, Mason calculated a catalogue of 387 fixed stars from them, which is annexed to the Nautical Almanack for 1773, and formed the basis of that which Hornsby afterwards inserted in the 1st volume[z] of the Observations. With these Mason was well acquainted, and he says, in the letter which has been before[b] referred to, "If your good-nature will ex-"cuse my freedom, I will beg leave to observe to you that I have seen one of the greatest "of men lost[a]. Some poor faithful hand might have been found, that, drove by necessity, "would gladly for about 30*l.* a year have reduced Dr. Bradley's observations one year "under another, and placed them in form proper to have appeared in the annals of "fame for ever." M. Delambre says[c], "jamais je n'ai trouvé le calcul d'une observa-"tion ennuyeux, quand il est fait le jour même:" and Mason (for he probably means his own faithful hand) might have done all which he professes if he had been enabled to undertake it successively. He would, however, have hardly done it so well as M. Bessel, who applied himself to the whole of this vast work at once: although Mason was a laborious computer, he had not the science necessary for such an undertaking as the com-

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"crainte que la place d'astronome, si elle valait "quelque chose, ne fût plus donnée à un astro-"nome." This could not be easily reconciled with his acceptance of the pension from the crown; but the truth is, that no such circumstances could have occurred to Bradley. Queen Caroline died five years before he was Astronomer Royal, and when there was another Queen of England, by the marriage of George the Third, in 1761, the deficiency of the stipend no longer existed.

[x] The grant was renewed to Bradley after the death of George the Second, and was continued to Bliss and Maskelyne, who succeeded as Astronomers Royal. George the Third was too munificent a patron of science, to withdraw an encouragement which was so well merited and so justly applied.

[y] Histoire de l'Astron. au xviii siècle, p. 426.

[z] p. xxxviii—xlv.

[a] "His own loser."

[b] P. lxxxix.

[c] Astronomie, vol. III. p. 245.

position of the Fundamenta Astronomiæ[d]. The elements, likewise, are now better understood than they were sixty years ago, and although it created a more serious call on the courage and perseverance of the calculator, he has been much better able to bring out his mean results from an investigation of the whole together, than could have been done for separate years.

The meridian observations constituted the regular business of the observatory, but although Bradley followed them with diligence, he was not drawn off by this part of his pursuit from others which were connected with it. The comets of 1723, 37, 42, 44, 48, had all in their turns been the objects of his particular attention; and there is in one of the books, a number of calculations to determine the orbit of the comet of 1707. He also [e]communicated to the Royal Society a particular account of that which appeared in 1757. There are two manuscript copies of the paper; one in the British Museum, which, as it belongs to Dr. Birch's collection, was probably that which was actually read; and the other at Oxford. This last is the rough draught, and there are a number of loose papers in it containing particulars of the observations and some of the calculations by which he traced out its elements. These are not so extensive as what we find for the comets of 1723 and 1737. Practice must have made him more ready in eliciting the quantities which he had to determine, and we have seen[f] that La Caille reports him to have found all the elements of the comet of 1742, before it had ceased to be visible. In the same volume of the Phil. Trans., in which Bradley's paper is printed, there is a letter to him from Klinkenberg[g] on this same comet of 1757. It contains some observations, in which, however, he had not sufficient confidence to think them worth the trouble of calculation. He tried the problem only graphically, and made a tolerably near approximation to the truth. His success in this respect is the most curious part of his dissertation, but it is of no further value; since Bradley's elements had been previously settled from his own observations, which Pingré [h] considered to be decidedly the best which were made of this comet.

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[d] There is one subordinate part of this valuable work, on which it may not be superfluous to state what has occurred from an examination of the original manuscripts. The author at p. 282-3 gives a list of 48 observations, which he found to be discordant from the known places of the fixed stars. From a careful collation of these, some variations have been observed in the following instances between the originals and the printed copies.

17. The hesitation and doubt about this star is hardly seen as the observation is printed. It was at first described as 38, which has been struck out, and " not a catalogue *" has been written against it. And for the following line, 40 had been written, but 39 was afterwards substituted, and then " qr. 39" has been added.

20. In the margin is written with a pencil, but not by Bradley, "Q. 41′ 17″."

30. A cross is made on the beginning of the line, and against d Aquilæ is affixed "q."

32. This observation ought not to have been printed, it was originally written $25\ 57\frac{1}{2}$, $25\ 56\frac{1}{2}$, which has been altered to $25\ 56\frac{1}{2}$, $26\ 36\frac{1}{2}$, and after all the pen was drawn through the whole.

41. 100° 10′ is written in the margin against this observation: this probably refers to the polar distance, and confirms Burkhardt's opinion of its being the 919th of Mayer.

44. This observation ought to have been printed even with the Q which the editor has annexed to it. The pen has been drawn through it in the original.

48. There is some little doubt with respect to the observation which is here referred to: on Dec. 6, 1751, (or Nov. 25. o. s.) there is the mention of a small star, to which P. D. 6° 20′ is affixed, answering to 83° 40′ of north declin. Now this is a press error for 62° 20′, and the bracket does not connect it with Andromeda in the MS., although the R. A. and real declination brings it into that constellation. On the other hand, this article 48. is referred to 23h. 50′ 59″ (the observed R. A. of 85 Pegasi) in the corrections of Bradley's observations, which were communicated by Bessel to the late Board of Longitude, and printed in the same volume with Mayer's Observations. This last, however, is probably a mistake: and it may not be amiss to correct another which the printer has committed in note [z] on the last page, by setting up 10 and 20 in the place 1_o and 2_o.

In Connaissance des Tems for 1821, Burkhardt published his remarks on these 48 observations, and expresses (p. 310) his suspicion of press errors having possibly occurred in some other observations which he mentions, these have been carefully examined and the printed text was found to agree accurately with the manuscript.

[e] P. 53.

[f] P, xliii.

In 1759, all the astronomers of Europe were anxiously looking out for the return of Halley's comet, and Bradley undoubtedly observed it very frequently. It is mortifying, however, for us to be able to derive comparatively little benefit from his labours on this interesting subject. Lalande[i] has preserved one of his observations, and eleven more have been found among his papers[k], which are now printed; but these are certainly not all which he made. Among the manuscripts at Greenwich there is a loose scrap of paper[l]; the top of it is torn off, but the remainder contains the following part of a note in Bradley's handwriting.

* * * * * *

"to be seen till the moon get near it again. We observed here again last night, and "compared it with the same star as the night before, which was 22d Sextantis.

"The elements which I have assumed are as under:

"Time of the perihelion, March 13th, 10h.		
"The place of the ascending node...	1^s 23° 30′	
"The inclination of the orbit.........	18 0	
"The log. of the per. dist.............	9,7659	
"The log. of the diurnal motion ...	0,3113	
"The perihelion after the node	111° 10′	
"or before the descending node......	68 50	J. B."

These elements identify the comet to which they refer, and the star shews that the note was written in the middle of May 1759: indeed Messier[m] compared the comet with 22d Sextantis from the 14th to the 17th of that month, but this star is not mentioned in any of the observations printed in the present volume. Lalande's observation of the 1st of May, differs likewise by an hour from that which we now have in addition to it for the same day, and every thing conspires to prove that these are only a part, and possibly a small part, of what might have been preserved under more favourable circumstances.

The second observation on the 25th of[n] May, suggested a hope of there being more to be found among the Shirburn observations, and there is the following entry in the transit book: “ ♃ May 3. 5h. 0′ 0″ The letter from Mr. Bliss (dated the 1st,) of the “ account of the comet, did not come till now, and the evening was cloudy, the comet “ could not be seen.” This gives us the time when it began to be seen in England, but nothing more. At Shirburn, as well as in Bradley's time at Greenwich, the observations of comets were written on separate loose pieces of paper, instead of being entered in the books; and to this we may attribute the loss, which in both instances has occurred, in 1759.

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[g] Vol. L. p. 483.

[h] Cometographie, vol. II. p. 62.

[i] Mem. de l'Ac. Roy. des Sciences, 1759, p. 35, 36.

[k] P. 379.

[l] In the book marked N°. 176.

[m] Mem. de l'Ac. Roy. des Sciences, 1760, p. 413-14.

[n] P. 380. A Greenwich observation of the 30th of April is written after it. These two last, together with the longitudes and latitudes, were written by Bradley, the rest is in Mason's hand.

Although every thing which Bradley did has a value in itself, there were so many observers in the middle of the eighteenth century, and the return of this comet was an object of such intense curiosity, that the loss is less severe. For more distant periods any addition to our stock of observations, though inferior individually in value, becomes, from the previous scarcity, of greater importance. Some, therefore, have been printed in the Appendix, which were taken when this comet appeared in 1607.

The account of the comet of 1757 was read before the Royal Society on Dec. 22. of that year: it was published in the fiftieth volume of the Philosophical Transactions, and will be found in the present[o] collection. It was the last communication which Bradley himself gave to the world: the “ Directions[p] for the use of the micrometer” were found among his papers by Dr. Maskelyne, and printed in 1772[q]. Halley mentions[r], that in 1719, when Mars was in opposition, Pound and Bradley observed its situation by means of Cassini's method[s], and the improvement, which Bradley made on the construction of it, may be seen in Smith's[t] Optics. This was “ the new contrived micrometer” which was finished in [u]Jan. 1721, and with which he made his observations of Mars in the following October. Lemonnier[x] mentions that Bradley used it for his observations of the comet of 1723, which were made, as it is expressed in his Latin[y] lecture, “ ope micrometri arti-“ ficiosissimi.” The Directions, however, apply not to this instrument, but to another kind of micrometer which consisted of two parallel threads, the one fixed, and the other attached to a frame, in which it was moved by a screw between two grooves that directed its motion. Smith's Optics, in[z] which Bradley's improvements of this instrument are mentioned, were published in 1738; and the directions are calculated for the form “ [a]as now “ contrived:” it is probable, therefore, that the paper is of a very early date. The construction of modern instruments makes the rules for this kind of micrometer of less practical advantage, but the particulars are valuable, because they are in fact a description of Bradley's method[b] of observing with it.

These, however, were only occasional additions to the regular work of the observatory, there was a much heavier labour of longer duration, for which the country is more indebted to Bradley than he has ever had duly acknowledged—labour to which he gave up those years of declining life, in which he might have been entitled to the quiet enjoyment of his well-earned fame. The great use to be derived from the moon's motion in determining the longitude, was admitted long before any practical use could be made of it. The two principal impediments were the want of a proper instrument for making the necessary observations, and the deficiency of the tables of the moon. The first had made Halley confine his [c]attention on this point to the appulses and occultations; but it was removed by the introduction of Hadley's sextant; and Bradley immediately[d] perceived the use which might be derived from it in finding the longitude at sea, "if ever the moon's theory be completed." This other difficulty did not admit in the same manner of being at once removed. In the preface to Halley's Tables, they are said to have been printed off in 1719. This is not quite accurate; for at a meeting of the Royal Society on the 2d of June, 1720, he exhibited "[e]his tables and discourse upon the motions of comets, being the last sheets of his book of astronomical Tables almost ready to be published." He was, however, now become Astronomer Royal, and the opportunity of verifying, and the hopes of improving his calculations, induced him to keep them by him for the remainder of his life. They were not therefore published till some years after his death, when certain portions were cancelled, and the precepts for the rest were supplied by [f]Dr. Bevis and Mr. Raper. Halley neglected several of the equations for the moon from their values being small, and during the earlier period of astronomy involved in uncertainty. His Tables, therefore, were not to be depended upon [g]within 1′; Newton [h]was of opinion, that if he had introduced all the equations and increased the longitudes by $1'\frac{1}{2}$, he would have come very nearly to exactness in his results. Notwithstanding this abridged form which was given to the Tables, La Caille, in 1744[i], expressed to Bradley his regret at not having had the advantage of being able to use them, and in 1748[k] he was again inquiring after them. Euler, however, had published lunar tables in 1745, of which he sent a copy to Bradley[l]; and there is at Oxford a letter of his to Gael Morris, from the beginning of which we may see the view which he had himself taken of them. He says, "Quod tabulas meas lunares non solum ad examen revocare, sed etiam exquisitissimas observationes Bradleianas mecum communicare volueris, id equidem gratissimo animo agnosco: neque admodum miror has tabulas tam enormiter ab observationibus aberrare, cum antehac ejusmodi observationibus potiri haud potuerim, ex quibus tabulas emendarem. Quamobrem etiam significaveram me eas ita exhibuisse prouti theoria Newtoniana mihi eas suppeditasset, meque de earum forma magis esse certum quam de singularum inequalitatum vera quantitate. Facile

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[o] P. 53.

[p] P. 70.

[q] Phil. Trans. vol. LXII.

[r] P. iv.

[s] Phil. Tr. vol. XX. p. 16.

[t] §. 876.

[u] P. 350.

[x] Théorie des Cometes, p. 91. and note p. 125.

[y] P. x.

[z] §. 878.

[a] P. 70.

[b] Compare P. 71 with P. 358, 376 note p; P. 72 with P. 359; P. 71, 73 with P. 357, 367, and P. 73 with P. 376, note p.

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[c] Lalande, §. 4172,3. [d] P. 505.

[e] Journal of the Royal Society.

[f] P. 431, 436, 459, 460. The license and the Latin title are both dated 1749, but 1752 is printed in the English title-page. The signatures at the end shew that great alterations were made after the work had gone through the press, either by Halley or his editors. Sheet P p was to have been cancelled, and is said in p. iii. of the Preface to have been omitted; but it is found in many copies, and contains the numbers referred to in P. 432. In Lord Macclesfield's copy at Shirburn Castle, there are some sheets not usually met with; they are marked Yyyy, Zzzz, which are not noticed in the Registrum at the end of the printed volume. They contain "stellarum annuas aberrationes supputandi methodus." If the subject did not prove it, the paper and printing would be immediately detected to be more recent than the rest. Heath, in his Royal Astronomer, (p. 234,) speaks of "the precept writer to Dr. Halley's Tables, (Mr. G. Morris,")who may have had some share in preparing the work for publication.

[g] Lalande, §. 1464. [h] Ib. §. 1459.

[i] P. 430. [k] P. 441. [l] P. 433.

"autem vir clarissime mihi largieris, si in singulis inequalitatibus levissimus error insit, "inde insignes errores in loca lunæ computata redundare posse. Statim ergo atque observationes quas mecum communicare voluisti, accepi, ad earum normam meas tabulas "revocavi, in necessarias singulorum titulorum emendationes inquisitus, quas quidem "ita eruisse mihi videor, ut ab istis observationibus nunquam ultra 2′ dissentiant." There is likewise a curious letter[m] of Grischow with respect to these very Tables, which shews the great attention that Bradley paid to the subject; but although he communicated his observations to Euler, and contributed his assistance to the editors of Halley's Tables, the greatest service he rendered to the cause was in the labour which he expended on those of Mayer. A copy of these was first put into his hands in [n]Dec. 1755, and having made more than 230 comparisons of them with his own observations, he reported favourably of them to the Admiralty in the beginning of the following February. This seemed to have revived his hopes of the lunar method's being brought into general use, and for the next four years he was most laboriously employed in deriving from his own observations every possible correction for the quantities introduced into the several equations. There is annexed to the Nautical Almanack for 1774, a detailed account of the elements used in four sets of lunar tables, which were formed at this epoch, and which are nearly connected with each other. The first is, that of Mayer's original Tables, which were never published; Bradley's is the second, derived from the improvements which he particularly employed himself in devising for the first. We have the testimony of Ferner (a Danish astronomer) to the advances which were thus made. He had been in England with Bradley, and on his return he wrote him [o]word that Clairaut and d'Alembert were unremittingly exerting themselves, each to give the greatest possible correctness to his own Tables of the moon; but he adds, that "selon ce que M. Clairaut m'a dit lui-même, "ses Tables n'atteindront pas à une telle perfection que vous m'avéz dit que celles de "M. Mayer par vos corrections ont déjà." The third set of elements was that of Gael Morris, which were likewise "[p]corrections upon Mayer's first manuscript Tables made "by the help of Dr. Bradley's observations, a few copies of which were printed, and "given by the author to his friends." These were published after the author's death, and are not commonly[q] to be met with, but they probably possessed considerable merit; for Maskelyne, though he had access to Mayer's manuscript, and must have been acquainted with Bradley's improvements, made use of them in framing some of the tables which he annexed to the British Mariner's Guide[r]. The fourth set of elements contained Mayer's last corrections for the tables, which were published in 1770.

These last Tables of Mayer were still a further advance upon Bradley's, which in consequence were never[s] printed. His were probably finished about 1760. Ferner's letter, which has just been alluded to, bears the date of Nov. 18, of that year; and in the passage which has been quoted from Mason's letter[t], he speaks of the years 59 or 60, as the time when the lunar computations were nearly gone through. This same conclusion seems to follow from Bradley's communication[u] to the Admiralty in April

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[m] P. 448. [n] P. 84. [o] P. 501.

[p] N. Alm. 1774, addit. p. 39.

[q] They are not mentioned in Lalande's Bibliog. Astron.

[r] See p. 123.

[s] They do not appear in the catalogue of MSS. in the library at Greenwich.

[t] P. lxxxix. [u] P. 86.

1760. In his first report[x], in 1756, he says, he did not find any difference[y] so great as $1'\frac{1}{2}$ between the observed longitude of the moon, and that which was computed from the tables; and in the report of April 14, 1760, he adds, that this small deficiency had been "further diminished by making alterations in some of the equations, whose true quantity "could not be determined without proper observations;" so that the difference did nowhere amount to more than a minute. This was determined from the calculations of more than 1100 observations which he had made at Greenwich with his new instruments, and Mason was afterwards employed to increase this number to 1220[z]. There are eleven books at Oxford: two, in which Bradley has computed the moon's places from Mayer's first tables so as to compare them with the earlier Greenwich observations as well as his own up to 1752, and a third in which he has done the same for observations made at the Cape, &c.: the other eight contain the comparison of the later elements with the Greenwich observations between Dec. 31, 1749, and April 3, 1760. These are by three hands in the following proportions:

Bradley	449 pages	about two computations in each page.
Mason	148	
G. Morris	60	

It appears, therefore, that his two assistants did not together execute one third of even the last part of the work for him; and Bradley most probably examined all their numbers, as there are frequent instances among them of his corrections.

Having made this advance in the improvement of the tables, he renewed his recommendation of the use of lunar distances for the discovery of the longitude at sea, which could now be found "within about half a degree or nearer." There are among the papers at Oxford a number of observations made for this purpose by Capt. Campbell and John Bradley[a], and calculations in Bradley's own hand, by which he worked out the necessary conclusions. It appears from these, that in 1757 they used Mayer's circle, which was probably the new instrument which was considered as Bradley's[b] by Capt. Campbell and the Secretary of the Admiralty. It was afterwards laid[c] aside, as he preferred a sextant of a larger radius when the limb was carefully divided by Bird; and it is probable that the order was made for this second instrument as early as April 1757[d]. The observations and computations were going on without intermission[e], although the final recommendation to the Admiralty for the adoption of the method was delayed till 1760. His representation succeeded, but it required some time before it could be established for general practice. The reductions were too difficult for common seamen, till convenient tables and more expeditious methods were devised for clearing the apparent distances from the effects of refraction and parallax.

Mayer died in 1762, worn out by his incessant labours, at the age of 39. We cannot wonder, therefore, at the same cause having a fatal effect on Bradley, who was further advanced in life at least by thirty years. Finding himself unequal to the continued fatigue of his many duties, he resigned his office of Reader in Experimental Philosophy at Oxford. These lectures “ [f]he continued with great reputation and with equal utility “ to the university, till within a year or two of his death.” It appears from the account book which has been already mentioned[g], that his last course was delivered in 1760, between Easter and the long vacation. It is gratifying to see from these records, that the number of attendants in this year was not below the average of the classes in his earlier days. He was still himself, and had not the mortification of being driven from his post by the neglect of those with whom he was connected.

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[x] Manuscript copies of both these reports are among the papers at Oxford.

[y] P. 84.

[z] Preface to Nautical Alm. for 1774. No mention is there made of G. Morris. He likewise made many lunar computations with Halley's tables.

[a] P. li. note [e].

[b] P. 493,4.

[c] P. 88.

[d] P. 495.

[e] P. 499.

The last scientific object, to which Bradley was able to pay even an imperfect attention, was the transit of Venus in 1761. Halley, as early as 1716[h], had pointed out the use which might be derived from it in determining the sun's parallax. It was a subject of peculiar interest to him, and he is said to have felt great anxiety for its being well observed. La Caille expressed [i]some fear that it was not susceptible of the precision to which he looked forward, and other astronomers may have entertained the same doubts; but the phenomenon was of rare occurrence; and as it approached, the necessary preparations for observing it were not neglected in England or on the continent.

In the minutes of the Royal Society we find the following entry: “ May 22, 1760, “ The council took into consideration a late resolution recommended to them by the “ Society; viz. the sending proper persons to proper places to observe the approaching “ transit of Venus over the sun, the several instruments that will be necessary for the “ work, and the expenses which are likely to attend it.

“ The propriety and expediency of the Society's directing this observation was agreed “ to unanimously: and the island of St. Helena was adjudged to be the most proper “ place of observation; to which Bencoolen, or rather Batavia, if it were not attended “ with uncertainty, might be added for a second observation.

“ Dr. Bradley declared that the instruments necessary were, a reflecting telescope of “ two feet, with Dollond's micrometer, Mr. Dollond's refracting telescope of ten feet, a “ quadrant of the radius of eight inches, and a clock or time piece. And Dr. Bradley “ was requested to inform himself against the next council upon what terms these several instruments might be hired for the occasion, the Society giving security for their “ restitution.”

The instruments were provided, and Maskelyne, who was then not quite eight and twenty, undertook to go to St. Helena, with Waddington for his assistant. The other observer was not so soon found, and there is a letter of Costard's among Dr. Hornsby's papers, in which he says, “ On Saturday last I met with Betts at Elsfield, and from him “ I learned that Dr. Bradley hath got a person to be sent to Bencoolen.” This was C. Mason, who had been his assistant in the observatory for[k] several years. Dixon went out with him, but unfortunately they never reached their place of destination. They sailed in a king's ship[l], (the Sea Horse,) but not many leagues from the Start, a French frigate fell in with them, and although she was beaten off, their vessel suffered so much in the action, that it was necessary to return to Plymouth to refit. After making some difficulties they sailed again, and arrived at the Cape[m] April 27; but the season being now very far advanced, they remained there for the observation of the transit. From La Caille and Maskelyne's[n] papers it appears that Bradley's cooperation was reckoned upon for the great event; but he was unable to take a share in what, had it occurred before he was quite worn out, would have been an object of his especial care. He had continued a member of the council of the Royal Society from his election into it in 1752. But the last day on which he was able to attend their meeting was 31st January 1761; and when the transit occurred in the month of May, he was so ill that he was obliged to apply to [o]Bliss to take his place at the observatory. His instructions[p] for Mason were probably the last paper of a public nature which he drew up: there is some degree of satisfaction, therefore, in being able to fix very nearly the date of it. Costard's letter was written on the 22d Sept. 1760, and the last day on which Mason's hand appears in the observation book of that year is in November. We cannot, therefore, err much in assigning Bradley's paper to the month of October. After that time he was able to do very little. Our eye looks into the Greenwich registers with feelings of interest for the traces of that hand which conveyed so much instruction to mankind, and catches occasionally the sight of it till 1st Sept. 1761, when the sun's transit was the last observation that Bradley ever entered—most probably, that he ever made. His existence continued for a few months longer, but his scientific career was closed. The splendour of the two great discoveries, to which [q]the exactness of astronomy now owes its existence, has dazzled the perception of some, and made them less sensible to the value of his other labours: but omitting any further mention of aberration and nutation, the idea of which is identified with the thought of Bradley, we must recollect that we are also indebted to him for,

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[f] Suppl. to Biog. Dict. p. 56. [g] P. xxxviii.

[h] Phil. Tr. vol. XXIX. p. 454. [i] P. 442.

[k] Lalande, in his Bibliographie Astronomique, p. 538, gives a list of assistants at Greenwich, but his account admits of some correction. It has been already mentioned, that John Bradley first took the office in the summer of 1742, and the last observation entered in his hand is in Sept. 1756. Oct. 1756, is the first time at which C. Mason's writing appears, and when he went out to observe the transit of Venus, Chas. Green took his place in November 1760. Heath, in his Royal Astronomer, (p. 39,) calls Gael Morris “ Ass^{t}. Ast. R.”; but this may be one of the author's coarse attempts at a joke. The stipend allowed for the assistant was only 26*l.* per ann.; and Mason, in his letter to Dr. Hornsby, (P. lxxxix.) alludes to this in characteristic language, where he expresses a wish that a better salary should be allowed, lest

1. A better knowledge of the motions of Jupiter's satellites, and improvement in the tables of them.

2. The orbits of several comets calculated directly from his own observations, and some of them at a time when the solution of the problem was attended with considerable difficulties.

3. Experiments on the length of the pendulum.

4. The practical reductions for refraction.

5. Considerable improvements in the Tables of the moon, and the promotion of the method of finding the longitude by lunar distances: and,

6. The renovation of the national observatory at Greenwich in 1750[r], and his invalu-

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" the hands of too many junior observers " may appear in half a revolution of the ☽'s " node."

[l] Minutes of the R. Soc.

[m] Phil. Trans. vol. LII. p. 380.

[n] Ib. p. 21, 26. [o] Ib. p. 173. [p] P. 388.

[q] Delambre Hist. de l'Ast. au xviii. siècle, p. 420.

[r] The observations which he made with his new instruments are contained in the two folio volumes published at Oxford.

able collection of meridional observations, which form a most important epoch in the progress of modern astronomy.

CHAP. XII.

Bradley's marriage—family—death—character—will.

BRADLEY, soon after his establishment at Greenwich, as Astronomer Royal, married, on the 25th of June, 1744, "Susannah, daughter of Samuel Peach, Esq. of Chalford "in Gloucestershire[a]." They had but one child, a daughter, named after her mother, (Susannah,) who was born at Greenwich in 1745. She was married at Rodmarton, Jan. 10, 1771, to her first cousin, the Rev. Samuel Peach, who took his master's degree at Hertford College, April 29, 1769, and was Rector of Compton Beauchamp, in Berkshire[b]. Bradley lost his wife in 1757, and she was buried at Minchinhampton on the 17th of May, near his mother, who died in 1747, at the advanced age of 94. He appears to have kept up a close and affectionate intercourse with the family with whom he was connected by his marriage, and he ended his days among them. The last two years of his life were spent under a melancholy depression of spirits. "His chief "distress arose from an apprehension that he should survive his rational faculties: but "this so much dreaded evil never came upon him; he still retained his understanding "till he was dying, and demonstrated the clearness of it by making a will, and settling "his affairs in the most judicious and sensible manner[c]." This instrument was executed Nov. 16, 1761; a codicil was added to it on the 3rd of the following December; and as an argument has been founded on it, a copy has been annexed to the end of these Memoirs. He only survived a few months longer; and at the end of the book in which the quadrant observations are recorded we have the following memorandum, probably written by one of his executors: "Dr. Bradley exchanged this life for a better, at Chal-"ford in Gloucestershire, July 13, 1762, aged 70."

[d]For several years before his death he frequently complained of pains, attended with other symptoms, which made him apprehend that they proceeded from gravel. On Wednesday, June 30, 1762, he rode out for the air, and on coming home complained of sufferings which only ended with his life. During his illness he was often

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[a] Supp. to Biog. Dict. p. 56.

[b] Mr. and Mrs. Peach had only one child, Harriet, who was born at Bristol, about 1775, and was married, June 10, 1800, to Thos. Edmeades, Esq., a surgeon, residing at Greenwich. She died Aug. 18, 1806; and, as she left no issue, there is no one now living who is the direct descendant of Dr. Bradley. Mrs. Peach, after her husband's death, spent the last years of her life at Greenwich, where she died, Sept. 21, 1812, and where she and her daughter are both buried. She succeeded to a handsome fortune on the death of her father; but the person who drew up her marriage settlement committed some mistake, by which a considerable portion of her property went, during her lifetime, to the child of Mr. Edmeades by a subsequent marriage. Application was made to the minister in hopes of procuring her a pension from the crown, and it was supported, though ineffectually, by a member of the royal family. She was not, however, left in penury. In her will, dated Jan. 7, 1812, she makes several bequests; and she added a codicil, in which she states, that in distributing her property she had considered herself as having nothing but a life interest in what was settled at her marriage: "I was not conscious," she says, "of being possessed of 3000*l.*," which she then goes on to dispose of.

[c] Supp. to Biog. Dict. p. 58.

[d] Phil. Trans. vol. LII. p. 636.

attacked by pains in the abdomen: his head would at intervals wander, and then would again become more cool and collected. It was the opinion of Dr. Jones, who attended him constantly in the country, as well as of Dr. Lewis and Dr. Lysons, who visited him occasionally from Oxford, that his pains were to a certain extent inflammatory; but where the inflammation was seated they could not precisely determine. It seemed often to shift its situation, and the patient himself was incapable of giving the necessary information, his weak state obliging him to signify his meaning more by signs than words, and those were not always intelligible. In consequence of these doubts the body was examined after death, and a particular account of the appearances which it presented were given by Dr. Lysons, in the Phil. Trans. He reports the case to be one which was attended by remarkable circumstances, and it appears that the disease was a general chronic inflammation of the abdominal viscera and the investing membrane[e].

His body was buried near those of his mother and wife, at Minchinhampton; and to an altar tomb in the church-yard an oval plate was fixed, with the following inscription, said to have been written by Dr. Blayney, who was afterward Regius Professor of Hebrew in the University of Oxford.

H. S. J.
Jacobus Bradley S. T. P.
Regalium Societatum
Londini, Lutetiæ, Parisiorum,
Berolini et Petropoli Sodalis, Astronomus Regius, et Astronomiæ apud
Oxonienses Professor Savilianus.
Vir in rerum physicarum scientiâ excolendâ,
præcipue vero in penitissimis arcanis
indagandis, tam felici diligentia, et sagaci
ingenio, ut quotquot ubique gentium
iisdem honestissimis studiis navabant
operam, illi omnes libenter assurgerent:
tam singulari interim modestiâ, ut
qua esset apud gravissimos judices
existimatione, ipse solus ignorâsse
videbatur. Discessit iii Id. Jul.
A. D. MDCCLXII. ætatis
LXX.

Some thieves, who made a paltry profit from stealing the brass plates from tombstones, had nearly carried this off: they had loosened all but one of the rivets, by which it had been firmly fastened in its place, when the circumstance became known to the Rev. W. Cockin, rector of the parish, and he immediately removed it into the church, where it was fixed against the walls of the chancel. "Data sunt ipsis quoque fata se-"pulcris." Archimedes owes no part of his immortality to the [f]symbols which were carved upon his tomb; and the fame of Bradley will flourish long after monumental brass shall have crumbled into dust: but there is a pious reverence attached to the earth which covered his remains, and it would have been painful to lose the indication which directed us to it. There will soon, however, be another monument to him, connected not with that doom which is common to all mankind, but with the exertion of those transcendent powers, for which his name will be honoured to the latest posterity. It is to be at Kew, with the following inscription on it, as a mark of Royal care for the honour of British science.

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[e] The generally received account of his death is very inaccurate. See Supp. to Biog. Dict. p. 58. and Hist. de l'Acad. Roy. des Sciences, 1762, p. 241. From the particulars given by Dr. Lysons, it is clear that a retention of urine was not the cause of his death, and was not produced by the diseased state of the kidneys.

On this spot,
in 1725,
The Rev. James Bradley
made the first observations
which led to his two great discoveries,
the aberration of light,
and the nutation of the earth's axis.
The telescope which he used
had been erected by Samuel Molyneux, Esq.
in a house which afterwards became a royal residence,
and was taken down in 1803.
To perpetuate the memory of so important a station,
this dial was erected on it in 1831,
by command of his most gracious Majesty
King William IV.[g]

Of Bradley's stature and figure we have no account; but he is said[h] to have "en-"joyed a great share of health, which enabled him to bear long watchings, and the most "tedious application to study, without fatigue:" and this was maintained by a degree of temperance which approached even to abstinence.

His disposition was truly amiable: "He[i] was humane, benevolent, and kind; a duti-"ful son, an indulgent husband, a tender father, and a steady friend." His own family were not in affluent circumstances, and he was invariably generous to such of his relations as needed his assistance. He was averse from the promiscuous conversation of common society; but when there was occasion, "[k]none was more ready, more open, or "more clear; no man would take more pains to adapt himself to the capacity and un-"derstanding of those he conversed with." "[l]He was always pleased to be asked ques-

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[f] Cicero Tusc. Disp. lib. V. c. 23.

[g] The offices of the old palace, and the walls which still remain about the site of it, were sufficient, with the assistance of sir William Chambers' measures, to find very nearly the situation of the stack of chimneys (P. xv.) from which the sector was suspended. But in executing his Majesty's commands, the earth was cleared away, and the old foundation of it was discovered. This was left untouched, and immediately in front of it a mass of brick-work, eight feet square, has been built, on the sides of which the railing is to be fixed which will enclose this most interesting position.

[h] Supp. to Biog. Dict. p. 58.

[i] Ibid. [k] Ibid.

[l] Supp. to Biog. Dict. p. 59.

"tions by the judicious; and even encouraged inquisitiveness in his pupils, by shewing "the greatest readiness to explain whatever was required by them." Dr. Hornsby, who knew him, used to describe him as peculiarly kind and gentle in his manners; and the portraits[m] which we have of him pourtray a countenance expressive of all these amiable qualities.

To his moral habits we have the testimony of one who knew him well. It may be seen[n] that lord Macclesfield, in language which evidently came from his heart, pronounced the most unqualified eulogium on them.

It is delightful to contemplate all this modest bearing, all these excellent qualities, and think of their being combined with those powers which made Newton declare him to be, even in early life, the best astronomer[o] in Europe; a preeminence which was afterwards universally[p] acknowledged. De Fouchy, in his Eloge[q], speaks of him as a man "d'un "tempérament vif;" but this is at variance with every thing which is related of him, and no traces whatever can be discovered of it in any of his writings. On the contrary, he was cautious and retiring. His great characteristics appear to be a most extraordinary clearness of perception, both mental and organic[r]; great accuracy in the combination of his ideas; and an inexhaustible fund of that "industry and patient thought" to which Newton, with such distinguished humility, ascribed his own great discoveries. Bradley was a man whose inventive mind did not build on the labours of others; he saw his object distinctly, and nothing diverted him from it: though years and years might be necessary for its developement, he concentrated the great powers of his intellect on the end which he had in view; nor did he relax his efforts before he had completely attained it.

He does not appear to have had much taste for mere abstract mathematics; but his papers prove that he was familiar with those branches which were most useful to him in their application. His demonstration of the [s]laws of aberration is to be taken rather as an historical curiosity, than a test of his geometrical abilities. Every one, who has investigated a new problem, must be aware that the first solution which he devises has seldom been the best. Bradley's sole object, in this case, was to ascertain the rules by which he was to calculate; he had no view to publication; if he had, he might have improved the form, and condensed the substance, of his demonstration. But in this instance, as in others, he was probably checked by a fear of submitting his thoughts to the opinion of the world. He set too low a value on his own works, and always feared lest any thing might lower his reputation. His love of [t]accuracy, likewise, acted as an impediment: those who take the most comprehensive views have the clearest knowledge of what may be deficient: he could see the improvements which were desirable, and while he had not yet attained them, he was unwilling to send out any thing to the public in a state which he considered to be imperfect. There was also another circumstance which operated against his publishing to any great extent: he certainly composed with difficulty; his writings are full of erasures; and after repeated transcriptions, his language was not always the happiest in its construction or arrangement. He was not remiss in noting what occurred to him, and making memoranda, where the words, which first occurred to him, were sufficient to register his thoughts, and recall them at any future time to his remembrance: but to dwell on the expressions which he should use, and to employ himself in polishing them for publication, seem to have been a task of irksome difficulty to him. No one writes well, who has not studied and practised it; and no one is inclined to acquire the habit, who does not enjoy some degree of facility in the execution of his purpose. It is not therefore astonishing that the voluminous historian of astronomy should have found that "Bradley n'avait presque "rien publiée[u]." Besides the tables of Jupiter's satellites and a few others of no great extent, all that he could be found to have himself given to the world is comprised in seventy-two pages of the present volume. But when every thing is considered, we have no reason for regret; if he had written more for the press, he must have done less for our information;—the facts which he established, and the discoveries which he made, are of an intrinsic and inestimable value, beyond all comparison with any dissertations, in which he might have enlarged upon them.

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[m] There are four paintings of him: one, by Hudson, in the picture gallery at Oxford, which was given to the University by his daughter, in 1769; a second, probably by the same artist, at Shirburn castle; a third, painted by Richardson, for his mother, which was given by the Rev. J. Dallaway to the Royal Society: all these three were taken when he was in middle life: a fourth, drawn at a more advanced age, had been reserved for herself by Mrs. Peach, and after her death was given by her executor, Sam. Lysons, Esq. to the Royal Society for the observatory at Greenwich. The Rev. Dan. Lysons has also a neatly finished miniature of him, drawn in Indian ink, by Ferguson. The Oxford portrait was engraved by Faber; and the original copper having been procured, the engraving was reduced so as to accommodate it to the size of the present volume. Bromley, in his Catalogue of Engraved British Portraits, mentions (p. 357.) another mezzotinto, which represents him as "sitting, "with a distant view of the observatory." A proof of this appears (p. 120.) in the sale catalogue of sir Wm. Musgrave's prints; it was bought by Mr. Tyssen, for 10*s.* 6*d.*: but when his library was sold, in 1802, the prints were lotted together, without specific descriptions, which might enable us to trace this individual specimen any further. No collector nor printseller has been met with in London who has ever seen it.

[n] P. xlviii. [o] P. xlvii.

[p] P. 430. Hist. de l'Ac. Roy. des Sciences, 1762, p. 242.

[q] Ibid. p. 241. [r] See P. 202, 206. [s] P. 287.

In the Biographia Britannica[x] it is said, that "there was not an astronomer of any "eminence in the world with whom he had not a literary correspondence." If we make every allowance for the rhetorical form of the expression, it will still be found to be a considerable exaggeration. De Fouchy[y] was nearer the truth, when he said, "Il "n'y avoit aucun astronome célèbre dans l'Europe qui ne se fît honneur d'être directe- "ment ou indirectement en correspondance avec lui." The present volume bears testimony to the fact: we find De l'Isle, La Caille, Chairaut, Grischow, courting his correspondence; Euler's [z]letter was probably intended for his notice, though addressed to Gael Morris; but there appears to have been no great inclination on Bradley's part to indulge a reciprocal desire. It may be said, that, as we have only the rough copies of his answers, which he may not in every instance have preserved, we do not know the number and extent of them. There certainly were many of which we have no remains; but still he seems to have seldom written unless some particular object required it, and there are repeated occasions[a] on which his correspondents indicate the fact of his not having answered the letters which he had received. If, therefore, his great disinclination from studied composition made him remiss in keeping up an intercourse with such distinguished characters, it is not astonishing that a number of idle letters which were addressed to him may have been wholly unattended to. Many of these have been found among his papers which were wholly unfit for publication. It might have been better if Nicholls had not printed Bowyer's[b] letter; but as he had sent it out into the world, it has been reprinted in the present collection, and may be taken as one of the better specimens of the useless applications to which Bradley, from his character and situation, was frequently exposed.

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[t] Suppl. to Biog. Dict. p. 59.

[u] Hist. de l'Astr. au xviii siècle, p. 426.

[x] Vol. II. p. 562

[y] Hist. de l'Ac. Roy. des Sciences, 1762, p. 242.

[z] P. xcvi. see also P. 432.

The following Will, of which a copy was procured from the registry of the Prerogative Court of Canterbury, was drawn up by some professional hand: the codicil was written by Bradley himself.

" IN the name of God, Amen.—I, James Bradley, of Greenwich, in the county of " Kent, D. D., do make and declare this my last Will and Testament in manner follow- " ing, (that is to say,) First, I give and bequeath unto my sister, Mary Mills, widow, " one annuity or clear yearly sum of fifteen pounds, to be paid to her during the term " of her natural life; and unto my sister, Elizabeth Jenner, widow, one annuity or clear " yearly sum of fifteen pounds, (over and besides the annuity of five pounds, for which " I have given her my bond,) to be paid to her during the term of her natural life; and " I will the said respective annuities be paid quarterly. I give and bequeath unto my " brother-in-law, John Dallaway, and to Rebecca his wife, my sister, and to my said " sisters Mary Mills and Elizabeth Jenner, and to Samuel Peach, of Chalford, in the " parish of Minchinhampton, in the said county of Gloucester, clothier, and Mary his " wife, my sister-in-law, ten pounds apiece, to buy them mourning. I give and be- " queath unto my daughter, Susanna Bradley, one hundred pounds, to buy her mourn- " ing, and for her immediate support. I give and bequeath unto my nephew, William " Dallaway, and John Peach, of Theescomb, in the said parish of Minchinhampton, son " of the said Samuel and Mary Peach, they acting in the trust in this my will reposed " in them, one hundred pounds apiece. All the rest and residue of my goods, chattels, " and personal estate whatsoever, (my funeral expenses and debts, and the said annuities " and legacies, being first paid and discharged,) I give and bequeath unto the said Wil- " liam Dallaway and John Peach, their executors, administrators, and assigns, upon " trust; nevertheless that they, my said trustees, do and shall collect, receive, and get in " the same, and, from time to time, place out and continue the same at interest on govern- " ment or real securities, and do and shall, by and out of the interest and produce " thereof, from time to time, pay and discharge the said several annuities, and also pay " unto my said daughter the sum of one hundred pounds annually, for her support and " maintenance, until she shall attain her age of twenty-one years, and also shall and will

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[a] P. 442, 3, 98, 501.

[b] P. 498.

" apply and dispose of such further part of the interest and produce of the said trust " estate to and for the support, maintenance, and education of my said daughter, as " occasions shall require, and as my said trustees in their discretion shall see fit; and " when my said daughter shall have attained her said age of twenty-one years, then " upon trust that they, my said trustees, do and shall pay the said trust monies and " estate to her my said daughter, to her own use: provided always, and my will is, " that if my said daughter shall die before she shall attain her said age of twenty-one " years, leaving one or more child or children, lawfully begotten, living at the time of " her decease, that then my said trustees shall and will pay and dispose of the said " trust monies and estate to and amongst all such children (if more than one) share " and share alike; and if but one child, to such only child solely: provided also, and my " will is, that if my said daughter shall die before she shall attain her said age of " twenty-one years, without issue, that then my said trustees do and shall pay, apply, and " dispose of the said trust monies and estate in manner following; (that is to say,) one " third part thereof to such of my own sisters as shall be then living, (if more than one,) " share and share alike, and if but one sister, to such only sister; and one other third " part thereof to and amongst all my nephews and nieces that shall be then living, (if " more than one,) share and share alike, and if there shall be but one such nephew or " niece then living, then in trust to pay such third part to such nephew or niece solely; " and one other and remaining third part thereof to the said Mary Peach, wife of the " said Samuel Peach, if she shall be then living; and in case she shall be then dead, " then to and amongst all and every the children of the said Mary Peach, by the said " Samuel Peach, (if more than one,) share and share alike; and if there shall be but " one such child, then to such only child solely. And I do hereby make and constitute " the said William Dallaway and John Peach joint executors in trust of this my will, " and guardians to my said daughter during her minority; and I do hereby revoke all " former wills by me made: provided always, and my will is, that it shall and may be " lawful to and for my said trustees to deduct and detain out of the trust monies all " their and each of their necessary costs and expenses relating to the said trust, and " that they shall not be answerable one for the other, or for the acts, receipts, or defaults " of the other, or for the involuntary loss of any of the trust monies, or for any more " money than they shall actually receive, or shall come to their hands respectively. " In witness whereof, I have to this my will, contained in two sheets of paper, to the " first sheet set my hand, and to this second and last sheet set my hand and seal, this " sixteenth day of November, in the year of our Lord one thousand seven hundred and " sixty one.

" JAMES BRADLEY."

" Signed, sealed, published and declared, by " the said James Bradley the testator, as and " for his last will and testament, in the presence " of us,

" THOMAS SELWYN.
" ANN BIBB."

"I give all my printed books to Samuel Peach, son of Samuel Peach, in my Will "named, and desire that this may be a codicil to my last Will and Testament, and taken "as part thereof, as witness my hand, this third day of December, in the year of our "Lord 1761.

"JAMES BRADLEY."

"Proved at London with a codicil, 1st September 1762, before the worshipful Andrew Coltee Ducarel, Doctor of Laws and surrogate, by the oaths of William Dallaway and John Peach, the executors, to whom administration was granted, having "been first sworn duly to administer."

⊙ The end of the axis with dot for the plumb line.
a. The Y on which it rests.
c. The screw for regulating the plumb line.
b.b. Adjusting screws.
d.d. Iron supporters.
i.k. Braſs supporter.
e. Screw & support for fastening the wood guard to the Braſs supporter i. k.
f. The Micrometer screw.
g. A screw for taking off the preſsure of the telescope from the micrometer screw when the instrument is not in use.
N.B. The telescope is put out of the vertical position in order to shew the wood guard for the plumb line. And the arc to the right of the telescope is broken off in order to shew the micrometer screw.
h. A back support on which the micrometer slides and to which it is clamped.

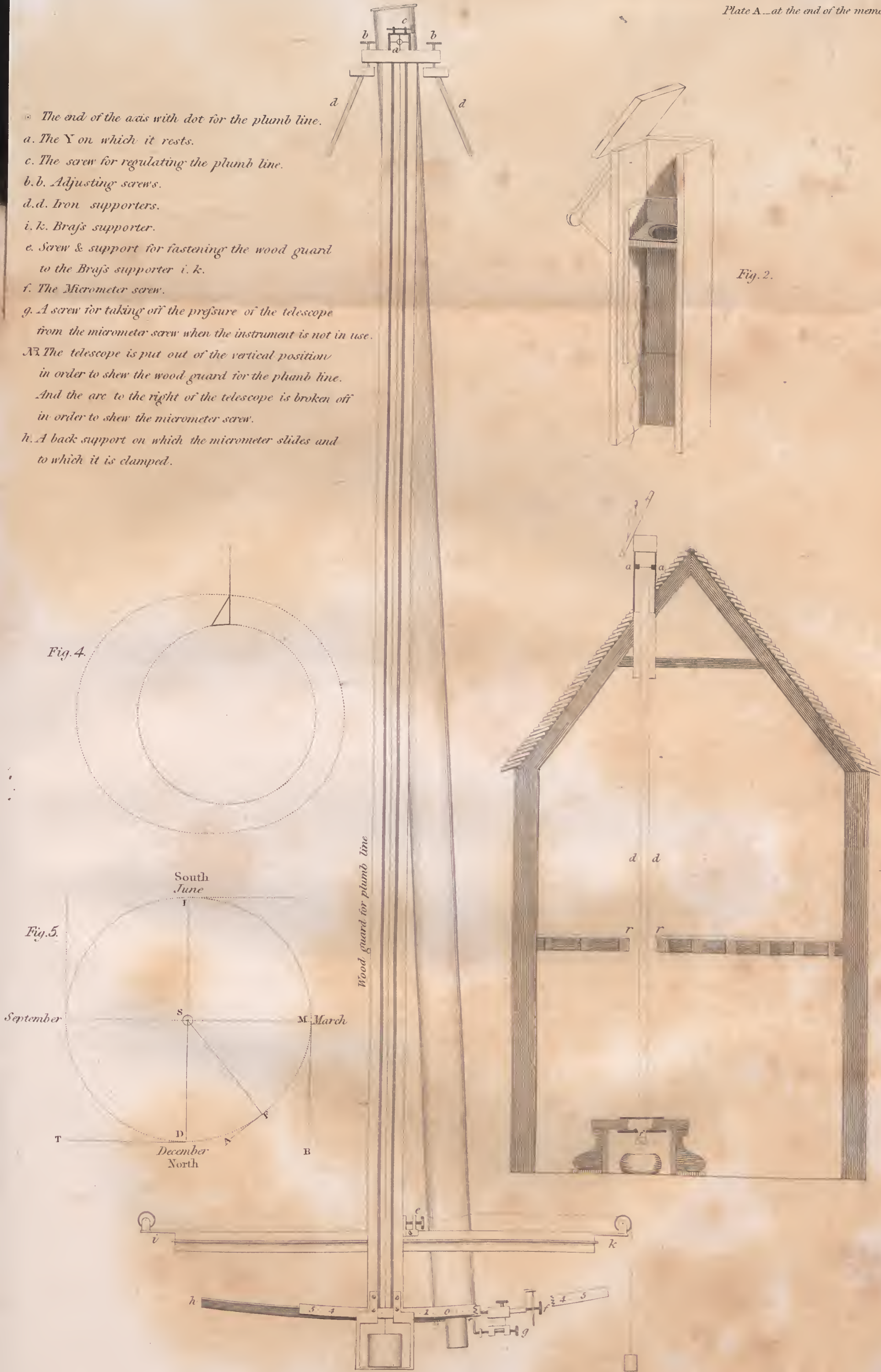


Fig. 1.

Fig. 2.

Fig. 3.

Fig. 4.

Fig. 5.

COMMUNICATIONS

TO

THE ROYAL SOCIETY.

A Letter to Dr. Edmund Halley, Astronom. Reg. &c. giving an account of a new-discovered Motion of the Fixed Stars.

[Philosophical Transactions, N°. 406. vol. XXXV. p. 637.]

SIR,

YOU having been pleased to express your satisfaction with what I had an opportunity sometime ago of telling you in conversation, concerning some observations that were making by our late worthy and ingenious friend, the honourable Samuel Molyneux, esq. and which have since been continued and repeated by myself, in order to determine the parallax of the fixed stars; I shall now beg leave to lay before you a more particular account of them.

Before I proceed to give you the history of the observations themselves, it may be proper to let you know that they were at first begun in hopes of verifying and confirming those that Dr. Hooke formerly communicated to the public, which seemed to be attended with circumstances that promised greater exactness in them, than could be expected in any other that had been made and published on the same account. And as his attempt was what principally gave rise to this, so his method in making the observations was in some measure that which Mr. Molyneux followed: for he made choice of the same star, and his instrument was constructed upon almost the same principles. But if it had not greatly exceeded the doctor's in exactness, we might yet have remained in great uncertainty as to the parallax of the fixed stars; as you will perceive upon the comparison of the two experiments.

This indeed was chiefly owing to our curious member, Mr. George Graham, to whom the lovers of astronomy are also not a little indebted for several other exact and well-contrived instruments. The necessity of such will scarce be disputed by those that have had any experience in making astronomical observations; and the inconsistency which is to be met with among different authors in their attempts to determine small angles, particularly the annual parallax of the fixed stars, may be a sufficient proof of it to others. Their disagreement indeed in this article is not now so much to be wondered at, since I doubt not but it will appear very probable, that the instruments commonly made use of by them, were liable to greater errors than many times that parallax will amount to.

The success then of this experiment evidently depending very much on the accurateness of the instrument, that was principally to be taken care of: in what manner this was done is not my present purpose to tell you; but if, from the result of the observations which I now send you, it shall be judged necessary to communicate to the curious the manner of making them, I may hereafter perhaps give them a particular description, not only of Mr. Molyneux's instrument, but also of my own, which hath since been erected for the same purpose and upon the like principles, though it is somewhat different in its construction, for a reason you will meet with presently.

Mr. Molyneux's apparatus was completed and fitted for observing about the end of November 1725, and on the third day of December following, the bright star in the head of Draco (marked γ by Bayer) was for the first time observed as it passed near the zenith, and its situation carefully taken with the instrument. The like observations were made on the 5th, 11th, and 12th days of the same month, and there appearing no material difference in the place of the star, a farther repetition of them at this season seemed needless, it being a part of the year wherein no sensible alteration of parallax in this star could soon be expected. It was chiefly therefore curiosity that tempted me (being then at Kew, where the instrument was fixed) to prepare for observing the star on December 17th, when having adjusted the instrument as usual, I perceived that it passed a little more southerly this day than when it was observed before. Not suspecting any other cause of this appearance, we first concluded that it was owing to the uncertainty of the observations, and that either this or the foregoing were not so exact as we had before supposed; for which reason we purposed to repeat the observation again, in order to determine from whence this difference proceeded; and upon doing it on December 20th, I found that the star passed still more southerly than in the former observations. This sensible alteration the more surprised us, in that it was the contrary way from what it would have been had it proceeded from an annual parallax of the star: but being now pretty well satisfied that it could not be entirely owing to the want of exactness in the observations, and having no notion of any thing else that could cause such an apparent motion as this in the star, we began to think that some change in the materials &c. of the instrument itself might have occasioned it. Under these apprehensions we remained some time, but being at length fully convinced, by several trials, of the great exactness of the instrument, and finding by the gradual increase of the star's distance from the pole, that there must be some regular cause that produced it; we took care to examine nicely, at the time of each observation, how much it was: and about the beginning of March 1726, the star was found to be 20″ more southerly than at the time of the first observation. It now indeed seemed to have arrived at its utmost limit southward, because in several trials made about this time, no sensible difference was observed in its situation. By the middle of April it appeared to be returning back again towards the north; and about the beginning of June, it passed at the same distance from the zenith as it had done in December, when it was first observed.

From the quick alteration of the star's declination about this time, (it increasing a second in three days,) it was concluded that it would now proceed northward, as it before had gone southward of its present situation; and it happened as was conjectured: for the star continued to move northward till September following, when it again became stationary, being then near 20″ more northerly than in June, and no less than 39″ more northerly than it was in March. From September the star returned towards the south, till it arrived in December to the same situation it was in at that time twelve months, allowing for the difference of declination on account of the precession of the equinox.

This was a sufficient proof that the instrument had not been the cause of this apparent motion of the star, and to find one adequate to such an effect seemed a difficulty. A nutation of the earth's axis was one of the first things that offered itself upon this occasion, but it was soon found to be insufficient; for though it might have accounted for the change of declination in γ Draconis, yet it would not at the same time agree with the phænomena in other stars; particularly in a small one almost opposite in right ascension to γ Draconis, at about the same distance from the north pole of the equator: for though this star seemed to move the same way as a nutation of the earth's axis would have made it, yet, it changing its declination but about half as much as γ Draconis in the same time, (as appeared upon comparing the observations of both made upon the same days, at different seasons of the year,) this plainly proved that the apparent motion of the stars was not occasioned by a real nutation, since, if that had been the cause, the alteration in both stars would have been near equal.

The great regularity of the observations left no room to doubt but that there was some regular cause that produced this unexpected motion, which did not depend on the uncertainty or variety of the seasons of the year. Upon comparing the observations with each other, it was discovered that in both the forementioned stars, the apparent difference of declination from the maxima was always nearly proportional to the versed sine of the sun's distance from the equinoctial points. This was an inducement to think that the cause, whatever it was, had some relation to the sun's situation with respect to those points. But not being able to frame any hypothesis at that time sufficient to solve all the phænomena, and being very desirous to search a little farther into this matter; I began to think of erecting an instrument for myself at Wansted, that, having it always at hand, I might with the more ease and certainty inquire into the laws of this new motion. The consideration likewise of being able by another instrument to confirm the truth of the observations hitherto made with Mr. Molyneux's was no small inducement to me; but the chief of all was, the opportunity I should thereby have of trying in what manner other stars were affected by the same cause, whatever it was. For Mr. Molyneux's instrument, being originally designed for observing γ Draconis, (in order, as I said before, to try whether it had any sensible parallax,) was so contrived as to be capable of but little alteration in its direction, not above seven or eight minutes of a degree: and there being few stars within half that distance from the zenith of Kew bright enough to be well observed, he could not, with his instrument, throughly examine how this cause affected stars differently situated with respect to the equinoctial and solstitial points of the ecliptic.

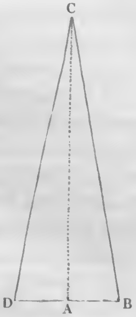
These considerations determined me; and by the contrivance and direction of the same ingenious person, Mr. Graham, my instrument was fixed up August 19, 1727. As I had no convenient place where I could make use of so long a telescope as Mr. Molyneux's, I contented myself with one of but little more than half the length of his, (viz. about 12½ feet, his being 24¼,) judging from the experience which I had already had, that this radius would be long enough to adjust the instrument to a sufficient degree of exactness; and I have had no reason since to change my opinion; for from all the trials I have yet made, I am very well satisfied, that when it is carefully rectified, its situation may be securely depended upon to half a second. As the place where my instrument was to be hung in some measure determined its radius, so did it also the length of the arch, or limb, on which the divisions were made to adjust it: for the arch could not conveniently be extended farther than to reach to about 6¼° on each side of my zenith. This indeed was sufficient, since it gave me an opportunity of making choice of several stars, very different both in magnitude and situation; there being more than two hundred inserted in the British catalogue, that may be observed with it. I needed not to have extended the limb so far, but that I was willing to take in Capella, the only star of the first magnitude that comes so near my zenith.

My instrument being fixed, I immediately began to observe such stars as I judged most proper to give me light into the cause of the motion already mentioned. There was variety enough of small ones; and not less than twelve that I could observe through all the seasons of the year; they being bright enough to be seen in the day-time, when nearest the sun. I had not been long observing, before I perceived that the notion we had before entertained of the stars being farthest north and south, when the sun was about the equinoxes, was only true of those that were near the solstitial colure: and after I had continued my observations a few months, I discovered what I then apprehended to be a general law, observed by all the stars, viz. that each of them became stationary, or was farthest north or south, when they passed over my zenith at six of the clock, either in the morning or evening. I perceived likewise, that whatever situation the stars were in with respect to the cardinal points of the ecliptic, the apparent motion of every one tended the same way, when they passed my instrument about the same hour of the day or night; for they all moved southward, while they passed in the day, and northward in the night; so that each was farthest north when it came about six of the clock in the evening, and farthest south when it came about six in the morning.

Though I have since discovered that the maxima in most of these stars do not happen exactly when they come to my instrument at those hours, yet not being able at that time to prove the contrary, and supposing that they did, I endeavoured to find out what proportion the greatest alterations of declination in different stars bore to each other; it being very evident that they did not all change their declination equally. I have before taken notice that it appeared from Mr. Molyneux's observations, that γ Draconis altered its declination about twice as much as the forementioned small star almost opposite to it; but examining the matter more particularly, I found that the greatest alteration of declination in these stars was as the sine of the latitude of each respectively. This made me suspect that there might be the like proportion between the maxima of other stars; but finding that the observations of some of them would not perfectly correspond with such an hypothesis, and not knowing whether the small difference I met with might not be owing to the uncertainty and error of the observations, I deferred the farther examination into the truth of this hypothesis, till I should be furnished with a series of observations made in all parts of the year; which might enable me not only to determine what errors the observations are liable to, or how far they may safely be depended upon; but also to judge whether there had been any sensible change in the parts of the instrument itself.

Upon these considerations I laid aside all thoughts at that time about the cause of the forementioned phænomena, hoping that I should the easier discover it, when I was better provided with proper means to determine more precisely what they were.

When the year was completed, I began to examine and compare my observations, and having pretty well satisfied myself as to the general laws of the phænomena, I then endeavoured to find out the cause of them. I was already convinced that the apparent motion of the stars was not owing to a nutation of the earth's axis. The next thing that offered itself was an alteration in the direction of the plumb-line, with which the instrument was constantly rectified; but this upon trial proved insufficient. Then I considered what refraction might do; but here also nothing satisfactory occurred. At last I conjectured that all the phænomena hitherto mentioned proceeded from the progressive motion of light and the earth's annual motion in its orbit. For I perceived that, if light was propagated in time, the apparent place of a fixed object would not be the same when the eye is at rest, as when it is moving in any other direction than that of the line passing through the eye and object; and that when the eye is moving in different directions, the apparent place of the object would be different.



I considered this matter in the following manner. I imagined C A to be a ray of light, falling perpendicularly upon the line B D; then if the eye is at rest at A, the object must appear in the direction A C, whether light be propagated in time or in an instant. But if the eye is moving from B towards A, and light is propagated in time, with a velocity that is to the velocity of the eye, as C A to B A; then light moving from C to A, whilst the eye moves from B to A, that particle of it by which the object will be discerned when the eye in its motion comes to A, is at C when the eye is at B. Joining the points B, C, I supposed the line C B to be a tube (inclined to the line B D in the angle D B C) of such a diameter as to admit of but one particle of light; then it was easy to conceive that the particle of light at C (by which the object must be seen when the eye, as it moves along, arrives at A) would pass through the tube B C, if it is inclined to B D in the angle D B C, and accompanies the eye in its motion from B to A; and that it could not come to the eye, placed behind such a tube, if it had any other inclination to the line B D. If instead of supposing C B so small a tube, we imagine it to be the axis of a larger; then, for the same reason, the particle of light at C could not pass through that axis, unless it is inclined to B D, in the angle C B D. In like manner, if the eye moved the contrary way, from D towards A, with the same velocity; then the tube must be inclined in the angle B D C. Although therefore the true or real place of an object is perpendicular to the line in which the eye is moving, yet the visible place will not be so, since that, no doubt, must be in the direction of the tube; but the difference between the true and apparent place will be (cæteris paribus) greater or less, according to the different proportion between the velocity of light and that of the eye. So that if we could suppose that light was propagated in an instant, then there would be no difference between the real and visible place of an object, although the eye were in motion; for in that case, A C being infinite with respect to A B, the angle A C B (the difference between the true and visible place) vanishes. But if light be propagated in time, (which I presume will readily be allowed by most of the philosophers of this age,) then it is evident from the foregoing considerations, that there will be always a difference between the real and visible place of an object, unless the eye is moving either directly towards or from the object. And in all cases the sine of the difference between the real and visible place of the object will be to the sine of the visible inclination of the object to the line in which the eye is moving, as the velocity of the eye to the velocity of light.

If light moved but 1000 times faster than the eye, and an object (supposed to be at an infinite distance) was really placed perpendicularly over the plane in which the eye is moving, it follows from what hath been already said, that the apparent place of such an object will be always inclined to that plane, in an angle of 89° $56'\frac{1}{2}$; so that it will constantly apppear $3'\frac{1}{2}$ from its true place, and seem so much less inclined to the plane, that way towards which the eye tends. That is, if A C is to A B (or A D) as 1000 to 1, the angle A B C will be 89° $56'\frac{1}{2}$, and A C B = $3'\frac{1}{2}$, and B C D = 2 A C B = 7′. So that, according to this supposition, the visible or apparent place of the object will be altered 7′, if the direction of the eye's motion is at one time contrary to what it is at another.

If the earth revolve round the sun annually, and the velocity of light were to the velocity of the earth's motion in its orbit (which I will at present suppose to be a circle) as 1000 to 1; then 'tis easy to conceive that a star, really placed in the very pole of the ecliptic, would, to an eye carried along with the earth, seem to change its place continually, and (neglecting the small difference on the account of the earth's diurnal revolution on its axis) would seem to describe a circle round that pole every way distant therefrom $3'\frac{1}{2}$. So that its longitude would be varied through all the points of the ecliptic every year; but its latitude would always remain the same. Its right ascension would also change, and its declination, according to the different situation of the sun in respect to the equinoctial points; and its apparent distance from the north pole of the equator would be 7′ less at the autumnal than at the vernal equinox.

The greatest alteration of the place of a star in the pole of the ecliptic (or which in effect amounts to the same, the proportion between the velocity of light and the earth's motion in its orbit) being known, it will not be difficult to find what would be the difference upon this account between the true and apparent place of any other star at any time; and, on the contrary, the difference between the true and apparent place being given, the proportion between the velocity of light and the earth's motion in its orbit may be found.

As I only observed the apparent difference of declination of the stars, I shall not now take any farther notice in what manner such a cause as I have here supposed would occasion an alteration in their apparent places in other respects; but, supposing the earth to move equally in a circle, it may be gathered, from what hath been already said, that a star which is neither in the pole nor plane of the ecliptic will seem to describe about its true place a figure insensibly different from an ellipse, whose transverse axis is at right angles to the circle of longitude passing through the star's true place, and equal to the diameter of the little circle described by a star (as was before supposed) in the pole of the ecliptic; and whose conjugate axis is to its transverse axis, as the sine of the star's latitude to the radius. And allowing that a star by its apparent motion does exactly describe such an ellipse, it will be found that if A be the angle of position, (or the angle at the star made by two great circles drawn from it through the poles of the ecliptic and equator,) and B be another angle, whose tangent is to the tangent of A as radius to the sine of the latitude of the star; then B will be equal to the difference of longitude between the sun and the star, when the true and apparent declination of the star are the same. And if the sun's longitude in the ecliptic be reckoned from that point wherein it is when this happens, then the difference between the true and apparent declination of the star (on account of the cause I am now considering) will be always as the sine of the sun's longitude from thence. It will likewise be found, that the greatest difference of declination that can be between the true and apparent place of the star, will be to the semi-transverse axis of the ellipse, (or to the semi-diameter of the little circle described by a star in the pole of the ecliptic,) as the sine of A to the sine of B.

If the star hath north latitude, the time when its true and apparent declination are the same is before the sun comes in conjunction with or opposition to it, if its longitude be in the first or last quadrant (viz. in the ascending semicircle) of the ecliptic; and after them, if in the descending semicircle; and it will appear nearest to the north pole of the equator at the time of that maximum (or when the greatest difference between the true and apparent declination happens) which precedes the sun's conjunction with the star.

These particulars being sufficient for my present purpose, I shall not detain you with the recital of any more, or with any farther explication of these. It may be time enough to enlarge more upon this head, when I give a description of the instruments, &c. if that be judged necessary to be done; and when I shall find what I now advance to be allowed of (as I flatter myself it will) as something more than a bare hypothesis. I have purposely omitted some matters of no great moment, and considered the earth as moving in a circle, and not an ellipse, to avoid too perplexed a calculus, which, after all the trouble of it, would not sensibly differ from that which I make use of, especially in those consequences which I shall at present draw from the foregoing hypothesis.

This being premised, I shall now proceed to determine from the observations what the real proportion is between the velocity of light and the velocity of the earth's annual motion in its orbit; upon supposition that the phænomena before mentioned do depend upon the causes I have here assigned. But I must first let you know, that in all the observations hereafter mentioned, I have made an allowance for the change of the star's declination on account of the precession of the equinox, upon supposition that the alteration from this cause is proportional to the time, and regular through all the parts of the year. I have deduced the real annual alteration of declination of each star from the observations themselves; and I the rather choose to depend upon them in this article, because all which I have yet made concur to prove that the stars near the equinoctial colure change their declination at this time $1'' \frac{1}{2}$ or $2''$ in a year more than they would do if the precession was only $50''$, as is now generally supposed. I have likewise met with some small varieties in the declination of other stars in different years, which do not seem to proceed from the same cause, particularly in those that are near the solstitial colure, which on the contrary have altered their declination less than they ought, if the precession was $50''$. But whether these small alterations proceed from a regular cause, or are occasioned by any change in the materials, &c. of my instrument, I am not yet able fully to determine. However, I thought it might not be amiss just to mention to you how I have endeavoured to allow for them, though the result would have been nearly the same if I had not considered them at all. What that is, I will shew, first, from the observations of γ Draconis, which was found to be $39''$ more southerly in the beginning of March than in September.

From what hath been premised, it will appear that the greatest alteration of the apparent declination of γ Draconis, on account of the successive propagation of light, would be to the diameter of the little circle which a star (as was before remarked) would seem to describe about the pole of the ecliptic, as $39''$ to $40'',4$. The half of this is the angle A C B, (as represented in the fig.) This therefore being $20'',2$, A C will be to A B, that is, the velocity of light to the velocity of the eye (which in this case may be supposed the same as the velocity of the earth's annual motion in its orbit) as 10210 to 1, from whence it would follow, that light moves or is propagated as far as from the sun to the earth in 8′ 12″.

It is well known that Mr. Roemer, who first attempted to account for an apparent inequality in the times of the eclipses of Jupiter's satellites, by the hypothesis of the progressive motion of light, supposed that it spent about 11 minutes of time in its passage from the sun to us: but it hath since been concluded by others, from the like eclipses, that it is propagated as far in about 7 minutes. The velocity of light therefore deduced from the foregoing hypothesis, is as it were a mean betwixt what had at different times been determined from the eclipses of Jupiter's satellites.

These different methods of finding the velocity of light thus agreeing in the result, we may reasonably conclude, not only that these phænomena are owing to the causes to which they have been ascribed; but also, that light is propagated (in the same medium) with the same velocity after it hath been reflected as before: for this will be the consequence, if we allow that the light of the sun is propagated with the same velocity, before it is reflected, as the light of the fixed stars. And I imagine this will scarce be questioned, if it can be made appear that the velocity of the light of all the fixed stars is equal, and that their light moves or is propagated through equal spaces in equal times, at all distances from them: both which points (as I apprehend) are sufficiently proved from the apparent alteration of the declination of stars of different lustre; for that is not sensibly different in such stars as seem near together, though they appear of very different magnitudes. And whatever their situations are, (if I proceed according to the foregoing hypothesis,) I find the same velocity of light from my observations of small stars of the fifth or sixth, as from those of the second and third magnitude, which in all probability are placed at very different distances from us. The small star, for example, before spoken of, that is almost opposite to γ Draconis, (being the 35th Camelopard. Hevelii in Mr. Flamsteed's Catalogue,) was 19″ more northerly about the beginning of March than in September. Whence I conclude, according to my hypothesis, that the diameter of the little circle described by a star in the pole of the ecliptic would be 40″, 2.

The last star of the Great Bear's tail of the second magnitude (marked η by Bayer) was 36″ more southerly about the middle of January than in July. Hence the maximum or greatest alteration of declination of a star in the pole of the ecliptic would be 40″, 4, exactly the same as was before found from the observations of γ Draconis.

The star of the fifth magnitude in the head of Perseus, marked τ by Bayer, was 25″ more northerly about the end of December than on the 29th of July following: hence the maximum would be 41″. This star is not bright enough to be seen as it passes over my zenith about the end of June, when it should be, according to the hypothesis, farthest south. But because I can more certainly depend upon the greatest alteration of declination of those stars, which I have frequently observed about the times when they become stationary, with respect to the motion I am now considering; I will set down a few more instances of such, from which you may be able to judge how near it may be possible from these observations to determine with what velocity light is propagated.

α Persei Bayeri was 23″ more northerly at the beginning of January than in July; hence the maximum would be 40″, 2. α Cassiopeæ was 34″ more northerly about the end of December than in June; hence the maximum would be 40″, 8. β Draconis was 39″ more northerly in the beginning of September than in March; hence the maximum would be 40″, 2. Capella was about 16″ more southerly in August than in February; hence the maximum would be about 40″. But this star being farther from my zenith than those I have before made use of, I cannot so well depend upon my observations of it, as of the others; because I meet with some small alterations of its declination that do not seem to proceed from the cause I am now considering.

I have compared the observations of several other stars, and they all conspire to prove that the maximum is about 40″ or 41″. I will therefore suppose that it is $40'' \frac{1}{2}$, or (which amounts to the same) that light moves or is propagated as far as from the sun to us in 8′ 13″. The near agreement which I met with among my observations induces me to think, that the maximum (as I have here fixed it) cannot differ so much as a second from the truth, and therefore it is probable that the time which light spends in passing from the sun to us may be determined by these observations within 5″ or 10″; which is such a degree of exactness as we can never hope to attain from the eclipses of Jupiter's satellites.

Having thus found the maximum, or what the greatest alteration of declination would be in a star placed in the pole of the ecliptic, I will now deduce from it (according to the foregoing hypothesis) the alteration of declination in one or two stars, at such times as they were actually observed, in order to see how the hypothesis will correspond with the phænomena through all the parts of the year.

It would be too tedious to set down the whole series of my observations; I will therefore make choice only of such as are most proper for my present purpose, and will begin with those of γ Draconis.

This star appeared farthest north about September 7th, 1727, as it ought to have done according to my hypothesis. The following table shews how much more southerly the star was found to be by observation in several parts of the year, and likewise how much more southerly it ought to be according to the hypothesis.

	The difference of declination by observation.	The difference of declination by the hypothesis.		The difference of declination by observation.	The difference of declination by the hypothesis.
	″	″		″	″
1727. October 20	4½	4½	1728. April 6	36	36½
November 17	11½	12	May 6	28½	29½
December 6	17½	18½	June 5	18½	20
——— 28	25	26	——— 15	17½	17
1728. January 24	34	34	July 3	11½	11½
February 10	38	37	August 2	4	4
March 7	39	39	September 6	0	0
——— 24	37	38			

Hence it appears that the hypothesis corresponds with the observations of this star through all parts of the year; for the small differences between them seem to arise from the uncertainty of the observations, which is occasioned (as I imagine) chiefly by the tremulous or undulating motion of the air, and of the vapours in it; which causes the stars sometimes to dance to and fro, so much that it is difficult to judge when they are exactly on the middle of the wire that is fixed in the common focus of the glasses of the telescope.

I must confess to you, that the agreement of the observations with each other, as well as with the hypothesis, is much greater than I expected to find before I had compared them; and it may possibly be thought to be too great by those who have been used to astronomical observations, and know how difficult it is to make such as are in all respects exact. But if it would be any satisfaction to such persons, (till I have an opportunity of describing my instrument and the manner of using it,) I could assure them, that in above seventy observations which I made of this star in a year, there is but one (and that is noted as very dubious on account of clouds) which differs from the foregoing hypothesis more than 2″, and this does not differ 3″.

This therefore being the fact, I cannot but think it very probable that the phænomena proceed from the cause I have assigned, since the foregoing observations make it sufficiently evident, that the effect of the real cause, whatever it is, varies in this star, in the same proportion that it ought according to the hypothesis.

But lest γ Draconis may be thought not so proper to shew the proportion in which the apparent alteration of declination is increased or diminished, as those stars which lie near the equinoctial colure; I will give you also the comparison between the hypothesis and the observations of η *Ursæ majoris,* that which was farthest south about the 17th day of January 1728, agreeable to the hypothesis. The following table shews how much more northerly it was found by observation in several parts of the year, and also what the difference should have been according to the hypothesis.

	The difference of declination by observation.	The difference of declination by the hypothesis.		The difference of declination by observation.	The difference of declination by the hypothesis.
	″	″		″	″
1727. September 14	29½	28½	1728. April 16	18½	18
——— 24	24½	25½	May 5	24½	23½
October 16	19½	19½	June 5	32	31½
November 11	11½	10½	——— 25	35	34½
December 14	4	3	July 17	36	36
1728. February 17	2	3	August 2	35	35½
March 21	11½	10½	September 20	26½	26½

I find upon examination that the hypothesis agrees altogether as exactly with the observations of this star as the former; for in about fifty that were made of it in a year, I do not meet with a difference of so much as 2″, except in one, which is marked as doubtful on account of the undulation of the air, &c. and this does not differ 3″ from the hypothesis.

The agreement between the hypothesis and the observations of this star is the more to be regarded, since it proves that the alteration of declination, on account of the precession of the equinox, is (as I before supposed) regular through all parts of the year; so far at least as not to occasion a difference great enough to be discovered with this instrument. It likewise proves the other part of my former supposition, viz. that the annual alteration of declination in stars near the equinoctial colure, is at this time greater than a precession of 50″ would occasion: for this star was 20″ more southerly in September 1728, than in September 1727, that is, about 2″ more than it would have been if the precession was but 50″. But I may hereafter, perhaps, be better able to determine this point, from my observations of those stars that lie near the equinoctial colure, at about the same distance from the north pole of the equator, and nearly opposite in right ascension.

I think it needless to give you the comparison between the hypothesis and the observations of any more stars; since the agreement in the foregoing is a kind of demonstration, (whether it be allowed that I have discovered the real cause of the phænomena or not,) that the hypothesis gives at least the true law of the variation of declination in different stars, with respect to their different situations and aspects with the sun. And if this is the case, it must be granted that the parallax of the fixed stars is much smaller than hath been hitherto supposed by those who have pretended to deduce it from their observations. I believe that I may venture to say, that in either of the two stars last mentioned it does not amount to 2″. I am of opinion, that if it were 1″ I should have perceived it, in the great number of observations that I made, especially of γ Draconis; which agreeing with the hypothesis (without allowing any thing for parallax) nearly as well when the sun was in conjunction with, as in opposition to, this star, it seems very probable that the parallax of it is not so great as one single second; and consequently that it is above 400,000 times farther from us than the sun.

There appearing therefore after all no sensible parallax in the fixed stars, the Anti-Copernicans have still room on that account to object against the motion of the earth; and they may have (if they please) a much greater objection against the hypothesis by which I have endeavoured to solve the forementioned phænomena, by denying the progressive motion of light, as well as that of the earth.

But as I do not apprehend that either of these postulates will be denied me by the generality of the astronomers and philosophers of the present age; so I shall not doubt of obtaining their assent to the consequences which I have deduced from them, if they are such as have the approbation of so great a judge of them as yourself. I am,

Sir, your most obedient
humble servant,
J. BRADLEY.

POSTSCRIPT.

AS to the observations of Dr. Hooke, I must own to you, that before Mr. Molyneux's instrument was erected, I had no small opinion of their correctness; the length of his telescope, and the care he pretends to have taken in making them exact, having been strong inducements with me to think them so. And since I have been convinced both from Mr. Molyneux's observations and my own, that the Doctor's are really very far from being either exact or agreeable to the phænomena, I am greatly at a loss how to account for it. I cannot well conceive that an instrument of the length of thirty-six feet, constructed in the manner he describes his, could have been liable to an error of near 30″, (which was doubtless the case,) if rectified with so much care as he represents.

The observations of Mr. Flamsteed of the different distances of the pole star from the pole at different times of the year, which were through mistake looked upon by some as a proof of the annual parallax of it, seem to have been made with much greater care than those of Dr. Hooke. For though they do not all exactly correspond with each other, yet from the whole Mr. Flamsteed concluded that the star was 35″, 40″, or 45″ nearer the pole in December than in May or July: and according to my hypothesis it ought to appear 40″ nearer in December than in June. The agreement therefore of the observations with the hypothesis is greater than could reasonably be expected, considering the radius of the instrument, and the manner in which it was constructed.

A Letter to the Rt. Hon. George Earl of Macclesfield, concerning an apparent Motion observed in some of the Fixed Stars.

[Philosophical Transactions, N°. 485. vol. XLV. p. 1.]

MY LORD,

THE great exactness with which instruments are now constructed hath enabled the astronomers of the present age to discover several changes in the positions of the heavenly bodies, which, by reason of their smallness, had escaped the notice of their predecessors. And although the causes of such motions have always subsisted, yet philosophers had not so fully considered what the effects of those known causes would be, as to demonstrate *a priori* the phænomena they might produce; so that theory itself is here, as well as in many other cases, indebted to practice, for the discovery of some of its most elegant deductions. This points out to us the great advantage of cultivating this, as well as every other branch of natural knowledge, by a regular series of observations and experiments.

The progress of astronomy indeed has always been found to have so great a dependence upon accurate observations, that till such were made, it advanced but slowly: for the first considerable improvements that it received, in point of theory, were owing to the renowned Tycho Brahe; who, far exceeding those that had gone before him in the exactness of his observations, enabled the sagacious Kepler to find out some of the principal laws relating to the motion of the heavenly bodies. The invention of telescopes and pendulum-clocks affording proper means of still farther improving the praxis of astronomy; and these being also soon succeeded by the wonderful discoveries made by our great Newton, as to its theory; the science, in both respects, had acquired such extraordinary advancement, that future ages seemed to have little room left for making any great improvements. But, in fact, we find the case to be very different; for, as we advance in the means of making more nice inquiries, new points generally offer themselves that demand our attention. The subject of my present letter to your lordship is a proof of the truth of this remark; for as soon as I had discovered the cause, and settled the laws of the aberrations of the fixed stars, arising from the motion of light, &c. whereof I gave an account in N°. 406.[a] of the Philosophical Transactions, my attention was again excited by another new phænomenon, viz. an annual change of declination in some of the fixed stars; which appeared to be sensibly greater about that time, than a precession of the equinoctial points of 50″ in a year would have occasioned. The quantity of the difference, though small in itself, was rendered perceptible, through the exactness of my instrument, even in the first year of my observations; but being then at a loss to guess from what cause that greater change of declination proceeded, I endeavoured to allow for it in my computations, by making use of the observed annual difference, as mentioned in p. 652[b] of the same Transaction.

From that time to the present I have continued to make observations at Wansted, as opportunity offered, with a view of discovering the laws and cause of this phænomenon: for, by the favour of my very kind and worthy friend Matthew Wymondesold, esq. my instrument has remained where it was first erected; so that I have been able, without any interruption, which the removal of it to another place would have occasioned, to proceed on with my intended series of observations for the space of twenty years: a term somewhat exceeding the whole period of the changes that happen in this phænomenon.

When I shall mention the small quantity of the deviation which the stars are subject to from the cause that I have been so long searching after, I am apprehensive that I may incur the censure of some persons, for having spent so much time in the pursuit of such a seeming trifle: but the candid lovers of science will, I hope, make due allowance for that natural ardour with which the mind is urged on towards the discovery of truths, in themselves perhaps of small moment, were it not that they tend to illustrate others of greater use.

The apparent motions of the heavenly bodies are so complicated, and affected by such a variety of causes, that in many cases it is extremely difficult to assign to each its due share of influence; or distinctly to point out what part of the motion is the effect of one cause, and what of another: and whilst the joint effects of all are only attended to, great irregularities and seeming inconsistencies frequently occur; whereas, when we are able to allot to each particular cause its proper effect, harmony and uniformity usually ensue.

[a] See above, p. 1.

[b] See above, p. 10.

Such seeming irregularities being also blended with the unavoidable errors which astronomical observations must be always liable to, as well from the imperfection of our senses, as of the instruments that we make use of, have often very much perplexed those who have attempted to solve the phænomena: and till means are discovered whereby we can separate and distinguish the particular part of the whole motion that is owing to each respective cause, it will be impossible to be well assured of the truth of any solution. For these reasons we generally find, that the more exact the instruments are that we make use of, and the more regular the series of observations is that we take, the sooner we are enabled to discover the cause of any new phænomenon. For when we can be well assured of the limits wherein the errors of the observations are contained; and have reduced them within as narrow bounds as possible, by the perfection of the instruments which we employ; we need not hesitate to ascribe such apparent changes, as manifestly exceed those limits, to some other causes. Upon these accounts it is incumbent upon the practical astronomer to set out at first with the examination of the correctness of his instruments, and to be assured that they are sufficiently exact for the use he intends to make of them; or at least he should know within what limits their errors are confined.

This practice has, in an eminent manner, been lately recommended by your lordship's noble example; who having, out of a singular regard for the science of astronomy, erected an observatory, and furnished it with as complete an apparatus of instruments as our best artists could contrive, would not fully rely on their exactness, till their divisions had undergone the strictest reexamination; whereby they are probably now rendered as perfect in their kind as any extant, or as human skill can at present produce.

The lovers of this science in general cannot but acknowledge their obligations to your lordship on this account; but I find myself more particularly bound to do it, since, by means of your lordship's most accurate observations, I have been enabled to settle some principal elements; which I could not at present otherwise have done for want of an instrument at the Royal Observatory proper for that purpose: for the large mural quadrant, which is there fixed to observe objects lying southward of the zenith, however perfect an instrument it may be in itself, is not alone sufficient to determine, with proper exactness, either the latitude of the Observatory, or the quantity of refraction corresponding to different altitudes: for it being too heavy to be conveniently removed, and the room wherein it is placed being too small to admit of its being turned to the opposite side of the wall, whereon it now hangs, I cannot, by actual observations of the circumpolar stars, settle those necessary points, and therefore have endeavoured to do it by comparing my own with your lordship's observations; and until this defect in the apparatus belonging to the Royal Observatory be removed, we must be indebted to your lordship for the knowledge of its true situation.

A mind, intent upon the pursuit of any kind of knowledge, will always be agreeably entertained with what can supply the most proper means of attaining it: such to the practical astronomer are exact and well-contrived instruments; and I reflect with pleasure on the opportunities I have enjoyed of cultivating an acquaintance and friendship with the person that, of all others, has most contributed to their improvement. For I am sensible that, if my own endeavours have, in any respect, been effectual to the advancement of astronomy, it has principally been owing to the advice and assistance given me by our worthy member Mr. George Graham; whose great skill and judgment in mechanics, joined with a complete and practical knowledge of the uses of astronomical instruments, enable him to contrive and execute them in the most perfect manner.

The gentlemen of the Royal Academy of Sciences, to whom we are so highly obliged for their exact admeasurement of the quantity of a degree under the arctic circle, have already given the world very convincing proofs of his care and abilities in those respects; and the particular delineation, which they have lately published, of the several parts of the sector, which he made for them, hath now rendered it needless to enter upon any minute description of mine at Wansted; both being constructed upon the same principles, and differing in their component parts chiefly on account of the different purposes for which they were intended.

As mine was originally designed to take only the differences of the zenith distances of stars, in the various seasons of the year, without any view of discovering their true places, I had no occasion to know exactly what point on the limb corresponded to the true zenith; and therefore no provision was made in my sector for the changing of its situation for that purpose. Neither was it necessary that the divisions or points on the arc should be set off, with the utmost accuracy, equidistant from each other; because, when I observe any particular star, the same spot or point being first bisected by the plumb-line, and then the screw of the micrometer turned until the star appears upon the middle of the wire that is fixed in the common focus of the glasses of the telescope, I can thereby collect how far the star is from that given point at the time of observation; and afterwards, by comparing together the several observations that are made of it, I am able to discover what apparent change has happened. The quantity of the visible alteration in the position of the stars being expressed by revolutions and parts of a revolution of the screw of the micrometer, I endeavoured to determine, with great care, the true angle answering thereto; and, after various trials, I thoroughly satisfied myself, both of the equality of the threads of the screw, and of the precise number of seconds corresponding to them.

But although these points could be settled with great certainty, I was nevertheless obliged to make one supposition; which perhaps to some persons may seem of too great moment in the present inquiry to be admitted without an evident proof from facts and experiments. For I suppose that the line of collimation of my telescope has invariably preserved the same direction, with respect to the divisions upon the arc, during the whole course of my observations. And indeed it was on account of the objections which might have been raised against such a postulate, that I thought it necessary to continue my series of observations for so many years, before I published the conclusions, which I shall at present endeavour to draw from them.

Whoever compares the result of the several trials that have been made by the gentlemen of the Academy of Sciences for determining the zenith point of their sector, since their return from the north, will, I presume, allow that mine is not an unreasonable or precarious supposition; since it is evident, from their observations, that the line of collimation of that instrument underwent no sensible change in its direction during the space of more than a whole year; although it was several times taken down, and set up again in different and remote places; whereas mine hath always remained suspended in the same place.

But besides such a strong argument for the probability of the truth of my supposition, I have the satisfaction of finding it actually verified by the observations themselves; which plainly prove, that at the end of the full period of the deviations which I am going to mention, the stars are found to have the same positions by the instrument as they ought to have, supposing the line of collimation to have continued unaltered from the time when I first began to observe.

I have already taken notice in what manner this phænomenon discovered itself to me at the end of my first year's observations, viz. by a greater apparent change of declination in the stars near the equinoctial colure, than could arise from a precession of 50″ in a year; the mean quantity now usually allowed by astronomers. But there appearing at the same time an effect of a quite contrary nature in some stars near the solstitial colure, which seemed to alter their declination less than a precession of 50″ required, I was thereby convinced that all the phænomena, in the different stars, could not be accounted for, merely by supposing that I had assumed a wrong quantity for the precession of the equinoctial points.

At first I had a suspicion that some of these small apparent alterations in the places of the stars might possibly be occasioned by a change in the materials, or in the position of the parts of my sector: but upon considering how firmly the arc, on which the divisions or points are made, is fastened to the plate wherein the wire is fixed that lies in the focus of the object-glass, I saw no reason to apprehend that any change could have happened in the position of that wire and those points. The suspension therefore of the plummet being the most likely cause from whence I conceived any uncertainty could arise, and the wire of which had been broken three or four times in the first year of my observations, I attempted to examine whether part of the forementioned apparent motions might not have been owing to the different plumb-lines that had been made use of. In order to determine this, I adjusted a particular point of the arc to the plumb-line with all the exactness I could; and then, taking off the old wire, I immediately hung on another, with which the same spot was again compared. I repeated the experiment three or four times, and thereby fully satisfied myself that no sensible error could arise from the use of different plumb-lines; since the various adjustments of the same point agreed with each other within less than half a second.

Having then, from such trials, sufficient reason to conclude that these second unexpected deviations of the stars were not owing to any imperfection of my instrument, after I had settled the laws of the aberrations arising from the motion of light, &c. I judged it proper to continue my observations of the same stars; hoping that, by a regular and longer series of them, carried on through several succeeding years, I might at length be enabled to discover the real cause of such apparent inconsistencies.

As I resided chiefly at Wansted, after my sector was erected there in the year 1727, till the beginning of May 1732, when I removed from thence to Oxford, I had, during my abode at Wansted, frequent opportunities of repeating my observations; and thereby discovered so many particulars relating to these phænomena, that I began to guess what was the real cause of them.

It appeared from my observations, that, during this interval of time, some of the stars near the solstitial colure had changed their declinations 9″ or 10″ less than a precession of 50″ would have produced; and, at the same time, that others near the equinoctial colure had altered theirs about the same quantity more than a like precession would have occasioned: the north pole of the equator seeming to have approached the stars, which come to the meridian with the sun, about the vernal equinox and the winter solstice; and to have receded from those which come to the meridian with the sun about the autumnal equinox and the summer solstice.

When I considered these circumstances, and the situation of the ascending node of the moon's orbit, at the time when I first began my observations; I suspected that the moon's action upon the equatorial parts of the earth might produce these effects: for if the precession of the equinox be, according to sir Isaac Newton's principles, caused by the actions of the sun and moon upon those parts, the plane of the moon's orbit being at one time above ten degrees more inclined to the plane of the equator than at another, it was reasonable to conclude, that the part of the whole annual precession, which arises from her action, would in different years be varied in its quantity; whereas the plane of the ecliptic, wherein the sun appears, keeping always nearly the same inclination to the equator, that part of the precession which is owing to the sun's action may be the same every year: and from hence it would follow, that although the mean annual precession, proceeding from the joint actions of the sun and moon, were 50″, yet the apparent annual precession might sometimes exceed, and sometimes fall short of that mean quantity, according to the various situations of the nodes of the moon's orbit.

In the year 1727, when my instrument was first set up, the moon's ascending node was near the beginning of Aries; and consequently her orbit was as much inclined to the equator as it can at any time be; and then the apparent annual precession was found by my first year's observations to be greater than the mean: which proved that the stars near the equinoctial colure, whose declinations are most of all affected by the precession, had changed theirs above a tenth part more than a precession of 50″ would have caused. The succeeding year's observations proved the same thing; and in three or four years' time the difference became so considerable, as to leave no room to suspect that it was owing to any imperfection either of the instrument or observations.

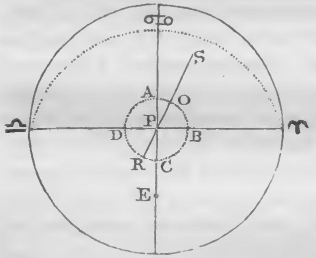
But some of the stars which I had observed, that were near the solstitial colure, having appeared to move, during the same time, in a manner contrary to what they ought to have done, by an increase in the precession; and the deviations in them being as remarkable as in the others, I perceived that something more than a mere change in the quantity of the precession would be requisite to solve this part of the phænomenon. Upon comparing my observations of stars near the solstitial colure that were almost opposite to each other in right ascension, I found that they were equally affected by this cause; for whilst γ Draconis appeared to have moved northward, the small star, which is the 35th Camelopardali Hevel. in the British Catalogue, seemed to have gone as much towards the south: which shewed that this apparent motion in both those stars might proceed from a nutation in the earth's axis; whereas the comparison of my observations of the same stars formerly enabled me to draw a different conclusion with respect to the cause of the annual aberrations arising from the motion of light. For the apparent alteration in γ Draconis, from that cause, being as great again as in the other small star, proved that that phænomenon did not proceed from a nutation of the earth's axis; as, on the contrary, this may. Upon making the like comparison between the observations of other stars that lie nearly opposite in right ascension, whatever their situations were with respect to the cardinal points of the equator, it appeared that their change of declination was nearly equal, but contrary, and such as a nutation or motion of the earth's axis would effect.

The moon's ascending node being got back towards the beginning of Capricorn in the year 1732, the stars near the equinoctial colure appeared about that time to change their declinations no more than a precession of 50″ required; whilst some of those near the solstitial colure altered theirs above 2″ in a year less than they ought. Soon after, I perceived the annual change of declination of the former to be diminished, so as to become less than 50″ of precession would cause; and it continued to diminish till the year 1736, when the moon's ascending node was about the beginning of Libra, and her orbit had the least inclination to the equator. But by this time some of the stars near the solstitial colure had altered their declinations 18″ less, since the year 1727, than they ought to have done from a precession of 50″. For γ Draconis, which in those nine years should have gone about 8″ more southerly, was observed in 1736 to appear 10″ more northerly than it did in the year 1727.

As this appearance in γ Draconis indicated a diminution of the inclination of the earth's axis to the plane of the ecliptic; and as several astronomers have supposed that inclination to diminish regularly; if this phænomenon depended upon such a cause, and amounted to 18″ in nine years, the obliquity of the ecliptic would at that rate alter a whole minute in thirty years; which is much faster than any observations before made would allow. I had reason therefore to think that some part of this motion at the least, if not the whole, was owing to the moon's action upon the equatorial parts of the earth; which I conceived might cause a libratory motion of the earth's axis. But as I was unable to judge, from only nine years' observations, whether the axis would entirely recover the same position that it had in the year 1727, I found it necessary to continue my observations through a whole period of the moon's nodes; at the end of which I had the satisfaction to see that the stars returned into the same positions again, as if there had been no alteration at all in the inclination of the earth's axis; which fully convinced me that I had guessed rightly as to the cause of the phænomena. This circumstance proves likewise, that if there be a gradual diminution of the obliquity of the ecliptic, it does not arise only from an alteration in the position of the earth's axis, but rather from some change in the plane of the ecliptic itself; because the stars, at the end of the period of the moon's nodes, appeared in the same places, with respect to the equator, as they ought to have done, if the earth's axis had retained the same inclination to an invariable plane.

During the course of my observations, our ingenious secretary of the Royal Society, Mr. John Machin, being employed in considering the theory of gravity, and its consequences with regard to the celestial motions, I acquainted him with the phænomena that I had observed, and at the same time mentioned what I suspected to be the cause of them. He soon after sent me a table, containing the quantity of the annual precession in the various positions of the moon's nodes, as also the corresponding nutations of the earth's axis; which was computed upon the supposition that the mean annual precession is 50″, and that the whole is governed by the pole of the moon's orbit only: and therefore he imagined that the numbers in the table would be too large, as in fact they were found to be. But it appeared that the changes which I had observed, both in the annual precession and nutation, kept the same law, as to increasing and decreasing, with the numbers of his table. Those were calculated upon the supposition that the pole of the equator, during a period of the moon's nodes, moved round in the periphery of a little circle whose centre was 23° 29′ distant from the pole of the ecliptic, having itself also an angular motion of 50″ in a year about the same pole: the north pole of the equator was conceived to be in that part of the small circle which is farthest from the north pole of the ecliptic at the time when the moon's ascending node is in the beginning of Aries, and in the opposite point of it when the same node is in Libra.

Such a hypothesis will account for an acceleration and retardation of the annual precession, as also for a nutation of the earth's axis; and if the diameter of the little circle be supposed equal to 18″, which is the whole quantity of the nutation, as collected from my observation of γ Draconis, then all the phænomena in the several stars which I observed will be very nearly solved by it.



Let P represent the mean place of the pole of the equator, about which point, as a centre, suppose the true pole to move in the circle A B C D, whose diameter is 18″. Let E be the pole of the ecliptic, and E P be equal to the mean distance between the poles of the equator and ecliptic; and suppose the true pole of the equator to be at A when the moon's ascending node is in the beginning of Aries, and at B when the node gets back to Capricorn, and at C when the same node is in Libra, at which time the north pole of the equator being nearer the north pole of the ecliptic by the whole diameter of the little circle A C equal to 18″, the obliquity of the ecliptic will then be so much less than it was when the moon's ascending node was in Aries. The point P is supposed to move round E with an equal retrograde motion, answerable to the mean precession arising from the joint actions of the sun and moon, while the true pole of the equator moves round P in the circumference A B C D, with a retrograde motion likewise, in a period of the moon's nodes, or of eighteen years and seven months. By this means, when the moon's ascending node is in Aries, and the true pole of the equator at A is moving from A towards B, it will approach the stars that come to the meridian with the sun about the vernal equinox, and recede from those that come with the sun near the autumnal equinox, faster than the mean pole P does. So that while the moon's node goes back from Aries to Capricorn, the apparent precession will seem so much greater than the mean, as to cause the stars that lie in the equinoctial colure to have altered their declination 9″ in about four years and eight months more than the mean precession would do; and in the same time the north pole of the equator will seem to have approached the stars that come to the meridian with the sun at our winter solstice about 9″, and to have receded as much from those that come with the sun at the summer solstice.

Thus the phænomena before recited are in general conformable to this hypothesis. But to be more particular; let S be the place of a star, P S the circle of declination passing through it, representing its distance from the mean pole, and ♈ P S its mean right ascension. Then if O and R be the points where the circle of declination cuts the little circle A B C D, the true pole will be nearest that star at O, and farthest from it at R; the whole difference amounting to 18″, or to the diameter of the little circle. As the true pole of the equator is supposed to be at A when the moon's ascending node is in Aries, and at B when that node gets back to Capricorn, and the angular motion of the true pole about P is likewise supposed equal to that of the moon's node about E, or the pole of the ecliptic; since in these cases the true pole of the equator is 90 degrees before the moon's ascending node, it must be so in all others.

When the true pole is at A, it will be at the same distance from the stars that lie in the equinoctial colure as the mean pole P is; for I neglect at present the case of such stars as are very near the pole of the equator; and as the true pole recedes back from A towards B, it will approach the stars that lie in that part of the colure represented by P ♈, and recede from those that lie in P ♎; not indeed with an equable motion, but in the ratio of the sine of the distance of the moon's node from the beginning of Aries. For if the node be supposed to have gone backwards from Aries 30°, or to the beginning of Pisces; the point, which represents the place of the true pole will in the mean time have moved in the little circle through an arc, as A O, of 30° likewise; and would therefore in effect have approached the stars that lie in the equinoctial colure P ♈, and have receded from those that lie in P ♎, $4''\frac{1}{2}$; which is the sine of 30° to the radius A P. For if a perpendicular fall from O upon P A, it may be conceived as part of a great circle passing through the true pole and any star lying in the equinoctial colure. Now the same proportion that holds in these stars will obtain likewise in all others; and from hence we may collect a general rule for finding how much nearer or farther any particular star is to or from the mean pole in any given position of the moon's node.

For if from the right ascension of the star we substract the distance of the moon's ascending node from Aries, then the radius will be to the sine of the remainder, as 9″ is to the number of seconds that the star is nearer to or farther from the true than the mean pole. When that remainder is less than 180°, the star is nearer to the true than to the mean pole; and the contrary when it is greater than 180°.

This motion of the true pole about the mean at P will also produce a change in the right ascensions of the stars, and in the places of the equinoctial points, as well as in the obliquity of the ecliptic; and the quantity of the equations, in either of these cases, may be easily computed for any given position of the moon's nodes. But as it may be needless to dwell longer on the explication of the hypothesis, I shall now proceed to shew its correspondency with the phænomena relating to the alterations of the polar distances of some of the stars which I have observed, by laying before your lordship the observations themselves, together with the computations that are necessary, in order to form a right judgment about the cause of these appearances.

I have endeavoured to find the exact quantity of the mean precession of the equinoctial points, by comparing my own observations made at Greenwich, with those of Tycho Brahe and others, which I judged to be most proper for that purpose. But as many of the stars which I compared gave a different quantity, I shall assume the mean result, which gives a precession of one degree in seventy-one years and an half; this agreeing very well likewise with my observations that were taken at Wansted. The numbers in the following tables, which express the change of declination in each star, are computed upon the supposition that the mean obliquity of the ecliptic was 23° 28′ 30″, and that it continued the same during the whole course of my observations. And as the moon's ascending node was in the beginning of Aries about the 27th day of March 1727, I have reduced the place of each star to that time; by allowing the proper change of declination from that day to the day of each respective observation.

It being also necessary to make an allowance for the aberrations of light, I have again examined my observations that were most proper to determine the transverse axis of the ellipsis which each star seems to describe, and have found it to be nearest to 40″; which number I therefore make use of in the following computations.

The divisions or points upon the limb of my sector are placed five minutes of a degree from each other, and are numbered so as to shew the polar distances nearly, the true polar distance exceeding that which is shewn by the instrument about 1′ 35″. When I first began to observe, I generally made use of that point on the limb which was nearest to the star's polar distance, without regarding whether it was more northerly or more southerly than the star; but as it sometimes happened that the original point, with which I at first compared the star, became in process of time pretty remote from it, I afterwards brought the plummet to another point that was nearer to it, and carefully examined what number of revolutions of the screw of the micrometer, &c. corresponded to the distance between the different points that I had made use of: by which means I was able to reduce all the observations of the same star to the same point, without supposing the several divisions to be accurately 5′ asunder.

I have expressed the distance of each star from the point of the arc with which it was compared in seconds of a degree and tenth parts of a second, exactly as it was collected from the observations; although I am sensible that the observations themselves are liable to an error of more than a whole second; because I meet with some that have been made within two or three days of each other that differ 2″, even when they are not marked as defective in any respect.

It would be too tedious to set down the whole number of the observations that I have made; and therefore I shall give only enough of them to shew their correspondency with the forementioned hypothesis in the several years wherein any were made of the stars here recited. When several observations have been taken of the same star within a few days of each other, I have either set down the mean result, or that observation which best agreed with it. I have likewise commonly chosen those that were made near the same season of the year, in such stars as gave me the opportunity of making that choice, particularly in γ Draconis, which was generally observed about the end of August or the beginning of September; that being the usual time when I went to Wansted on purpose to observe both that and also some of the stars in the Great Bear. But the weather proving cloudy at that season, in the year 1744, prevented my making a single observation either of γ Draconis or any other star while I was there, which is the cause of one vacancy in a series of twenty succeeding years, wherein that particular star had been observed. Such stars as were either not visible in the day-time, towards the beginning of September, or came at such hours of the night as would have incommoded the family of the house wherein the instrument is fixed, were but seldom observed after I went to reside at Oxford; which is the reason why the series of observations of those is so imperfect, as sometimes to leave a chasm for several years together. But notwithstanding this, I doubt not but upon the whole they will be found sufficient to satisfy your lordship of the general correspondency between the hypothesis and the phænomena in the several stars, however different their situations are with respect to the cardinal points of the equator.

As I made more observations of γ Draconis than of any other star, and it being likewise very near the zenith of Wansted, I will begin with the recital of some of them. The point upon the limb with which this star was compared was 38° 25′ from the north pole of the equator, according to the numbers of the arc of my sector. The first column in the following table shews the year and the day of the month when the observations were made; the next gives the number of seconds that the star was found to be south of 38°.25′; the third contains the alterations of the polar distance which the mean precession, at the rate of one degree in $71\frac{1}{2}$ years, would cause in this star from the 27th day of March 1727 to the day on which the observation was taken; the fourth shews the aberrations of light; the fifth, the equations arising from the forementioned hypothesis; and the sixth gives the mean distance of the star from the point with which it was compared, found by collecting the several numbers, according to their signs, in the third, fourth, and fifth columns, and applying them to the observed distances contained in the second.

If the observations had been perfectly exact, and the several equations of their due quantity, then all the numbers in the last column would have been equal; but since they differ a little from one another, if the mean of all be taken, and the extremes are compared with it, we shall find no greater difference than what may be supposed to arise from the uncertainty of the observations themselves; it nowhere amounting to more than $1''\frac{1}{2}$. The hypothesis therefore seems in this star to agree extremely well with the observations here set down; but as I had made above 300 of it, I took the trouble of comparing each of them with the hypothesis; and although it might have been expected that in so large a number some great errors would have occurred, yet there are very few, viz. only eleven that differ from the mean of these so much as 2″, and not one that differs so much as 3″. This surprising agreement, therefore, in so long a series of observations, taken in all the various seasons of the year, as well as in the different positions of the moon's nodes, seems to be a sufficient proof of the truth both of this hypothesis and also of that which I formerly advanced, relating to the aberrations of light; since the polar distance in this star may differ in certain circumstances almost a minute, viz. 56″½, if the corrections resulting from both these hypotheses are neglected; whereas, when those equations are rightly applied, the mean place of the star comes out the same, as nearly as can be reasonably expected.

γ Draconis.	South of 38°. 25′.	Precession.	Aberration.	Nutation.	Mean Dist.
	″	″	″	″	″
1727. September 3	70.5	− 0.4	+19.2	−8.9	80.4
1728. March 18	108.7	− 0.8	−19.0	−8.6	80.3
September 6	70.2	− 1.2	+19.3	−8.1	80.2
1729. March 6	108.3	− 1.6	−19.3	−7.4	80.0
September 8	69.4	− 2.1	+19.3	−6.4	80.2
1730. September 8	68.0	− 2.9	+19.3	−3.9	80.5
1731. September 8	66.0	− 3.8	+19.3	−1.0	80.5
1732. September 6	64.3	− 4.6	+19.3	+2.0	81.0
1733. August 29	60.8	− 5.4	+19.0	+4.8	79.2
1734. August 11	62.3	− 6.2	+16.9	+6.9	79.9
1735. September 10	60.0	− 7.1	+19.3	+7.9	80.1
1736. September 9	59.3	− 8.0	+19.3	+9.0	79.6
1737. September 6	60.8	− 8.8	+19.3	+8.5	79.8
1738. September 13	62.0	− 9.6	+19.3	+7.0	78.7
1739. September 2	66.6	−10.5	+19.2	+4.7	80.0
1740. September 5	70.8	−11.3	+19.3	+1.9	80.7
1741. September 2	75.4	−12.1	+19.2	−1.1	81.4
1742. September 5	76.7	−12.9	+19.3	−4.0	79.1
1743. September 2	81.6	−13.7	+19.1	−6.4	80.6
1745. September 3	86.3	−15.4	+19.2	−8.9	81.2
1746. September 17	86.5	−16.2	+19.2	−8.7	80.8
1747. September 2	86.1	−17.0	+19.2	−7.6	80.7

I made about 250 observations of β Draconis, which I find correspond as well with the hypothesis as those of γ; but since the positions of both these stars, in respect to the solstitial colure, differ but little from each other, it will be needless to set down the observations of β. I shall therefore proceed to lay before your lordship some observations of a small star that is almost opposite to γ Draconis in right ascension, being the 35th Camelopardali Hevel. in the British Catalogue. Mr. Flamsteed, indeed, has not given the right ascension of this star; but that being necessary to be known in order to compute the change of its declination arising from the precession of the equinox, I compared the time of its transit over the meridian with that of some other stars near the same parallel, whereby I found that its right ascension was 85° 54′$\frac{1}{2}$ at the beginning of the year 1737.

This small star was compared with the same point of the limb of my sector as γ Draconis; and the second column in the following table shews how many seconds it was found to be south of that point at the time of each respective observation. The other columns contain, as in the foregoing table, the equations that are necessary to find what its mean distance from the same point would have been on the 27th day of March 1727, which is exhibited in the last column. The whole number of my observations of this star did not much exceed forty, the greatest part of which were made before the year 1730; in some of the following years none were taken, and only a single one in any other, except in 1739. However, their correspondency seems sufficient to evince the truth of the hypothesis: for if the mean of these contained in the table be taken, not one among the rest of the observations will differ from it more than 2″.

35th Camelopard. Hevelii.	South of 38°. 25′.	Precession.	Aberration.	Nutation.	Mean Dist. south.
	″	″	″	″	″
1727. October 20	73.6	+ 0.9	−6.7	+8.9	76.7
1728. January 12	60.8	1.2	+6.1	8.8	76.9
March 1	57.8	1.4	+9.4	8.7	77.3
September 26	75.2	2.3	−8.8	8.1	76.8
1729. February 26	56.4	2.8	+9.4	7.6	76.2
1730. March 3	57.8	4.4	9.4	5.4	77.0
1731. February 5	59.1	5.6	8.5	+3.0	76.2
1733. January 31	64.1	8.7	8.2	−2.9	78.1
1738. December 30	61.8	17.2	4.3	6.5	76.8
1739. February 4	56.9	17.3	8.5	6.3	76.4
1740. January 20	56.0	18.6	7.0	−4.0	77.6
1747. February 27	32.3	28.5	9.4	+8.4	78.6

The observations of the foregoing stars are the most proper to prove the change of the inclination of the earth's axis to the plane of the ecliptic; those which follow will shew in what manner the stars that lie near the equinoctial colure are affected, as well as others that are differently situated with respect to the cardinal points of the equator. Some of these stars are indeed more remote from the zenith than I would have chosen, if there had been others, of equal lustre, in more proper positions; because experience has long since taught me, that the observations of such stars as lie near the zenith do generally agree best with one another, and are therefore the fittest to prove the truth of any hypothesis. I shall begin with those near the vernal equinox. α Cassiopeæ was compared with the point marked 34° 55′; and at first was found to be more southerly, but afterwards became more northerly than that point, as in the following table; the last column of which shews its mean distance south of that point on the 27th of March 1727. The observation of the 23d day of December, in the year 1738, differs 3″ from the mean of the others, as does also another that was taken five days after this, neither of which being marked as uncertain, I judged it proper to insert one of them; although they give the mean place of the star near two seconds more northerly than any other, in a series of above 100; all of which correspond with the mean of these here recited within less than 2″, excepting two, that give the star's mean distance almost 3″ more southerly; but these last mentioned are marked as dubious; and indeed they appear to have been bad, by comparing them with several others that were made near the same time, from which they differ almost 2″.

α Cassiopeæ.	South of 34° 55′.	Precession.	Aberration.	Nutation.	Mean Dist. south.
	″	″	″	″	″
1727. September 9	55.0	+ 9.0	+ 2.2	+2.4	68.6
1728. September 17	30.8	29.4	+ 4.6	5.2	70.0
1729. June 8	35.7	43.8	−16.3	6.8	70.0
December 3	N. 9.4	53.5	+16.5	7.7	68.3
1730. June 11	S. 13.8	64.0	−16.2	8.4	70.0
December 9	N. 30.8	73.8	+16.3	8.8	68.1
1732. January 8	N. 49.2	95.4	12.9	8.9	68.0
1733. January 21	64.8	116.0	+10.0	7.9	69.1
1734. June 13	62.8	143.8	−16.1	5.0	69.9
December 11	105.4	153.7	+16.2	+3.7	68.2
1738. December 23	176.3	234.0	+15.2	−7.2	65.7
1740. June 2	169.1	262.8	−16.5	−8.9	68.3
1747. February 27	332.3	397.0	+ 0.2	+4.7	69.6

Although I have taken no observation of τ Persei since the 22d day of January 1740, yet, as this star is very near the zenith, and a sufficient number were made about the times when the equation resulting from the hypothesis was at its maximum, I judged it proper to insert some of them in the next table; the last column of which shews how much the star's mean distance was south of 38° 20′ on the 27th day of March 1727. Among near 60 observations I meet with two only that differ from the mean of these so much as 2″, and those differ almost as much from the mean of others that were taken near the same time; so that the hypothesis seems to correspond in general with the observations of this star as well as with either of the foregoing.

τ Persei.	South of 38° 20′.	Precession.	Aberration.	Nutation.	Mean Dist. south.
	″	″	″	″	″
1727. September 16	60.1	+ 7.4	− 3.2	+ 6.7	71.0
December 29	39.7	11.9	+ 12.9	7.2	71.7
1728. December 21	22.5	27.2	12.8	8.7	71.2
1729. December 2	S. 9.2	42.0	11.5	9.0	71.7
1731. January 3	N. 8.2	59.0	12.8	8.3	71.9
1732. January 8	22.0	74.8	12.7	6.7	72.2
1733. January 21	34.6	91.0	11.7	+ 4.3	72.4
1738. December 23	117.0	183.4	12.8	− 9.0	70.2
1740. January 22	132.5	200.2	11.7	8.6	70.8

After the last recited observations, it may perhaps seem needless to add those of α Persei, which is farther from the zenith; but, however, as this star lies very nearly at an equal distance from the equinoctial and solstitial colures, and the series of observations of it is somewhat more complete than that of τ Persei, I shall insert one at least for each year wherein it has been observed, whereby it may appear, that the hypothesis solves the phænomena of stars in this situation as exactly as in others: for if a mean be taken of the numbers in the last column of the following table, which expresses the mean distance of the star south of 41° 5′ on March 27th, 1727, it will agree within two seconds with every one of 80 observations that have been made of this star.

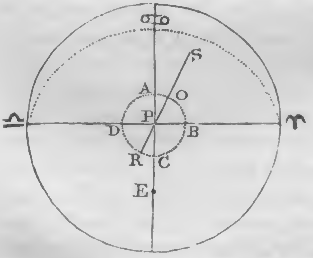
α Persei.	South of 41° 5′.	Precession.	Aberration.	Nutation.	Mean Dist. south.
	″	″	″	″	″
1727. December 29	79.4	+ 10.5	+ 11.4	+ 7.9	109.2
1728. April 7	87.5	14.3	− 0.8	8.2	109.2
July 5	94.6	17.7	− 11.4	8.5	109.4
December 13	65.7	23.8	+ 10.6	8.8	108.9
1729. December 3	53.4	37.2	9.7	8.9	109.2
1731. January 3	38.6	52.3	11.4	7.8	110.1
1732. January 8	26.8	66.2	+ 11.4	+ 5.9	110.3
1734. July 11	S. 21.3	101.0	− 11.4	− 1.1	109.8
1738. December 24	N. 56.3	162.6	+ 11.2	9.0	108.5
1740. January 21	71.8	177.4	10.9	− 8.2	108.3
1747. February 27	182.5	275.4	6.6	+ 8.5	108.0

Having already given examples of stars lying near both the solstices and the vernal equinox, I shall now add the observations of one that is not far from the autumnal equinox, viz. η Ursæ Majoris, the brightest star in that part of the heavens, which approaches the zenith of Wansted within a degree; and which, by reason of its lustre and position, gave me the opportunity of making my series of observations of it more complete than of many others. This star was compared with the point marked 39° 15′, and was south of it, as in the following table; wherein your lordship will see that the observations of the years 1740 and 1741 give the polar distances 3″ greater than the mean of the other years. Had there been only a single observation taken in either of those years, part of this apparent difference might have been supposed to arise from their uncertainty; but as there were eight observations taken within a week, either before or after the 3rd day of June 1740, which agree well with each other; and three were made within twenty days in September 1741, which likewise corresponded with each other, I am inclined to think that the forementioned differences must be owing to something else besides the error of the observations. This phænomenon therefore may deserve the consideration of those gentlemen, who have employed their time in making computations relating to the quantity of the effects which the power of gravity may on various occasions produce. For I suspect that the position of the moon's apogee, as well as of her nodes, has some relation to the apparent motions of the stars that I am now speaking of.

My series of observations of several stars abound, of late years, with so many and long interruptions, that I cannot pretend to determine this point; but probably the differences before taken notice of in the observations of α Cassiopeæ, and some others that I have found likewise among the observations of other stars, that are not here recited, may be owing to such a cause; which, although it should not have any large share of influence, may yet, in certain circumstances, discover a defect in a hypothesis that pays no regard at all to it. But whether these differences do arise from the cause already hinted at, or whether they proceed from any defect of the hypothesis itself in any other respect, it will not be very material in point of practice; since that hypothesis, as it was before laid down, appears to be sufficient to solve all the phænomena to as great a degree of exactness as we can in general hope or expect to make observations. For if I take the mean of all the numbers of the last column of the following table for η Ursæ Majoris, and compare it with any one of 164 observations that were taken of it, the difference will not exceed three seconds.

η Ursæ Majoris.	South of 39° 15′.	Precession.	Aberration.	Nutation.	Mean Dist. south.
	″	″	″	″	″
1727. October 17	153.3	− 10.2	+ 1.0	−5.2	138.9
1728. January 24	176.4	15.2	−17.6	5.8	137.8
July 17	150.8	23.9	+17.8	6.9	137.8
October 11	170.6	28.2	+ 2.6	7.3	137.7
1729. January 16	196.6	33.1	−17.8	7.8	137.9
July 21	170.4	42.4	+17.8	8.4	137.4
1730. July 19	189.6	60.6	+17.8	9.0	137.8
December 28	232.4	68.7	−16.7	8.9	138.1
1731. September 18	218.1	81.9	+ 9.4	8.4	137.2
1732. January 10	250.7	87.7	−17.7	8.0	137.3
April 13	238.7	92.3	− 0.8	7.7	137.9
1734. July 11	255.7	133.3	+17.6	−2.3	137.7
1735. September 10	280.8	154.6	+11.4	+1.2	138.8
1736. September 8	294.7	172.8	11.6	4.1	137.6
1737. July 3	303.0	187.8	17.2	6.1	138.5
1738. June 29	319.0	205.8	16.8	7.9	137.9
1739. April 25	348.0	220.8	2.5	8.8	138.5
1740. June 3	360.3	241.1	12.8	8.9	140.9
1741. September 23	390.9	265.0	7.9	+7.4	141.2
1745. September 5	466.7	337.1	12.4	−3.3	138.7
1746. September 20	492.0	356.2	8.8	5.9	138.7
1747. September 2	507.2	373.5	13.2	7.8	139.1

You may perceive, my lord, by inspecting the tables which contain the observations of α Cassiopeæ and η Ursæ Majoris, that the greatest differences that occur therein may be diminished, by supposing the true pole of the equator to move round the point P in an ellipsis, instead of a circle. For if the transverse axis, lying in the direction A C, be 18″, and the conjugate, as D B, be about 16″, the equations resulting from such an hypothesis will make the numbers in the last columns agree with each other nearer than as they now stand. But since this would not entirely remove the inequalities in all the positions of the moon's nodes, I shall refer the more accurate determination



of the locus of the true pole to theory; and at present only give the equations for the precession of the equinoctial points, and the obliquity of the ecliptic, as also the real quantity of the annual precession, to every fifth degree of the place of the moon's ascending node, in the following tables, just as they result from the hypothesis as at first laid down; it appearing, from what has already been remarked, that these will be sufficiently exact for practice in all cases.

The Equation of the Equinoctial Points.

☽'s ☊ from ♈	Sig. O.	I.	II.	Subt.
	Sig. VI.	VII.	VIII.	Add.
°	″	″	″	°
0	0.0	11.3	19.6	30
5	2.0	13.0	20.5	25
10	3.9	14.5	21.2	20
15	5.8	16.0	21.8	15
20	7.7	17.3	22.2	10
25	9.6	18.5	22.5	5
30	11.3	19.6	22.6	0
Subt.	Sig. V.	IV.	III.	☽'s ☊ from ♈
Add.	Sig. XI.	X.	IX.	

The Equation of the Obliquity of the Ecliptic.

☽'s ☊ from ♈	Sig. O.	I.	II.	Add.
	Sig. VI.	VII.	VIII.	Subt.
°	″	″	″	°
0	9.0	7.8	4.5	30
5	9.0	7.4	3.8	25
10	8.9	6.9	3.1	20
15	8.7	6.4	2.3	15
20	8.5	5.8	1.6	10
25	8.2	5.2	0.8	5
30	7.8	4.5	0.0	0
Subt.	Sig. V.	IV.	III.	☽'s ☊ from ♈
Add.	Sig. XI.	X.	IX.	

The Annual Precession of the Equinoctial Points.

☽'s ☊ from ♈	Sig. O.	I.	II.	III.	IV.	V.	
°	″	″	″	″	″	″	°
0	58.0	57.0	54.2	50.3	46.5	43.7	30
5	57.9	56.6	53.6	49.7	46.0	43.4	25
10	57.9	56.2	53.0	49.0	45.5	43.2	20
15	57.7	55.7	52.3	48.4	45.0	43.0	15
20	57.5	55.2	51.7	47.7	44.5	42.8	10
25	57.3	54.7	51.0	47.1	44.1	42.8	5
30	57.0	54.2	50.3	46.5	43.7	42.7	0
	Sig. XI.	X.	IX.	VIII.	VII.	VI.	☽'s ☊ from ♈

Sir Isaac Newton, in determining the quantity of the annual precession from the theory of gravity, upon supposition that the equatorial is to the polar diameter of the earth as 230 is to 229, finds the sun's action sufficient to produce a precession of $9''\frac{1}{8}$ only; and collecting from the tides the proportion between the sun's force and the moon's to be as 1 to $4\frac{1}{2}$, he settles the mean precession resulting from their joint actions at 50″. But since the difference between the polar and equatorial diameter is found, by the late observations of the gentlemen of the Academy of Sciences, to be greater than what sir Isaac had computed it to be; the precession arising from the sun's action must likewise be greater than what he has stated it at, nearly in the same proportion. From whence it will follow, that the moon's force must bear a less proportion to the sun's than $4\frac{1}{2}$ to 1; and perhaps the phænomena which I have now been giving an account of will supply the best data for settling this matter.

As I apprehend that the observations already set down will be judged sufficient to prove in general the truth of the hypothesis before advanced, I shall not trouble your lordship with the recital of more that I made of stars lying at greater distances from the zenith; those not being so proper, for the reason before mentioned, to establish the point that I had chiefly in view. But as it may perhaps be of some use to future astronomers to know what were the mean differences of declination, at a given time, between some stars that lie nearly opposite to one another in right ascension, and not far from either of the colures, I shall set down the result of the comparison of a few that differ so little in declination, that I could determine the quantity of that difference with great certainty.

By the mean of 64 observations that were made of α Cassiopeæ before the end of the year 1728, I collect, after allowing for the precession, aberration, and nutation, as in the foregoing tables, that the mean distance of this star was 68″ 7 south of 34° 55′ on the 27th day of March 1727. By a like comparison of 40 observations taken of γ Ursæ Majoris during the same interval of time, I find this star was, at the same time, 39″6 south of 34° 45′. I carefully measured, with the screw of the micrometer, the distance between the points with which these stars were compared, and found them to be 9′ 59″ from each other, or one second less than they ought to have been. Hence it follows, that the mean difference of declination between these two stars was 10′ 28″ 1 on the 27th day of March 1727.

By the mean of 65 observations that were taken of β Cassiopeæ before the end of the year 1728, this star was 25″ 8 north of 32° 20′ on the 27th day of March 1727; and by the mean of 52 observations, ε Ursæ Majoris was 87″ 6 south of 32° 30′ at the same time. The distance between these points was found to be 9′ 59″ 3; from whence it follows, that the mean difference of declination between these two stars was 11′ 52″ 7 on March 27th 1727.

By the mean of 100 observations taken before the end of the year 1728, the mean distance of γ Draconis was 79″ 8 south of 38° 25′ on March 27th, 1727; and by the mean of 35 observations, the 35th Camelopard. Hevel. was south of the same spot 76″ 4. So that the mean polar distance of γ Draconis was only 3″ 4 greater than that of the 35th Camelopard. Hevel.; but as the equation for the nutation in both these stars was then near the maximum, and to be applied with contrary signs, the apparent polar distance of γ Draconis was 21″ 4 greater on the 27th day of March 1727.

The differences of the polar distances of the stars, as here set down, may be presumed, both on account of the radius of the instrument and the number of observations, to be very exactly determined, to the time when the moon's ascending node was at the beginning of Aries; and if a like comparison be hereafter made, of observations taken of the same stars, near the same position of the moon's nodes, future astronomers may be enabled to settle the quantity of the mean precession of the equinox, so far as it affects the declination of these stars, with great certainty; and they may likewise discover, by means of the stars near the solstitial colure, from what cause the apparent change in the obliquity of the ecliptic really proceeds, if the mean obliquity be found to diminish gradually.

The forementioned points indeed can be settled only on the supposition that the angular distances of these stars do continue always the same, or that they have no real motion in themselves, but are at rest in absolute space. A supposition which, though usually made by astronomers, nevertheless seems to be founded on too uncertain principles to be admitted in all cases. For if a judgment may be formed, with regard to this matter, from the result of the comparison of our best modern observations with such as were formerly made, with any tolerable degree of exactness; there appears to have been a real change in the position of some of the fixed stars with respect to each other; and such as seems independent of any motion in our own system, and can only be referred to some motion in the stars themselves. Arcturus affords a strong proof of this; for if its present declination be compared with its place, as determined either by Tycho or Flamsteed, the difference will be found to be much greater than what can be suspected to arise from the uncertainty of their observations.

It is reasonable to expect that other instances of the like kind must also occur among the great number of the visible stars, because their relative positions may be altered by various means. For if our own solar system be conceived to change its place with respect to absolute space, this might, in process of time, occasion an apparent change in the angular distances of the fixed stars, and, in such a case, the places of the nearest stars being more affected than of those that are very remote, their relative positions might seem to alter, though the stars themselves were really immoveable. And on the other hand, if our own system be at rest, and any of the stars really in motion, this might likewise vary their apparent positions; and the more so, the nearer they are to us, or the swifter their motions are, or the more proper the direction of the motion is, to be rendered perceptible by us. Since then the relative places of the stars may be changed from such a variety of causes, considering that amazing distance at which it is certain some of them are placed, it may require the observations of many ages to determine the laws of the apparent changes even of a single star: much more difficult therefore must it be to settle the laws relating to all the most remarkable stars.

When the causes which affect the places of all the stars in general are known, such as the precession, aberration, and nutation, it may be of singular use to examine nicely the relative situations of particular stars; and especially of those of the greatest lustre, which it may be presumed lie nearest to us, and may therefore be subject to more sensible changes, either from their own motion, or from that of our system. And if at the same time that the brighter stars are compared with each other, we likewise determine the relative positions of some of the smallest that appear near them, whose places can be ascertained with sufficient exactness, we may perhaps be able to judge to what cause the change, if any be observable, is owing. The uncertainty that we are at present under, with respect to the degree of accuracy wherewith former astronomers could observe, makes us unable to determine several things relating to the subject that I am now speaking of; but the improvements which have of late years been made, in the methods of taking the places of the heavenly bodies, are so great, that a few years may hereafter be sufficient to settle some points, which cannot now be settled by comparing even the earliest observations with those of the present age.

It were to be wished therefore, that such persons as are provided with proper instruments would attempt to determine, with great care, the present relative positions of several of the principal stars in various parts of the heavens; especially of those that are least affected by refraction: that cause having many times so uncertain an influence on the places of objects that are very remote from the zenith, that, wherever it is concerned, the conclusions deduced from observations that are much affected by it will always remain doubtful, and too precarious, in many cases, to be relied upon.

The advantages arising from different persons attempting to settle the same points of astronomy near the same time are so much the greater, as a concurrence in the result would remove all suspicion of incorrectness in the instruments made use of. For which reason I esteem the curious apparatus at Shirburn Castle, and the observations there taken, as a most valuable criterion, whereby I may judge of the accuracy of those that are made at the Royal Observatory; and, as a lover of science, I cannot but wish that our nation abounded with more frequent examples of persons, of like rank and ability with your lordship, equally desirous of promoting this, as well as every other branch of natural knowledge, that tends to the honour and benefit of our country.

But were the patrons of arts and sciences ever so numerous, the subject of my present letter is of such a nature, as must direct me to beg leave to address it to the Earl of Macclesfield; not only as a most competent judge of it, but as the sole person in this nation that hath instruments proper to examine into the truth of the facts here related. And it is a particular satisfaction to me, that, after so long an attendance upon these phænomena, I am allowed the honour of transmitting the account of them to the public through your lordship's hands: as it gives me at the same time an opportunity of professing the grateful sense I shall ever retain, both of the signal favours which I formerly received from the noble earl your father, and of the many recent obligations conferred by yourself upon,

My lord,
Your lordship's most obedient
humble servant,
J. BRADLEY.

Greenwich, Dec. 31,
1747.

Observations upon the Comet that appeared in the months of October, November, and December, 1723.

[Philosophical Transactions, N°. 382. vol. XXXIII. p. 41.]

THE small comet which was seen in these parts of Europe in the months of October, November, and December, 1723, was first observed in England by Dr. Halley, on October 9, between seven and eight of the clock in the evening; it appearing then to the naked eye not much unlike a star of the third magnitude. Looking at it through a telescope, he saw some small telescopical stars near it, whose situation he noted, together with the comet's, in order to see which way it tended. About nine, he again viewed the comet, and found it considerably moved from its former station, having now passed a small star which at the time of the first observation was on the other side of it. Comparing the two situations of the comet together, he perceived that its apparent motion at that time was about eight or nine minutes in an hour, in a direction towards Sagitta; and that the comet passed very near, if it did not wholly eclipse, the forementioned small star, whose place he afterwards found to be in ♒ 7° 22′ 15″ with 5° 2′ north latitude. From the situation of the comet at the time of the first observation, he judged that it was in conjunction with the star at 8h. 5′ equal time.—Note that the equal, and not the apparent time, is likewise made use of in all the following observations.

The next day he was pleased to communicate to me the substance of what he had observed, whereby I was enabled, the night following, to see the comet at Wansted. The clouds hindered me from observing it in the manner that I had designed; but I had time enough to measure its distance (with a micrometer in a telescope of seven foot) from a star in Aquarius, marked ε by Bayer. At 6h. 21′ the observed distance between this star and the comet was 1° 13′ 53″, and a great circle passing through the star and comet made an angle with the vertical circle of 60°¼. The comet was more southerly and westerly than the star. By this observation, the comet preceded the star in right ascension 1° 3′ 50″, being 39′ 5″ more southerly; so that the comet's right ascension was 307° 6′ 40″, and its declination 11° 8′ 15″ south.

The place of ε here assumed is according to the British Catalogue, as are also the places of the other stars hereafter mentioned, from which the comet was observed. The right ascensions and declinations which are here set down of several small stars that are not in that Catalogue, were determined by observing the differences of right ascension and declination between those small stars and others that were in the Catalogue, and had nearly the same declinations.

The same evening, at 7h. 3′, a small star that was more easterly than the comet, and had about the same declination with it, was distant from it 35′ 40″. About the same time another small star, that had nearly the same right ascension with the comet, but was more southerly, was distant from it 39′ 58″. The places of these two stars I have not yet observed.

The next night proved cloudy, so that I could not see the comet again till October 12, when (the air being very serene and clear) we had an opportunity of comparing it with two or three small stars that were near it; my uncle, the reverend Mr. Pound, assisting in this and most of the following nights' observations.

At 7h. 22′, a small star, whose right ascension was found 304° 40′ 23″, and its declination 7° 8′ 22″ south, preceded the comet in right ascension 26′ 21″, being 10′ 42″ more northerly. Hence the comet's right ascension was 305° 6′ 44″, and its declination 7° 19′ 4″ south.

At 8h. 50′, the comet was in the same parallel of declination with another small star, whose right ascension was found 305° 9′ 56″, and its declination 7° 13′ 20″ south, and preceded the said star 6′ 20″ in right ascension. Hence the right ascension of the comet was 305° 3′ 36″, and its declination 7° 13′ 20″ south. These observations were made with a telescope of fifteen foot, furnished with a micrometer, as were also all those of the following nights.

The next night, October 13, 6h. 58′, the comet followed a small star 4′ 10″ in right ascension, being more northerly than the star 11′ 45″. The clouds did not permit us to observe the place of this star; but its right ascension must be about 304° 22′, and its declination 6° 10′ south.

October 14, the comet was near two stars which are the 66th and 67th of Aquila and Antinous in the British Catalogue, and at 8h. 57′ it followed the southermost of them 20′ 37″ in right ascension, being 29′ 8″ more southerly. Hence the comet's right ascension was 303° 49′ 10″, and its declination 4° 43′ 54″ south.

October 15, 6h. 35′, the comet preceded the northermost of the said stars

23′ 6″ in right ascension, being more southerly than the star 4′ 15″. Hence the right ascension of the comet was 303° 24′ 40″; its declination 3° 51′ 3″ south.

October 21, 6h. 22′, a small star, whose right ascension was found 301° 7′ 17″, and its declination 0° 11′ 50″ south, preceded the comet 41′ 6″ in right ascension, being 5′ 50″ more southerly. Hence the comet's right ascension was 301° 48′ 23″, and its declination 0° 6′ 0″ south.

October 22, 6h. 24′, a small star, whose right ascension was found 301° 39′ 47″, and its declination 0° 32′ 43″ north, followed the comet ½ a minute in right ascension, being 13′ 43″ more northerly. Hence the comet's right ascension was 301° 39′ 17″, and its declination 0° 19′ 0″ north.

October 24, 8h. 2′, a small star, whose right ascension was found 301° 24′ 57″, and its declination 1° 9′ 22″ north, preceded the comet 0′ 37″ in right ascension, being 5′ 12″ more northerly. Hence the comet's right ascension was 301° 25′ 34″, and its declination 1° 4′ 10″ north.

October 29, 8h. 56′, a small star, whose right ascension was found 301° 6′ 20″, and its declination 2° 51′ 0″ north, preceded the comet one minute in right ascension, being 23′ 40″ more northerly. Hence the comet's right ascension was 301° 7′ 20″, and its declination 2° 27′ 20″ north.

October 30, 6h. 20′, the same star had exactly the same right ascension with the comet, being 11′ 33″ more northerly. Hence the comet's right ascension was 301° 6′ 20″, and its declination 2° 39′ 27″ north.

November 5, 5h. 53′, a small star, whose right ascension was found 300° 35′ 00″, and its declination 3° 45′ 30″ north, preceded the comet 33′ 0″ in right ascension, being 2′ 8″ more southerly. Hence the comet's right ascension was 301° 8′ 0″, and its declination 3° 47′ 38″ north.

November 8, 7h. 6′, a bright star, (placed by Hevelius in Rostro Aquilæ, but not inserted in the British Catalogue,) whose right ascension at this time was found 302° 21′ 30″, and its declination 4° 28′ 40″ north, followed the comet 1° 7′ 40″ in right ascension, being 13′ 3″ more northerly. Hence the comet's right ascension was 301° 13′ 50″, and its declination 4° 15′ 37″ north.

November 14, 6h. 20′, a star, whose right ascension was found 301° 27′ 10″, and its declination 4° 59′ 40″ north, preceded the comet 5′ 35″ in right ascension, being 5′ 50″ more southerly. Hence the comet's right ascension was 301° 32′ 45″, and its declination 5° 5′ 30″ north.

This was the last time that I observed the place of the comet until after the full moon, my affairs calling me to Oxford, where I had no convenience for making such observations.

Dr. Halley and Mr. Graham continued to observe the comet till November 20; and according to both their observations that evening at 7h. 45′, the comet followed β in Collo Aquilæ, 6° 33′ 55″ in right ascension, being about 4′ more northerly than the star. Hence the comet's right ascension was 301° 59′ 50″, and its declination 5° 48′ 55″ north.

The light of the moon daily increasing prevented them from making any more observations, the comet being by this time grown so faint, as to become in a manner imperceptible while the moon shone bright. And the faint appearance which it made before the moon obstructed the sight of it, gave little hopes of its being to be seen again after the full moon. Notwithstanding which, on December 3, (being then near Cirencester in Gloucestershire,) I was tempted by the serenity of the evening, and the use of a very good telescope of ten foot, to look for it again before the moon rose; and I found it among some small telescopical stars; but it appeared so faint and dull, as made it doubtful, whether what I took for the comet might not be a small star with a little haziness about it. But this doubt was cleared two nights after, when I perceived that the comet was moved from its former situation towards a bright telescopical star, from which I afterwards took its difference of right ascension and declination, upon my return to Wansted, on December 7. This star's right ascension was then found 303° 39′ 20″, and its declination 7° 32′ 30″ north. And December 7, 6h. 45′, the comet followed it 3′ 15″ in right ascension, being 14′ more northerly than the star. Hence the comet's right ascension was 303° 42′ 35″, and its declination 7° 46′ 30″ north.

This was the last night that I saw the comet, though I believe I might have continued to have observed it, had not an uninterrupted succession of cloudy evenings prevented so long, that it became uncertain where to look for it.

The forementioned observations are the principal of all that were made at Wansted; and most of them being taken from stars which are not in the British Catalogue, whose places therefore are here determined only by comparing them with some that were, it cannot be supposed that the comet's places deduced from them are altogether exact. For which reason I have all along set down, not only the place of the comet and star where it was known, but also the particulars of the observation, that if any hereafter should be willing to examine the tract of this comet more nicely, they may know where to find the stars from which it was observed. The places of the stars here set down are abundantly sufficient for that purpose, as will appear from the following table, which contains the longitudes and latitudes of the comet deduced from the foregoing observations, together with the places of the comet calculated from the theory of gravity, for the times of observation on the several days therein mentioned, as also the differences between the observed and computed places. Those differences not exceeding one minute, shew that the observations are not only consonant to each other, but that the places of the stars are likewise near the truth, since the comet's places deduced from them are found all along to agree sufficiently near with the theory of gravity; the truth of which having long since been established by its great author, sir Isaac Newton, and my worthy colleague Dr. Halley, needs not the confirmation of so short a series of observations as was made of this comet. But short as it is, I presume 'twill be no easy matter to account for the observations with the same degree of exactness any other way than by that theory, according to which the following computations are made.

1723. Temp. Æquat.	Comet. Long. Observat.	Lat. Bor. Observ.	Comet. Long. Comput.	Lat. Bor. Comput.	Differ. Long.	Differ. Latit.
D. H. ′	° ′ ″	° ′ ″	° ′ ″	° ′ ″	″	″
Octob. 9 8 5	♒ 7 22 15	5 2 0	♒ 7 21 26	5 2 47	+ 49	− 47
10 6 21	6 41 12	7 44 13	6 41 42	7 43 18	− 30	+ 55
12 7 22	5 39 58	11 55 0	5 40 19	11 54 55	− 21	+ 5
14 8 57	4 59 49	14 43 50	5 0 37	14 44 1	− 48	− 11
15 6 35	4 47 41	15 40 51	4 47 45	15 40 55	− 4	− 4
21 6 22	4 2 32	19 41 49	4 2 21	19 42 3	+ 11	− 14
22 6 24	3 59 2	20 8 12	3 59 10	20 8 17	− 8	− 5
24 8 2	3 55 29	20 55 18	3 55 11	20 55 9	+ 18	+ 9
29 8 56	3 56 17	22 20 27	3 56 42	22 20 10	− 25	+ 17
30 6 20	3 58 9	22 32 28	3 58 17	22 32 12	− 8	+ 16
Nov. 5 5 53	4 16 30	23 38 33	4 16 23	23 38 7	+ 7	+ 26
8 7 6	4 29 36	24 4 30	4 29 54	24 4 40	− 18	− 10
14 6 20	5 2 16	24 48 46	5 2 51	24 48 16	− 35	+ 30
20 7 45	5 42 20	25 24 45	5 43 13	25 25 17	− 53	− 32
Dec. 7 6 45	♒ 8 4 13	26 54 18	♒ 8 3 55	26 53 42	+ 18	+ 36

In order to determine the orbit of this comet, I supposed it to describe a parabola agreeable to what is delivered in the third book of sir Isaac Newton's Princip. Math. and then I found the inclination of the planes of the orbit and ecliptic 49° 59′; the place of the ascending node ♈ 14° 16′; the place of the perihelion ♉ 12° 52′ 20″; the distance of the perihelion from the node 28° 36′ 20″; the logarithm of the perihelion distance 9,999414; the logarithm of the diurnal motion 9,961007; the time of the comet's being in its perihelion September 16, 16h. 10′ equal time. In its orbit thus situated, the motion of the comet was retrograde, or contrary to the order of the signs.

From these elements, by the help of Dr. Halley's general table for comets, (to which they are adapted,) I computed the places in the foregoing table; which agreeing with the observed places as near as the observations themselves agree with one another, shew that it would be a vain attempt to pretend to determine the true ellipse in which this comet moves, or its periodical revolution, from so small a part of its orbit as that was, which it described between the first and last of the foregoing observations; this therefore must be left to posterity, especially since it is certain that this comet is not one of those, of which observations have hitherto been transmitted to us sufficient to determine the situation of their orbits.

The nucleus of this comet was very little, for it appeared but of a small diameter when I first saw it, although it was then above three times nearer to the earth than the sun is at its mean distance. Its tail was then hardly discernible with the naked eye, but through a telescope one might perceive a faint light extending itself above a degree from the body.

I have not yet heard that this comet was seen before October 6, although it was in a proper situation to have been observed in the morning, most part of September, especially from the time it was in its perihelion, till near the end of that month. For about that time it crossed the milky way between the mast of the ship and the head of the Great Dog, passing between the bright stars in the body and tail of the Great Dog towards the head of the Dove, where it was about September 29, being by that time got so far towards the south pole, as not to rise above our horizon. From thence it passed under the tail of Xiphias, within about 15° of the south pole of the ecliptic; and moving on between the head of Hydrus and the bright star in Eridanus, called Acharnar, it went by the stars in the body and neck of the Crane about October 5, when it came again above our horizon. From hence passing under the tail of the southern Fish, and between the stars in the shoulder of Capricorn, it crossed the ecliptic October 8, in about 8°½ of Aquarius. From thence it moved on by the hands of Aquarius and Antinous towards the head of the Eagle, according to its course before described.

The comet was in opposition to the sun October 1, when it had near 74°

southern latitude, and altered its longitude two signs in a day. About October 3, it was in its perigæon, or nearest distance to the earth, being then almost ten times nearer to it than the sun is at its mean distance; and its apparent motion was then about 20° in a day, and when I last saw it, 'twas above twice as far off as the sun.

Observations upon the Comet that appeared in the months of January, February, and March, 1737, made at Oxford.

[Philosophical Transactions, N°. 446. vol. XL. p. 111.]

I MADE several observations on the late comet, during the last five weeks of its appearance, which enabled me to find out the elements of a parabolic trajectory, upon which a calculus might be founded, that would correspond with each of my observations within about a minute of a degree; but the first of them being taken many days after the time of the perihelion, and the whole series comprehending but a very small portion of the trajectory, I was sensible that a little error, either in the observations themselves, or in the places of the fixed stars with which the comet was compared, might occasion a considerable difference in the situation and magnitude, &c. of the orbit deduced from them alone; and therefore I was desirous of having some earlier and accurate observations, in order to determine those elements with more certainty: but not having yet been able to procure such, I shall not longer defer laying before the Society the particulars of my own, together with the comparison between the observed places of the comet, and those computed from such elements as I have already collected from my own observations.

I first saw the comet on the 15th of February 1737, between six and seven in the evening, when its nucleus appeared small and indistinct, and its tail (extending above a degree from the body) pointed towards the star in Lino Austral. Piscium, marked ξ by Bayer. Applying my micrometer to a good seven foot tube, I observed, that at 7h. 32′ temp. æquat. the comet preceded the said star 1° 1′ 40″ in right ascension, and was 20′ 20″ more southerly than the star.—Note that the equal time is likewise made use of in all the following observations.

Assuming the place of this star as it is settled in the British Catalogue, (as I shall likewise the places of others hereafter mentioned,) it follows, that the comet's right ascension was 23° 58′ 0″, and its declination 1° 31′ 55″ north.

February 17, 7h. 33′ the comet followed α in Nodo Lin. Piscium 31′ 25″ in right ascension, and was 52′ 30″ more northerly. Hence the comet's right ascension was 27° 38′ 20″, and its declination 2° 21′ 10″ north.

February 18, 7h. 14′ a small star (whose right ascension was afterwards found to be 29° 0′ 5″, and declination 2° 58′ 30″ north) preceded the comet 24′ 0″ in right ascension, and was 15′ 30″ more northerly. Hence the comet's right ascension was 29° 24′ 5″, and its declination 2° 34′ 0″ north.

February 21, 7h. 25′ the comet preceded ν Ceti 1° 6′ 0″ in right ascension, and was 38′ 20″ more southerly. Hence its right ascension was 34° 25′ 10″, and its declination 3° 47′ 20″ north.

February 22, 7h. 45′ the comet followed ν Ceti 30′ 5″ in right ascension, and was 18′ 45″ more southerly. Hence the comet's right ascension was 36° 1′ 15″ and its declination 4° 6′ 55″ north.

February 25, 7h. 45′, a small star (whose right ascension was afterwards found to be 40° 34′ 0″, and declination 5° 5′ 30″ north) followed the comet 2′ 30″ in right ascension, and was 2′ 30″ more northerly than the comet. Hence the comet's right ascension was 40° 31′ 30″, and its declination 5° 3′ 0″ north.

The difference of right ascension and declination between this star and the comet, was taken with a fifteen foot telescope; but the place of the star was determined by one observation made with the seven foot tube.

February 27, 8h. 45′, the comet preceded a small star 1° 16′ 0″ in right ascension, and was 2′ 15″ more southerly. The right ascension of this star was afterwards (by a single observation) found to be 44° 37′ 40″, and its declination 5° 38′ 30″ north. Hence the comet's right ascension was 43° 21′ 40″, and its declination 5° 36′ 15″ north.

March 4, 8h. 0′, a small star (whose right ascension was found to be 49° 30′ 30″, and its declination 6° 38′ 30″ north) preceded the comet 7′ 30″ in right ascension, and was 10′ 0″ more southerly. Hence the right ascension of the comet was 49° 38′ 0″, and its declination 6° 48′ 30″.

March 12, 8h. 25′, the comet preceded μ Tauri 2° 5′ 50″ in right ascension, and was 4′ 25″ more northerly than the star. Hence the comet's right ascension was 58° 12′ 40″, and its declination 8° 16′ 50″ north.

March 14, 9h. 0′, the comet followed the 47th star of Taurus in the British Catalogue 12′ 50″ in right ascension, and was 0′ 15″ more northerly than the star. Hence the comet's right ascension was 60° 8′ 5″, and its declination 8° 34′ 5″ north. This, and all the following observations, were made with a good fifteen foot telescope, the comet now appearing too faint to be well observed with the seven foot tube.

March 17, 8h. 40′, the comet followed *r* Tauri 25′ 5″ in right ascension, and was 9′ 40″ more northerly. Hence its right ascension was 62° 47′ 55″, and its declination 8° 58′ 45″ north.

March 19, 7h. 50′, the comet followed the same star 2° 4′ 50″ in right ascension, being 23′ 55″ more northerly. Hence its right ascension was 64° 27′ 40″, and declination 9° 13′ 0″ north.

The same night at 9h. 0′, the comet preceded *d* Tauri 47′ 40″ in right ascension, and was 22′ 50″ more southerly. Hence its right ascension was 64° 30′ 20″, and declination 9° 12′ 35″ north.

March 20, 8h. 5′, the comet preceded *d* Tauri 0′ 30″ in right ascension, and was 16′ 35″ more southerly than the star. Hence its right ascension was 65° 17′ 30″, and declination 9° 18′ 50″ north.

March 22, 8h. 15′, the comet followed the same star 1° 36′ 10″ in right ascension, and was 3′ 50″ more southerly. Hence its right ascension was 66° 54′ 10″, and declination 9° 31′ 35″ north.

This was the last night that I saw the comet; for the moon, being then in her increase, entirely obstructed its farther appearance. The light of the comet was indeed (even in the moon's absence) so very weak, that I found it difficult, in some of the latter observations, to take its place with any tolerable certainty; which is, in part, the cause of some little disagreement observable in the comet's places taken from the same stars on different nights; though there are likewise other irregularities that occur in this series of observations, which seem to arise from small errors in the assumed places of the fixed stars.

Supposing the trajectory described by this comet to be nearly parabolical, conformable to what sir Isaac Newton has delivered in the third book of his Princip. Math. I collect from the foregoing observations that the motion of this comet in its own orbit was direct, and that it was in its perihelion, January 19, 8h. 20′ temp. æquat. Lond. That the inclination of the plane of the trajectory to the ecliptic was 18° 20′ 45″; the place of the descending node ♉ 16° 22′; the place of the perihelion ♒ 25° 55′; the distance of the perihelion from the descending node 80° 27′; the logarithm of the perihelion distance from the sun 9,347960; the logarithm of the diurnal motion 0,938188.

From these elements (by the help of Dr. Halley's general table for comets to which they are adapted) I computed the places in the following table; which also contains the longitudes and latitudes of the comet, calculated from the observed right ascensions and declinations above-mentioned, together with the differences between the observed and computed places.

Oxon. 1737. Temp. Æquat.	Com. Longit. Observat.	Lat. Aust. Observat.	Com. Longit. Computat.	Lat. Aust. Comput.	Diff. Long.	Diff. Lat.
D. h. ′	° ′ ″	° ′ ″	° ′ ″	° ′ ″	″	″
Febr. 15 7 32	♈ 22 45 7	7 53 27	♈ 22 45 00	7 53 1	+ 7	+26
17 7 33	26 30 30	8 27 21	26 30 44	8 28 6	−14	−45
18 7 14	28 18 14	8 44 20	28 17 46	8 43 57	+28	+23
21 7 25	♉ 3 26 34	9 26 50	♉ 3 26 53	9 26 46	−19	+ 4
22 7 45	5 4 53	9 40 00	5 5 28	9 39 27	−35	+33
25 7 45	9 42 18	10 12 21	9 41 19	10 12 22	+59	− 1
27 8 45	12 36 43	10 31 42	12 36 16	10 31 13	+27	+29
Mar. 4 8 00	19 3 00	11 6 46	19 3 5	11 7 8	− 5	−22
12 8 25	27 49 58	11 43 3	27 49 53	11 43 19	+ 5	−16
14 9 00	29 47 42	11 49 59	29 47 19	11 49 26	+23	+33
17 8 40	♊ 2 30 57	11 56 31	♊ 2 30 50	11 56 49	+ 7	−18
19 7 50	4 12 36	12 00 19	4 12 45	12 00 47	− 9	−28
9 00	4 15 11	12 1 12	4 15 13	12 00 52	− 2	+20
20 8 5	5 3 10	12 3 5	5 3 32	12 2 33	−22	+32
22 8 15	6 41 30	12 6 15	6 41 19	12 5 42	+11	+33

From the small differences between the comet's observed and computed places, exhibited in the two last columns of this table, we may reasonably conclude that the orbit, as above determined, cannot differ much from the truth, and must therefore be near enough to enable future astronomers to distinguish this comet upon another return, and thereby to settle its period; which I cannot at present pretend to do, not having met with an account of any former comet that seems likely to have been the same with this, whereof a description has been given particular enough to determine this point.

Observations upon the Comet that appeared in the months of September and October 1757, *made at the Royal Observatory.*

[Philosophical Transactions, vol. L. p. 408.]

I DEFERRED to give an account of my observations upon the comet that hath lately appeared, till I could settle the places of the stars with which it had been compared; several of them not being inserted in the British Catalogue, and those which are requiring some small corrections, which I have since made from my own observations.

When I first discovered this comet, it appeared to the naked eye like a dull star of the fifth or sixth magnitude; but viewing it through a seven foot telescope, I could perceive a small nucleus, (surrounded, as usual, with a nebulous atmosphere,) and a short tail extended in a direction opposite to the sun.

Some small stars then appearing in the field of the telescope with the comet, I measured its distance from them with a micrometer; and on September 12d. at 16h. 2′ mean time, I found it to be 1° 13′ 5″ distant from a small star, whose right ascension was afterwards found to be 89° 49′ 40″, and declination 36° 11′ 30″ north: and near the same time the comet was observed to be 43′ 10″ from another star, whose right ascension was 90° 20′ 0″, and declination 35° 12′ 0″ north.

Hence I collected that the comet's right ascension was 89° 29′ 10″, and its declination 35° 0′ 20″ north.

September 13d. 12h. 37′ mean time, (which is likewise made use of in the following observations,) the comet had the same right ascension with a small star, whose right ascension was 93° 5′ 30″, and declination 34° 36′ 40″ north; and it was about two minutes more northerly than the star. Hence the comet's right ascension was 93° 5′ 30″, and its declination 34° 38′ 40″ north.

September 14d. 14h. 0′, the comet preceded θ Geminorum 1° 31′ 35″ in right ascension, and was 11′ 35″ more southerly. The apparent right ascension of θ Geminorum was then 99° 11′ 40″, and its declination 34° 13′ 25″ north. Hence the right ascension of the comet was 97° 40′ 5″, and its declination 34° 1′ 50″ north.

September 17d. 13h. 0′, a small star (whose right ascension was 109° 55′

20″, and declination 31° 27′ 40″) preceded the comet 47′ 10″ in right ascension, and was 12′ 30″ more northerly. Hence the comet's right ascension was 110° 42′ 40″, and its declination 31° 15′ 10″ north.

September 19d. 15h. 17′, a star (whose right ascension was 118° 29′ 40″, and declination 28° 9′ 45″) preceded the comet 1° 14′ 0″ in right ascension, and was more southerly 15′ 45″. Hence the comet's right ascension was 119° 43′ 40″ and declination 28° 25′ 30″ north.

September 23d. 15h. 57′, a star (whose right ascension was 134° 55′ 45″, and declination 22° 15′ 55″ north) preceded the comet 12′ 30″ in right ascension, and was 29′ 0″ more northerly. Hence the comet's right ascension was 135° 8′ 15″, and its declination 21° 46′ 55″ north.

September 24d. 15h. 21′, the comet had the same declination with a small star that preceded it 10′ 15″ in right ascension. This star's right ascension was afterwards found to be 138° 13′ 45″, and its declination 20° 5′ 20″. Hence the comet's right ascension was 138° 24′ 0″, and its declination 20° 5′ 20″ north.

September 28d. 16h. 22′, the comet followed Regulus 1° 7′ 12″ in right ascension, and was 14′ 45″ more northerly. The right ascension of Regulus being then 148° 51′ 13″, and its declination 13° 8′ 35″ north; the comet's right ascension was 149° 58′ 25″, and its declination 13° 23′ 20″ north.

September 30d. 16h. 24′, ϱ Leonis (whose right ascension was 155° 0′ 10″, and declination 10° 32′ 53″ north) followed the comet 18′ 45″ in right ascension, and was 7′ 53″ more northerly. Hence the comet's right ascension was 154° 41′ 25″, and its declination 10° 25′ 0″ north.

October 2d. 16h. 48′, the 37th star, Sextantis Hevel. in the British Catalogue, (whose right ascension was 158° 21′ 25″, and declination 7° 38′ 40″ north,) preceded the comet 32′ 50″ in right ascension, and was 3′ 20″ more southerly. Hence the comet's right ascension was 158° 54′ 15″, and its declination 7° 42′ 0″ north.

October 3d. 16h. 45′, *c* Leonis (whose right ascension was 162° 2′ 15″, and declination 7° 24′ 0″ north) followed the comet 1° 12′ 55″ in right ascension, and was 56′ 40″ more northerly. Hence the comet's right ascension was 160° 49′ 20″, and its declination 6° 27′ 20″ north.

October 4d. 17h. 0′, *d* Leonis (whose right ascension was 162° 0′ 15″, and declination 4° 54′ 57″ north) preceded the comet 40′ 15″ in right ascension, and was more southerly 20′ 53″. Hence the comet's right ascension was 162° 40′ 30″, and its declination 5° 15′ 50″ north.

October 7d. 16h. 54′, the 79th Leonis in the British Catalogue (whose right ascension was 167° 53′ 37″, and declination 2° 44′ 15″ north) followed the comet 13′ 0″ in right ascension, and was more northerly 38′ 35″. Hence the comet's right ascension was 167° 40′ 37″, and its declination 2° 5′ 40″ north.

October 8d. 16h. 53′, the comet preceded υ Leonis 1° 53′ 30″ in right ascension, and was 37′ 20″ more northerly. The right ascension of this star was 171° 7′ 45″, and its declination 0° 30′ 55″ north; therefore the comet's right ascension was 169° 14′ 15″, and its declination 1° 8′ 15″ north.

October 11d. 16h. 52′, the comet followed υ Leonis 2° 33′ 30″ in right ascension, and appeared 1° 55′ 5″ more southerly; but it being near the horizon, the difference of right ascension must have been contracted by refraction about 1′ 5″, and the difference of declination about 1′ 30″: so that the corrected right ascension of the comet was 173° 42′ 20″, and its declination 1° 25′ 40″ south.

Immediately after this observation a fog arose, which prevented me from repeating it; and several mornings following proving hazy or cloudy, I could not see the comet again till October 18th, about an hour and a quarter before sun-rising; when the twilight being strong, and the comet low, it appeared very faint. However, I was unwilling to omit the opportunity of determining its place, as near as I could, by a single observation, in the following manner.

At 6h. 59′ 54″ ½ sidereal time, I observed the passage of the comet over the perpendicular wire of my equatorial sector; then leaving the instrument in the same position till the next evening, I observed, that at 22h. 8′ 15″ sidereal time, the 17th star of Eridanus in the British Catalogue passed over the same wire (or horary circle) 9′ 30″ more southerly than the comet. And at 23h. 45′ 36″ sidereal time, the star marked *h* in Eridanus passed 19′ 55″ more northerly than the comet.

I found that the situation of my instrument was not sensibly altered between the 18th and 19th of October; for the transits and the difference of declination of the same stars being observed with it again on the 19th of October, they agreed very well with those that were taken the preceding night. It may therefore be supposed, that the position of the instrument continued the same likewise during the time of the foregoing observations.

The right ascension of the 17th star of Eridanus being 49° 39′ 10″, and its declination 5° 55′ 25″ south; and the right ascension of *h* of Eridanus being 73° 59′ 25″, and its declination 5° 25′ 10″ south; I collected, that when the comet passed the wire, (or horary circle,) which was October 17d. 17h. 12′ mean time, its right ascension was 182° 34′ 0″, and its declination 5° 45′ 35″ south.

The last time that I saw the comet was on the 19th of October in the morning; but it then appeared so faint, that I could not observe its place. Its elongation from the sun was then but about twenty degrees; and from that day to the present it hath always been less; which is the principal reason why it was invisible to us at the time when it was in its perihelion, and hath remained so ever since. The elongation will indeed soon become greater, and yet it is probable that we shall not be able to see the comet again; because its real distance from the sun will be greater than it was when I first saw it, and it will be also four times farther from us than it was at that time.

The comet kept nearly at the same distance from the earth for ten or twelve days together after I first saw it; but its brightness gradually increased then, because it was going nearer to the sun. Afterwards, when its distance from the earth increased, although it continued to approach the sun, yet its lustre never much exceeded that of stars of the second magnitude, and the tail was scarce to be discerned by the naked eye.

All the forementioned observations were made with a micrometer in a seven foot tube, excepting those of the 3d, 11th, and 17th days of October, which were taken with a curious sector constructed for such purposes by the late ingenious Mr. George Graham; of which Dr. Smith has given a very exact description in his third book of Optics.

Supposing the trajectory of this comet to be parabolic, I collected from the foregoing observations that its motion round the sun is direct, and that it was in its perihelion October the 21st, at 7h. 55′ mean (or equated) time, at Greenwich. That the inclination of the plane of its trajectory to the ecliptic is 12° 50′ 20″; the place of the descending node ♉ 4° 12′ 50″; the place of the perihelion ♌ 2° 58′ 0″; the distance of the perihelion from the descending node 88° 45′ 10″; the logarithm of the perihelion distance 9,528328; the logarithm of the diurnal motion 0,667636.

From these elements (which are adapted to Dr. Halley's general table for the motion of comets in parabolic orbits) I computed the places of this comet for the respective times of the foregoing observations, as in the following table; which contains likewise the longitudes and latitudes deduced from the observed right ascensions and declinations, and also the differences between the computed and observed places. These differences (nowhere exceeding 40″) shew, that the elements here set down will be sufficient to enable future astronomers to distinguish this comet upon another return; but as they do not correspond with the elements of the orbit of any other comet hitherto taken notice of, we cannot determine at present the period thereof.

Greenwich, 1757. Mean Time.	Comet, Long. Observ.	Latit. Observ.	Long. Comp.	Latit. Comput.	Diff. Long.	Diff. Latit.
D. H. ′	S. ° ′ ″	° ′ ″	S. ° ′ ″	° ′ ″	″	″
Sept. 12 16 2	♊ 29 34 13	11 32 16 N.	♊ 29 34 11	11 32 20 N.	− 2	+ 4
13 12 37	♋ 2 35 34	11 12 13	♋ 2 35 47	11 12 11	+ 13	− 2
14 14 0	6 27 45	10 44 3	6 27 42	10 43 43	− 3	− 20
17 13 0	17 49 40	9 3 31	17 50 16	9 3 11	+ 36	− 20
19 15 17	26 6 8	7 36 49	26 5 50	7 36 30	− 18	− 19
23 15 57	♌ 11 19 18	4 33 38	♌ 11 19 4	4 33 32	− 14	− 6
24 15 21	14 44 19	3 49 37	14 44 3	3 49 39	− 16	+ 2
28 16 22	27 23 43	1 3 44 N.	27 23 32	1 3 52 N.	− 11	+ 8
30 16 24	♍ 2 45 43	0 5 30 S.	♍ 2 45 39	0 5 17 S.	− 4	− 13
Oct. 2 16 48	7 37 43	1 5 50	7 37 42	1 5 32	− 1	− 18
3 16 45	9 51 36	1 32 22	9 51 29	1 31 55	− 7	− 27
4 17 0	12 1 4	1 56 42	12 0 25	1 56 23	− 39	− 19
7 16 54	17 51 3	2 56 48	17 51 6	2 56 24	+ 3	− 24
8 16 53	19 39 45	3 13 7	19 39 33	3 12 28	− 12	− 39
11 16 52	24 47 22	3 48 49	24 47 47	3 49 29	+ 25	+ 40
17 17 12	♎ 4 38 58	4 15 42 S.	♎ 4 38 36	4 15 2 S.	− 22	− 40

The Longitude of Lisbon and the Fort of New York from Wansted and London, determined by Eclipses of the First Satellite of Jupiter.

[Philosophical Transactions, N°. 394. vol. XXXIV. p. 85.]

SOME curious astronomical observations having lately been communicated to this Society from Lisbon, among which were several eclipses of the first satellite of Jupiter[a]; I was willing to examine whether I had made any at Wansted which tallied with them, that by comparing such together, the true difference of longitude between those places might be found. But looking over my observations of the first satellite, made last year and the beginning of this, I meet only with two emersions that were observed the same night both at Lisbon and Wansted. There are others, indeed, made within a few days of each other, which may likewise be made use of, to determine the difference of longitude; but not with the same degree of certainty, by reason of the irregular motion of the satellite; which, I presume, chiefly arises from the gravity of the other satellites towards it. For although the effect of the influence that the satellites have on each other is most remarkable in the second, whose motion will sometimes be accelerated or retarded thereby as much as amounts to thirty or forty minutes in time, in the space of about seven months, or in half the period in which the three innermost satellites return to have nearly the same position with respect to themselves and the shadow of Jupiter; yet the first seems also liable to inequalities that cannot well be accounted for, but from some such cause as is before mentioned, the effect of which will not easily be reduced to any rule, but from a long and exact series of observations. And till some better and more certain rule can be found out, we may suppose, that the effect produced by this cause is, during small intervals, proportionable to the time. On this supposition I have compared some observations with others not made the same nights; and the result is nearly the same as in those which were observed at the same time in both places, as will appear by the following particulars.

The immersion of the first satellite was observed at Wansted with Mr.

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[a] By John Baptist Carbo; see p. 90 of the 34th volume of the Philosophical Transactions.

Hadley's reflecting telescope on August 4, N. S., 1725, about 45″ after the time of the immersion, as calculated from my tables. By another observation made August 29, N.S., the true immersion preceded the calculation from the same tables 1′ 10″: so that in twenty-five days the satellite's motion was accelerated as much as answered to 1′ 55″ in time. Supposing therefore the acceleration to have been in the same proportion between July 28, and August 4, N.S., then the true immersion July 28, N.S., would have happened at Wansted about 1′ 15″ after the time by the tables, which make the immersion at 12h. 48′ 45″ app. time. The true immersion therefore was at Wansted, July 28, N.S., 12h. 50′ 0″ app. time; and at Lisbon 'twas observed at 12h. 12′ 26″ app. time, the difference being 37′ 34″.

September 28, N.S., the first satellite was seen emerging in the reflector at Wansted 3′ 50″ sooner than the tables make the emersion; and by the mean of two more observations made at the same place, and with the same telescope, on the 14th and 16th of October, N.S., the true emersion preceded the calculation 4′ 30″. We may therefore from hence conclude, that on September 21, N.S., the true emersion at Wansted preceded the calculation by the tables about 3′ 35″, and that the true emersion there was at 12h. 1′ 15″ app. time; but this emersion was observed at Lisbon at 11h. 24′ 55″, the difference being 36′ 20″.

The observations at Wansted being made with Mr. Hadley's reflecting telescope, (by which one may see the first satellite near $\frac{1}{4}$ of a minute sooner when 'tis emerging, than in a refracting telescope of fifteen feet, and the contrary when 'tis immerging,) there ought to be some allowance made on account of different telescopes made use of at Lisbon and Wansted, by deducting 10″ or 15″ from the difference of time collected from the immersions, and adding as much to the difference deduced from the emersions. Such correction being made, the difference of meridians by the immersion observed July 28, will be 37′ 20″, and by the emersion September 21, 36′ 35″.

The emersion observed at Lisbon, December 8, N.S., at 8h. 32′ 40″ apparent time, was likewise seen at Wansted in a fifteen foot tube at 9h. 10′ 5″ apparent time, the air being a little hazy, which may probably make the difference 37′ 25″ a little too great.

The emersion seen at Lisbon, January 16, 1726, N.S., at 6h. 51′ 10″, which seems accompanied with circumstances that argue its exactness, was likewise very well observed at Wansted in a fifteen foot tube at 7h. 28′ 22″ apparent time, the difference being 37′ 12″.

These are the only observations among those which were last communicated, that I could compare with any degree of certainty with my own: but I find others printed in the Philosoph. Transact. N°. 385, which were likewise made by the same curious persons, who observed an emersion of the first satellite at Lisbon, September 2, 1724, N. S., at 9h. 36′ 57″. This was seen also at Wansted in the reflector at 10h. 13′ 28″ apparent time. Hence, allowing for the different telescopes, the difference of meridians is 36′ 45″.

This emersion at Wansted preceded the calculation by the tables 4′ 40″: and another emersion observed with the same telescope on September 18, N. S., preceded the calculation 5′ 10″. We may therefore suppose, that on September 9, N. S., the true emersion at Wansted preceded the computed about 4′ 52″. The emersion that day by the tables was at 12h. 15′ 34″ app. time; therefore the true emersion at Wansted was at 12h. 10′ 42″. At Lisbon 'twas observed at 11h. 34′ 26″: so that allowing for the difference of telescopes, the difference of meridians by this observation is 36′ 30″.

The mean of all these differences is about 36′ 58″, from which, subtracting 28″ for the difference of meridians between London and Wansted, the remainder will be the difference of meridians between London and Lisbon; viz. $36'\frac{1}{2} = 9° 7'\frac{1}{2}$, Lisbon being so much to the westward of London. This difference of longitude is about $5'\frac{1}{2}$ greater than what is determined in the forementioned Transaction: but as the gentlemen to whom we are indebted for these observations have given us hopes that they will continue to make and communicate more, we need not doubt but their exact care and diligence will soon enable us to judge yet more nicely of the true situation of those cities with respect to each other.

The same Transaction containing some observations of eclipses of the same satellite made in the fort of New York, communicated by his excellency William Burnet, esq. governor of New York, I shall take this opportunity of determining the longitude of that fort more exactly than it can be supposed to be there done, by the bare comparison of the observations with the tables; having two observations made at Wansted, which tally with two made at New York, on August 25, and September 10.

By the observation made August 25, 1723, O. S., which is esteemed the most distinct and best, the satellite emerged at 9h. 35′ 14″ by the clock, which went about $1'\frac{1}{4}$ too fast for the apparent time at the emersion, as appears by the altitudes of the sun's limb taken the morning before and after the observation; so that the emersion at New York was at 9h. 34′ apparent time; that is, 9h. 32′ 20″ mean time.

August 27, 8h. 57′ 40″ mean time, the satellite was seen emerging at Wansted in the reflector; and September 12, 7h. 17′ 15″ mean time, 'twas seen emerging again in the same telescope: so that in 15d. 22h. 19′ 35″ there were nine emersions; and the interval between each was about 1d. 18h. 28′ 50″. This subtracted from the time of the emersion observed at Wansted August 27, will give the true emersion at Wansted on August 25, 14h. 28′ 50″ mean time; that is, 4h. 56′ 30″ later than it was observed at New York.

September 10, 8h. 0′ 10″ by the clock, another emersion was observed at New York. From the altitudes of the sun's limb taken the morning before, I compute the error of the clock at the time of the emersion to be 1′ 10″, and that the emersion happened at 7h. 59′ apparent time, that is, 7h. 51′ 52″ mean time at New York. But subtracting the forementioned interval of 1d. 18h. 28′ 50″ from the time of the emersion observed at Wansted September 12, 7h. 17′ 15″ mean time, we shall have the time of the true emersion at Wansted on September 10, at 12h. 48′ 25″ mean time, which is 4h. 56′ 33″ later than 'twas observed at New York. The difference therefore of meridians between Wansted and New York, allowing about 15″ for the difference of telescopes, is about 4h. 56′ 45″, and between London and New York, 4h. 56′$\frac{1}{4}$: so that the true longitude of New York from London is 74° 4′ west.

An Account of some Observations made in London, by Mr. George Graham, F.R.S.; and at Black-River in Jamaica, by Colin Campbell, Esq. F. R. S. concerning the Going of a Clock, in order to determine the Difference between the Lengths of Isochronal Pendulums in those places.

[Philosophical Transactions, N°. 432. vol. XXXVIII. p. 302.]

ALTHOUGH it is now above sixty years since Mr. Richer first discovered that pendulums of the same length do not perform their vibrations in equal times in different latitudes; and though several experiments made since in different parts of the earth concur to prove that pendulums swinging seconds are in general shorter as we approach the equator; yet what the real difference is between their lengths in different latitudes, does not seem to have been determined with sufficient exactness by the observations that have hitherto been communicated to the public; as may be gathered from the twentieth proposition of the third book of sir Isaac Newton's Principia, where they are compared as well with each other, as with the theory of that illustrious author. It were therefore to be wished, that more of this kind of experiments could be made with greater accuracy in proper places, by such persons as have sufficient skill and opportunities to do it; that we might thereby be enabled to judge with more certainty concerning the true figure of the earth, and the nature of its constituent parts.

As an inducement to such as may have it in their power to put the like again into practice, I shall lay before the Society an account of a very curious experiment of this sort lately made in Jamaica, by our worthy member Colin Campbell, esq., whose knowledge and abilities in every respect qualifies him for the improvement of arts and sciences; and whose genius prompts him to cultivate them so assiduously, that I doubt not but we shall soon have the satisfaction of receiving many other valuable observations from him, particularly such as relate to astronomy; he having furnished himself with an apparatus of instruments not unworthy the observatory of a prince. Among these is a clock whose pendulum vibrates seconds, made by our ingenious member Mr. George Graham, justly esteemed for his great skill in mechanics; who judging that an opportunity was now offered of trying with the utmost exactness what is the true difference between the lengths of isochronal pendulums at London and Jamaica, readily embraced it; and in framing the parts of the clock, carefully contrived that its pendulum might at pleasure be reduced to the same length, whenever there should be occasion to remove the clock from one place, and set it up in another.

This clock being chiefly designed for astronomical observations, had no striking part, and its pendulum was adjusted to such a length, that in London it vibrated seconds, of sidereal, and not of solar time. When it was finished, Mr. Graham fixed it up in a room situated backward from the street, and on the north side of his house, to prevent its being disturbed by coaches or other carriages that passed through the street, and that it might be as little affected by the sun as possible. Having set it going, he compared it with the transits of the star Lucida Aquilæ over the meridian, which passed

		H.	′	″	
1731, August	20 at	8	59	15	by the clock.
	22 at	8	59	18	
	23 at	8	59	20½	
	25 at	8	59	22	
	28 at	8	59	25½	
	29 at	8	59	26	
	30 at	8	59	27	

Hence it appears, that the clock gained twelve seconds in ten apparent revolutions of the star.

In order to estimate how much the pendulum may be lengthened by greater degrees of heat, or how much slower the clock would go on that account, when removed into a warmer climate, a thermometer was fixed by the side of it; and between the hours of ten and eleven in the morning, and at night, notice was taken at what height the spirits stood, and the mean height for each day was as follows:

		Therm.
1731, August	21	32½ divisions.
	22	30¾
	23	28¾
	24	27¾
	25	28¼
	26	27¼
	27	27½
	28	27½
	29	27½
	30	27¾

Hence the mean height for all these days was about 28½ divisions.

The clock-weight that keeps the pendulum in motion is 12lb. $10\frac{1}{2}$ oz. and is to be wound up once in a month. The weight of the pendulum itself is 17lb. and (during the time that the clock was compared with the transits of the star) it vibrated each way from the perpendicular 1° 45′. The magnitude of the vibrations was estimated by means of a brass arc, which was fixed just under the lower end of the rod of the pendulum, and divided into degrees, &c.

August 31, Mr. Graham took off the weight belonging to the clock, and hung on another of 6lb. 3 oz. and with this weight the pendulum vibrated only 1° 15′ on each side; and the clock went one second and an half slower in twenty-four hours, than when its own weight of 12lb. $10\frac{1}{2}$ oz. was hung on.

This experiment shews, that a small difference in the arcs described by the pendulum, or a small alteration in the weight that keeps it in motion, will cause no great difference in the duration of the vibrations; and therefore a little alteration in the tenacity of the oil upon the pivots, or in the foulness of the clock, will not cause it to accelerate or retard its motion sensibly; from whence we may conclude, that whatever difference there shall appear to be, between the going of the clock at London and in Jamaica, it must wholly proceed from the lengthening of the pendulum by heat, and the diminution of the force of gravity upon it.

A particular written account of the observations and experiments hitherto taken notice of, was delivered to me by Mr. Graham in September 1731, about the same time the clock was put on shipboard to be carried to Jamaica. He likewise sent very full directions to Mr. Campbell, describing in what manner the clock was to be fixed up, and how the pendulum might be reduced exactly to the same state as it was when in England; but no intimation was given concerning the going of the clock, that the experiment might be made with all possible care and caution, and without any bias or prejudice in favour of any hypothesis, or former observations.

In July 1732, we received an account of the success of the experiment, by the hands of Mr. Joseph Harris, who was present at the making of it in Jamaica, whither he went the year before with Mr. Campbell, in order to assist him in his design of erecting an observatory for the improvement of astronomy, and the promoting other parts of natural knowledge in that island: but his ill state of health obliging him to return into England, he brought with him the original journal of the observations of the transits of two stars (viz. Sirius & β Canis Majoris) over the meridian, compared with the clock, after it was fixed up in Jamaica, as Mr. Graham had directed; together with the height of the spirits of the forementioned thermometer upon the several days of observation.

The chief of those observations are contained in the following table, the first column whereof shews the day of the month; the second the name of the star, and the time by the clock of its observed transit over the meridian; the third contains the hour of the day when the thermometer was observed, together with the height of the spirit at those hours; the morning hours being denoted by the letter A, and those of the afternoon by the letter P.

	Can. Maj.	Time of Transit.	Hour of Day.	Thermometer.
		H. ′ ″	H.	
1732. Jan. 23.	β	11 59 50	10½ A	14¾
	α	12 22 14	9½ P	11
24.		Cloudy.	11½ A	15¼
25.	β	11 55 40	8½ A	17½
	α	12 18 4	9¼ P	11¼
26.	β	11 53 35	8 A	20
	α	12 16 00	2 P	8½
			9 P	10
27.	β	11 51 31	7 A	17½
	α	12 13 55	2 P	8½
			9¾ P	12½
28.	β	11 49 26	7 A	20½
	α	12 11 51	2 P	11
			10 P	12
29.	β	11 47 22	6¾ A	19
	α	12 9 46	3 P	9
			9 P	11¼
30.		Cloudy.	7 A	20½
			4 P	7
			11 P	13
31.	β	11 43 12	7 A	20
	α	12 5 37	9 P	8½
Feb. 1.	β	11 41 8½	10 A	18¾
	α	12 3 33	11 P	16
2.	β	11 39 0	9½ A	17½
	α	12 1 23½	2 P	9
			5 P	6
			9 P	8½
3.	β	11 36 53	8½ A	19
			1 P	9½
			9 P	9
		H. ′ ″	H.	
1732. Feb. 4.	β	11 34 46	6¼ A	18
	α	11 57 11	12	9½
			9 P	8
5.	β	11 32 40	7½ A	19¼
	α	11 55 5	3½ P	6
			8½ P	8
6.	β	11 30 35	7 A	18½
	α	Cloudy.	4 P	7½
			8½ P	8
7.	β	11 28 31	7 A	20½
	α	11 50 55	12	12
			8½ P	8½
8.	β	Cloudy.	6½ A	21½
	α	11 48 50	8½ P	8¼
9.	β	11 24 20	9½ A	14
	α	11 46 44	8½ P	8
10.	β	11 22 12½	7¼ A	16
	α	11 44 37	11½ A	10
			3¾ P	3¾
			8½ P	6
11.	β	11 20 6	7½ A	16
	α	11 42 30	12	9½
			8½ P	5¾
12.	β	11 18 0	10 A	17½
	α	11 40 24	12	13
			8 P	5¾
13.		Clouds.	9 A	17
			8 P	6
14.	β	Cloudy.	7½ A	16
	α	11 36 15	12	11
			8 P	10

	Can. Maj.	Time of Transit.	Hour of Day.	Thermometer.
		H. ′ ″	H.	
1732. Feb. 15.		Clouds.	9 A	18
			12	13¾
			8½ P	7½
16.	β	Cloudy.	8 A	14
	α	11 32 4	8 P	7
17.	β	11 7 34	12	12
	α	11 29 59	8 P	6¼
1732. Feb. 18.	β	11 5 29	12	12¼
	α	11 27 53		

The pendulum during this interval vibrated about 1° 52′ each way from the perpendicular.

The transits of the stars over the meridian were observed with a telescope fixed at right angles to an horizontal axis, whose ends lay exactly east and west; by the turning of which axis the line of collimation of the telescope was constantly directed in the plane of the meridian. This instrument was daily adjusted to a mark fixed in the meridian; and in the journal, between the second and third of February, the following remark was made.

N. B. This day was hotter than usual, as appears by the thermometer; and the transit instrument had lost the level a little; but after we had adjusted it, it pointed exactly to our meridian mark, and therefore we are at a loss for the cause of this difference in the clock.

From the foregoing table it appears that the clock lost 54′ 21″ in twenty-six revolutions of the stars; that is, about 2′ 5″½ in one revolution; the difference from this medium somewhat varying, upon account of a greater or less degree of heat on different days.

The mean of all the observed heights of the thermometer from January 26th, to February 18th, was about 12½ divisions. Therefore the difference between the mean heights of the thermometer at Jamaica, and London, during the intervals of the respective observations, was 15½ divisions; the spirits standing so much higher in Jamaica, because of the great heat in that island.

That we might be able to judge how much the different degrees of heat, corresponding to any number of divisions upon this thermometer, would cause the clock to go slower by lengthening its pendulum, Mr. Graham took notice of the lowest point to which the spirits sunk at London in the winter, 1731; and the greatest height to which they rose in the following summer; and comparing the motion of the spirits in this thermometer with the alterations in another made with quicksilver, which he has for some years made use of;

he concluded, that at London the spirits in this thermometer would stand (communibus annis) above sixty divisions higher in summer than in winter.

By several years' experience, he has likewise found that his clocks, (of the same sort with Mr. Campbell's,) when exposed as usual to the different degrees of heat and cold of our climate, do not vary in their motion above twenty-five or thirty seconds in a day.

From these observations and experiments therefore we may reasonably conclude, that sufficient allowance will be made for the lengthening of the pendulum by heat, if we suppose the clock, upon that account, to go one second in a day slower, when the spirits of this thermometer stand two divisions higher, and in the same proportion for other heights.

Admitting then that the mean height of the thermometer, while the clock was compared with the stars at Jamaica, exceeded that at London between fifteen and twenty divisions; if we allow eight or nine seconds upon that account, the remaining difference must be wholly owing to the difference of the force of gravity in the two places.

Upon comparing the observations, it appears that in one apparent revolution of the stars, the clock went $2' \, 6''\frac{1}{2}$ slower in Jamaica than at London; deducting therefore $8''\frac{1}{2}$, on account of the great heat in Jamaica, there remains a difference of $1' \, 58''$, which must necessarily arise from the diminution of gravity, in the place nearest the equator.

I have allowed the clock to have lost somewhat more, on account of the difference of heat, than the mean heights of the thermometer may seem to require, upon a supposition, that the total heat of the days, compared with the cold of the nights, bears a greater proportion in Jamaica than London; but if that supposition be not admitted, then the clock in Jamaica must have gone rather more than $1' \, 58''$ in a day slower than in England.

Mr. Campbell's observations were made at Black-River, in 18° north latitude. Now if we suppose, with sir Isaac Newton, that the difference in the going of the clock is owing to the greater elevation of the parts of the earth towards the equator, it will follow from these observations, and what is delivered by him in the twentieth proposition of the third book of his Principia, that the equatorial diameter is to the polar as 190 to 189; the difference between them being $41\frac{1}{2}$ miles; which is somewhat greater than what sir Isaac Newton had computed from his theory, upon the supposition of an uniform density in all the parts of the earth.

I shall not enter into the dispute about the figure of the earth, but at present suppose, with sir Isaac Newton, that the increase of gravity, as we recede from the equator, is nearly as the square of the sine of the latitude; and that the difference in the length of pendulums is proportional to the augmentation or diminution of gravity. Upon these suppositions, I collect from the forementioned observations, that if the length of a simple pendulum (that swings seconds at London) be 39,126 English inches, the length of one at the equator would be 39,00, and at the poles 39,206. And (abstracting from the alteration on account of different degrees of heat) a pendulum clock, that would go true time under the equator, will gain 3′ 48″¼ in a day at the poles; but the number of seconds which it would gain in any other latitude would be to 3′ 48″¼ nearly as the square of the sine of that latitude is to the square of the radius: from whence it follows, that the number of seconds which a clock will lose in a day, upon its removal to a place nearer to the equator, will be to 3′ 48″¼ nearly as the difference between the squares of the sines of the latitudes of the two places to the square of the radius. Thus the difference of the squares of the sines of 51°½ and 18°, the latitudes of London and Black-River being to the square of the radius as 118 to 228¼, the clock will go 1′ 58″ in a day slower at Black-River than at London, as was found by observation.

It may be hoped that Mr. Campbell's success in this experiment, and the little trouble there is in making it, will induce those gentlemen who may hereafter carry pendulum-clocks into distant countries, to attempt a repetition of it after his manner; that is, by keeping or restoring the pendulums of their clocks to the same length in the different places, and carefully comparing them with the heavens, and at the same time taking notice of the different degrees of heat, by means of a thermometer. From a variety of such experiments we should be enabled to determine how far sir Isaac Newton's theory is conformable to truth, with much greater certainty than from those trials which are made by actually measuring the lengths of simple pendulums; because a difference of one hundredth part of an inch in the length of a pendulum corresponds to eleven seconds in a day; and it being easy to observe how much a clock gains or loses in a day, even to a single second, it is certain that by means of a clock, compared in the manner above mentioned, we may distinguish a difference (in the lengths of isochronal pendulums) of one thousandth part of an inch, or less; whereas it will be scarce possible to measure their true lengths without being liable to a greater error than that. Besides, by taking notice how much a clock gains or loses, upon the falling or rising of a thermometer, we can better allow for the different degrees of heat in this, than in the other method of making the experiment, by actual measurement; since it may not be easy to determine how much the measure itself, which we make use of, will be lengthened by different degrees of heat.

For these reasons, I esteem Mr. Campbell's experiment to be the most accurate of all that have hitherto been made, and properest to determine the difference of the gravity of bodies in different latitudes; and therefore I shall subjoin a table which I computed from it, containing the difference of the length of a simple pendulum swinging seconds at the equator, and at every fifth degree of latitude, together with the number of seconds that a clock would gain in a day in those several latitudes, supposing it went true when under the equator; by means of which, any one may readily compare other the like observations with his, and thereby discover whether the alteration of gravity in all places be uniform, and agreeable to the rule laid down by sir Isaac Newton, or not.

The latitude of the place.	The difference of the length of the pendulum in parts of an English inch.	Seconds gained by a clock in one day.	The latitude of the place.	The difference of the length of the pendulum in parts of an English inch.	Seconds gained by a clock in one day.
Deg.	Inch.	Seconds.	Deg.	Inch.	Seconds.
5	0.0016	1.7	50	0.1212	134.0
10	0.0062	6.9	55	0.1386	153.2
15	0.0138	15.3	60	0.1549	171.2
20	0.0246	26.7	65	0.1696	187.5
25	0.0369	40.8	70	0.1824	201.6
30	0.0516	57.1	75	0.1927	213.0
35	0.0679	75.1	80	0.2003	221.4
40	0.0853	94.3	85	0.2050	226.5
45	0.1033	114.1	90	0.2065	228.3

Directions for using the common Micrometer.

[Philosophical Transactions, vol. LXII. p. 46.]

MICROMETERS, as first contrived, being only adapted to the measuring small angles, as the diameters of the sun and moon, or other planets, and taking the distance of such objects as appeared within the aperture of the telescope at the same time, were not of so general use as those which are contrived not only to answer the ends that the first inventors aimed at, but likewise to take the difference of right ascension and declination of such objects as are farther asunder than the telescope will take in at once, but which pass through the aperture of it at different times. Mr. Cassini first made use of threads, intersecting one another at half right angles, for determining the difference of right ascensions and declinations of objects near the same parallel; and this apparatus, being simple and easily procured, is of very great use to such as are not provided with a micrometer made according to the late improvements. But, where such a one is at hand, that method, however curious, need not be made use of, the micrometer serving for the same purpose with greater exactness. It was for this reason indeed that the late alteration in the form of the micrometer was made, they being before not so convenient for making such sort of observations, both hairs being usually moveable, and no provision being made for setting the hairs parallel to the diurnal motion of the objects to be observed; both which inconveniences are avoided in the present micrometers.

The micrometer, as now contrived, is not only of use in measuring small angles or distances between such objects as appear within the aperture of the telescope at the same time, but likewise in taking the difference of right ascension and declination between stars and planets, &c. which in their apparent diurnal motion follow one another through the telescope, if kept in the same situation. In making the first kind of observations, turn the short tube which carries the eyeglass and micrometer, &c. till the cross thread (or that which cuts the parallel threads at right angles) lies parallel to a line passing through the objects whose distance is to be measured, and then, by raising or depressing the telescope by help of the stand, bring the objects to appear upon or near the cross thread, and one of them just to touch the fixed parallel thread; then turn the index of the micrometer till the moveable thread touches the other object, and the number of revolutions and parts of a revolution shewn by the index, turned into minutes and seconds by the table made as hereafter directed, will be the apparent angular distance of those objects. It is here supposed, that the threads exactly close, so as to touch each other when the index stands at the beginning of the divisions: for if they do not, there must be an allowance made in every observation; to avoid which, it is always best to adjust the threads to the beginning of the divisions when they are first put on; for which purpose, the holes in the little plate which carries the moveable thread are made oblong, to give room to move it as occasion requires, before it is pinched hard by the small screws which fasten it to the moveable arm, through which the long screw passes. The other parallel thread, which I call the fixed one, must be first adjusted by setting its edge exactly over the two marks made on each side the short diameter of the aperture in the broad plates, and the cross thread must be likewise set to agree with the strokes made on each side the longest diameter, and then the intersection of the cross thread, and the fixed parallel one, will be the centre of the motion given to the outer plate of the micrometer (to which the great screw index and threads are fastened) by the worm, by turning of which, the fixed parallel thread may easily be made to lie parallel to the apparent motion of any object, in order to take the difference of declination and right ascension from any other that follows through the aperture of the telescope.

This contrivance is of very great use to make a star, &c. move true along the fixed parallel thread, which is absolutely necessary, in order to take the true difference of right ascension and declination between it and any other that follows. Without this contrivance, it is very difficult to make a star move exactly upon the thread, and it can only be done by repeated trials, which may sometimes take up a great deal of time.

If therefore a star is made to move on the parallel thread just at the cross, and (the telescope continuing fixed in the same position) it is afterwards near its going out of the aperture found not to be upon the thread, that must then be brought to the star by the help of the worm, and then the thread will lie parallel to the diurnal motion of the star in that part of the heavens, and consequently the cross thread will represent a meridian, and the others parallels of declinations, and the difference of time between the passage of the star at the cross wire, (which was made to move along the thread,) and the transit of any other star, &c. over the cross thread which represents a meridian, turned into degrees and minutes, will give the difference of right ascension. And, if the moveable parallel thread be brought, by turning the index, to touch the other star about the time of its passage over the cross thread, then the number of revolutions and parts shewn by the index, (turned into minutes and seconds of a degree by the table,) will be the difference of declination between the two stars. If the star is made to pass along the fixed thread, so as to seem perfectly bisected, there must be an allowance made for the semidiameter of the thread or wire, because I suppose the index to be adjusted as before, to the inner edges of the wires; but it may, if it is found convenient, be adjusted to the middle of the threads, or else correction may be made in the observed distance.

In taking any angle, it is convenient that each of the parallel threads be about the same distance from the middle of the aperture of the eyeglass; and for this reason, the whole micrometer is contrived to slide to and fro, as the case requires. The same motion is also of use in taking the difference of right ascension and declination, by sliding the fixed parallel thread (on which the preceding star is brought to move) towards one side of the eyeglass; for by that means a greater angle may be taken in between the parallel threads, if need be. And it must always be remembered, that the moveable parallel thread should be set either north or south of the other, according as the following star is expected to be really south or north of the preceding.

In making an observation, either the inner or the outer edges, or the middle of the wires, may be brought to touch the objects; but then, it must be remembered to allow something for the thickness of the wire, in case the observation be not made from that part to which the index is adjusted. In observing the diameters of the sun, moon, or planets, it may perhaps be most convenient to make use of the outer edges of the threads, because they will appear most distinct when quite within the limb of the planet, &c.; but if there should be any sensible inflection of the rays of light in passing by the wires, this would be best avoided by using the inner edge of one wire, and the outer edge of the other. And in taking the distance or difference of declination between two stars, &c. the middle of the threads may perhaps be most convenient: but, however the observation is made, due correction must be allowed for the thickness of the wire, if requisite.

The difference of declination of two stars, &c. may be observed with great exactness, because the motion of the stars is parallel to the threads; but in taking any other distance, the motion of the stars being oblique to them is a great impediment, because if one star be brought to one thread, before the eye can be directed so as to judge how the other thread agrees to the other star, the former must be somewhat removed from its thread, so that in this sort of observations the best way of judging when the threads are at the proper distance is by frequently moving the eye backwards and forwards from one to the other: this method must chiefly be made use of when the distance of the objects is pretty large, and the motion or rolling of the eye great.

The micrometer is so contrived, that it may be applied to telescopes of different lengths; but then there must be a table for each telescope, by which the revolutions of the screw may be turned into minutes and seconds of a degree. In order to this, it is necessary that the threads of the micrometer should be placed exactly in the common focus of the object-glass and eyeglass, that is, where the images of objects seen through the telescope are distinctly formed. The readiest way of doing this is, first to slide the micrometer into the grooves fixed to the short brass tube which carries the whole apparatus of eyeglass, &c., and then to draw the eyeglass out by means of its sliding work, till the threads of the micrometer are in its focus, which is known by their appearing most distinct, &c.; then thrust the short tube before-mentioned into its proper place, as far as the shoulders of the brass work will admit, and place the object-glass in its cell, and looking through the telescope at some very distant object, slide the wooden tube in or out till you make the object appear most distinct, or till it has the least motion upon the threads when the eye is moved to and fro; for then the threads of the micrometer will be in the common focus of both glasses, and that will be the proper distance that the object-glass ought always to be at from the threads; and there should be made some mark or catch in the wooden tube, in order to set it always at the same distance.

The proper distance of the threads from the object-glass being thus settled, the table for turning the revolutions, &c. of the screw into angles or minutes and seconds of a degree, may be made several ways; but as good and easy a method as any is carefully to measure how many inches and parts of an inch the object-glass is distant from the threads, and with the same scale to find also how many inches and parts of an inch an hundred revolutions or threads of the screw of the micrometer are equal to: then, making the first distance radius, the last will be the sine or tangent of an angle answering to one hundred revolutions. And having the angle answering to one hundred revolutions, the angle for any other number will be easily known and set down in the table, as also the parts of a revolution; for in small angles, such as can be observed with the micrometer, their sines, tangents, or chords, are nearly in the same proportion with the angles themselves. The distance before-mentioned, (to be used as radius,) ought strictly to be taken from the threads to a point within the object-glass, about one third of its thickness from that surface which is towards the wires, if the glass be, as usual, equally convex on both sides; but if the focus of the object-glass is pretty long, and its thickness not great, the error that can arise by measuring from any part of the object-glass will become insensible as to the alteration in the angle.

The table for the micrometer may likewise be made by setting up two marks at a distance on the ground, and observing with the micrometer the revolutions, &c. which they subtend when seen through the telescope, and then computing the angles those objects subtend at the object-glass, by measuring their distance from each other and from the object-glass. The like may also be done by opening the threads to any number of revolutions, and then making a star move exactly upon the perpendicular thread, and noting the time it is passing from one parallel thread to the other; for that time turned into minutes and seconds of a degree, by allowing for the star's declination and going of the clock, &c. will be the angle answering to the number of revolutions; from which the whole table may be made. This method perhaps might be most advantageously practised in stars near the pole, where the apparent motion being slow a second in time will answer to a much smaller angle than towards the equator. But I believe, upon trial, the first method will be found most easy and practicable, especially if the scale made use of be well divided.

Extracts from Dr. Maskelyne's Observations on the Latitude and Longitude of the Royal Observatory at Greenwich.

[Philosophical Transactions, vol. LXXVII. p. 154.]

DR. BRADLEY having been furnished by Government in the year 1750 with a brass mural quadrant of eight feet radius, constructed by that excellent artist Mr. John Bird, an instrument far superior to any before used in the practice of astronomy, assiduously observed the pole star and other stars lying to the north of the zenith with it for upwards of three years, and then removed it to the opposite side of the wall, making it change place with the iron quadrant of the same radius constructed by Mr. Graham, likewise an excellent instrument, though inferior to this, and commenced a regular series of observations of the sun, planets, and fixed stars, which have been ever since continued in the same manner. Moreover, the temperature of the air, shewn by the barometer and thermometer, is affixed to each observation; and the zenith point of the quadrant settled from time to time by the help of a zenith sector of 12½ feet radius, turned alternately contrary ways, the same with which Dr. Bradley had before made his two useful and admirable discoveries of the aberration of light and the nutation of the earth's axis.

By the observations of the pole star and other circumpolar stars, above and below the pole, Dr. Bradley got the apparent zenith distance of the pole; by the apparent and equal zenith distances of the sun at the two equinoxes, having at the same time opposite right ascensions, as found from comparing his observed transits over the meridian with those of fixed stars, after the manner used by Mr. Flamsteed for deducing the right ascensions of the fixed stars, he found the apparent zenith distance of the equator, which lessened by parallax and added to the apparent zenith distance of the pole gave a sum less than 90° by the sum of the two refractions belonging to the pole and meridian zenith distance of the equator. But he remarked, that the difference of refractions, belonging to these zenith distances, would come out the same within 2″ or 3″ by any of the best tables then extant, whether deduced solely from observations, or partly from observations and partly from theory. The sum and difference of refractions answering to the pole and equator being thus given, the refractions themselves are given, the greater of which added to the apparent zenith distance of the equator gives the latitude of the place, and the less refraction added to the apparent zenith distance of the pole gives the co-latitude.

He afterwards, from the consideration that the refractions at the pole and equator may be taken without sensible error as the tangents of the zenith distances, according to Mr. Thomas Simpson's theory of refractions in his Mathematical Dissertations, divided more accurately the sum of the refractions at the pole and the equator into the just parts answering to each zenith distance, and thereby found the latitude with more exactness. In this manner he found the latitude of the Royal Observatory to be 51° 28′ 39″½, and the mean refraction at 45° 3′ to be 57″, the barometer standing at 29,6 inches, and the thermometer of Fahrenheit's scale at 50°.

But, not to let a matter of so much consequence rest on my assertion or memory, when further proof can be given of it, I have by me, in the handwriting of Dr. Bradley, among other particulars, his calculations of the latitude of the Observatory from his observations, according to the manner above explained; in which he first states it at 51° 28′ 38″, and finally more correctly in these words. " The apparent zenith distance of the equator, " by the mean of twenty observations in 1746-47, 51° 27′ 28″. The mean " apparent distance of the pole, by the observations made between 1750-52, " 38° 30′ 35″. Sum 89° 58′ 3″. Sum of refractions 1′ 57″. Polar refraction " 0′ 45″½. Equatorial refraction 1′ 11″½. Latitude 51° 28′ 39″½. Co-latitude " 38° 31′ 20″½."

The latitude of the Observatory being thus settled, as well as the quantity of refractions for all stars passing the meridian between the pole and the equator, Dr. Bradley readily inferred from his observations the true distance of all such stars from the north pole, which, compared with their zenith distances observed below the pole, gave the refractions at those lower altitudes. Finally, by comparing the refractions together observed in extreme degrees of heat and cold, he deduced the law of their variation as affected by heat and cold; and thus at length he inferred his elegant rule for determining the refraction in all circumstances, that it is to 57″, in the direct compound ratio of the tangent of the apparent zenith distance lessened by three times the refraction to the radius, and of the height of the barometer in inches to 29,6 inches, and in the inverse ratio of the degree of height of Fahrenheit's thermometer increased by 350 to 400.

But it may be proper to confirm this rule for refractions also from the same manuscript of Dr. Bradley, which I before cited for confirming the latitude, by the following passage, which immediately follows the other. “ Sup-“ pose the mean refraction at 45° $3' = 57''$, and $y = 350$; then

“ $y + t$: bar. :: $77''$: refr. at 45° $3'$.

“ Rad. : tan. ZD :: $3'$: m..... Rad. : $\overline{\text{tan. ZD} - m}$ :: refr. at 45° $3'$: r “ [the refraction required].” It is easy to see that this rule agrees with the other: for putting $t = 50$, and barometer $= 29{,}6$, the first analogy, putting the barometer down in tenths of an inch, is $350 + 50 = 400 : 296 :: 77'' : 56'',98$ for the refraction at 45° $3'$, or $57''$ within $\frac{1}{500}$th of a second. The second analogy serves to give the treble refraction nearly, called m. Whence it is evident, the last analogy coincides with the rule above given.

* * * * * * * *

For further confirmation of the certainty of the astronomical refractions, and latitude of the Observatory, as settled by Dr. Bradley, it may be proper to add, that the Greenwich brass mural quadrant underwent a trial, which all astronomical instruments ought to be submitted to, but which very few ever have been, on account of the difficulty and nicety of the operation, namely, an examination of the total arc; when it was found by Dr. Bradley to be an accurate quadrant, the arc appearing at one trial to differ only a fraction of a second from 90°, and another time, after an interval of above six years, to be a perfect quadrant. See p. 24. of Bird's Method of constructing Mural Quadrants, published by the Board of Longitude in 1768. In like manner he had before examined the total arc of the iron quadrant, first put up by Mr. Graham, for the use of Dr. Halley, in the year 1725, by means of a level, and found it to be 16″ less than a quadrant. See Bird's Method of constructing Mural Quadrants, p. 7. and Memoirs of the Royal Academy of Sciences at Paris, for 1752, p. 424. But this quadrant was, in the year 1753, re-divided by Mr. Bird, and, in this respect, probably rendered as accurate as the other. See Bird's Method of constructing Mural Quadrants, p. 24.

Dr. Bradley made a curious use of the new set of divisions, soon after they were laid upon the quadrant, to re-examine the error of the total arc laid down originally by Mr. Graham (which by the plumb-line and level he had found to be 16″ less than a quadrant in 1745) according to the following passage contained in the manuscript before cited.

" August 12, 1753, I measured with the screw of my micrometer the " difference of the arcs (of $\frac{64}{96}$) as set off by Mr. Graham originally, and by " Mr. Bird when he put on a new set of divisions upon the old quadrant, " and I found that Mr. Graham's arc was less than Mr. Bird's by $\frac{8}{40}$ divisions " of my micrometer, which to a radius of 96 inches answers to 10″,6; so " that the whole arc of 96 differs from a true quadrant 15″,9, which is the " same difference that I formerly found by means of the level, &c."

Let me further add, that Dr. Bradley had informed me, that he had found the same refractions, latitude of the Observatory, and obliquity of the ecliptic, by both quadrants, making a proportionable allowance, in the use of the iron quadrant, for the error of 16″ in the total arc in proportion to the zenith distance of the object before it was new divided.

The reverend Dr. Hornsby, F. R. S. Savilian professor of astronomy at Oxford, to whose care Dr. Bradley's original observations have been committed, in order to their being printed and published, having favoured me with calculations of the latitude of the Royal Observatory from observations of the pole star made with both quadrants, from a manuscript of Dr. Bradley, I think it proper to give it a place here, not only as a very curious paper, but also as strongly confirming the latitude of this place before stated.

Transcribed from a loose paper of Dr. Bradley.

" The mean zenith distances of the pole star above and below the pole, " corrected by refraction, aberration, &c. and reduced to January 1751, " O. St. as collected from the observations made after the new quadrant was " balanced, November 24, 1750.

Number of observations.	Above the pole.	Number of observations.	Below the pole.	to
	° ′ ″		° ′ ″	
23	36 29 46,66	8	40 33 2,95	Jan. 15, 1751
22	45,42	26	3,37	Aug. 27, 1751
23	45,13	27	2,14	May 4, 1752
19	44,63	28	1,67	Nov. 10, 1752
28	45,00	23	1,74	May 31, 1753
9	44,59	8	1,54	July 26, 1753
124	36 29 45,24	120	40 33 2,24	
			36 29 45,24	
			77 2 47,48	
			38 31 23,74	
		Error of collimation	1,74	
		Co-latitude	38 31 22, 0	

	° ′ ″ 77 1 15,8			
	38 30 37,9	Refraction.	° ′ ″	° ′ ″
Collimation	1,4	42″ +	36 29 2½	4 3 10,8
Refraction	+ 45,5	49 −	40 32 13½	+6,6
	38 31 22,0	6½	4 3 11	4 3 17,4
By new quad.	51 28 38,0	Refraction	+ 6½	
			4 3 17½	
			2 1 39—the mean distance from mean pole, Jan. 1751.	

The apparent zenith distance of the pole, by the mean of 310 observations, is

	° ′ ″ 38 30 36 allowing—2″ for the error of the line of collimation.		
Refraction	+ 45½		
	38 31 21½	Co-lat.	° ′ ″ 38 31 21½ by the new quadrant.
Latitude	51 28 38½		38 31 18½ by the old quadrant new divisions.

Apparent zenith distances of the pole observed with the iron quadrant.

1753	Apparent zenith distance of the pole.	Barom.	Thermom.	
			in.	out.
	° ′ ″	Inches.	°	°
Sept. 13	38 30 40,1	30,10	64	65
23	41,5	29,88	61	60
Oct. 2	42,5	29,67	56	53
5	41,0	30,02	57	57
11	41,1	29,48	61	57
19	40,2	29,82	50	43
31	40,8	29,65	42	34
Nov. 5	42,1	29,69	45	39
16	40,5	29,39	42	35
19	42,2	29,59	40	35
20	40,9	29,84	40	33
24	40,0	30,00	43	35
29	40,0	29,80	38	30
Dec. 3	39,3	30,16	39	33
8	37,4	30,06	35	27
17	41,5	29,50	50	49
30	38,0	30,11	33	22
Mean	38 30 40,5	29,81	47	41½
Refraction	+ 46,4			
Col.	− 8,4			
Pol. corr.	38 31 18,5			

So far the manuscript.

Thus the latitude by the brass quadrant being 51° 28′ 38″$\frac{1}{2}$, and by the iron quadrant with new divisions 51° 28′ 41″$\frac{1}{2}$, the mean by both quadrants is 51° 28′ 40″, or only half a second greater than settled in another manner, according to the manuscript of Dr. Bradley in my possession. Also the apparent zenith distance of the pole with the mean refraction 45″,4 being 38° 30′ 36″,1 by the brass quadrant, and 38° 30′ 33″,1 by the iron quadrant, the mean by both is 38° 30′ 34″,6, or only $\frac{4}{10}$ths of a second less than by Dr. Bradley's manuscript cited before.

REMARKS

UPON

JUPITER'S SATELLITES, MAYER'S TABLES, &c.

The reverend Mr. James Bradley's Observations on his Tables of Jupiter's Satellites.

[From the precepts to Halley's Tables.]

IN these tables we have determined the mean motions of the satellites, by comparing such of the oldest observations we could procure, as seemed to be the most accurate, with our own lately taken at Wansted; when Jupiter, after four revolutions, was nearly in the same place in his orbit. But comparing in like manner the observations at the distances of one, two, and three revolutions of Jupiter, we have sometimes found very remarkable differences in the motions of the three inferior satellites, especially in that of the second, or next but one to Jupiter.

It is not yet certain whether these inequalities do not in part arise from the excentricities of their orbits, and the motion of their apsides; but by what we can collect from the motions of the second satellite, it is probable they may be occasioned by the mutual action of the satellites on each other: for sometimes the motion of this satellite does in so short a time vary so considerably from its mean, that a small excentricity will not account for it; while on the other hand, the rest of the observations will not allow a great one. As far as we can hitherto find, the period of these errors nearly answers to the time the three inferior satellites take in returning to the same situation with respect to each other, and to the axis of the shadow of Jupiter, which is 437 days, or after 123 revolutions of the second satellite. After this period, the like errors return, nearly in the same order: but in the intermediate time, that is, after sixty revolutions, this satellite will deviate ten, twenty, thirty, and even sometimes forty minutes of time from its rate of motion during the seven preceding, or the seven following months. Now because the satellites are not found in the same place in the heavens after the aforesaid period is completed, it is possible these errors may vary somewhat on that account. And if the orbit of this satellite be likewise excentric, as the late observations seem to make it, the inequalities arising from both causes must be very intricate, and not easily to be separated by observation alone.

The errors of the first and third satellite are not so great, but seem to arise from the same causes; for they do not depend wholly on the excentricity. We have also remarked a sensible difference between the durations of the eclipses of the first, made at the different nodes, which were alternately longer and shorter; that is to say, the durations of the eclipses at the descending node in Leo, in the year $168\frac{2}{3}$, $169\frac{4}{5}$, and 1718, were at least 2h. 20′; whereas at the ascending node in Aquarius, in the years 1677, 1689, they did not exceed 2h. 14′; as it plainly appeared by comparing many observations of such immersions and emersions as were as near together as could be got. And it is manifest this difference did not arise wholly from the excentricity of the orbit, if it have any; but to what cause it is owing we are hitherto ignorant. In the mean while, till we can get more light in this matter from future observations, it were to be wished that some geometer, in imitation of the great Newton, would apply himself to the investigating these irregularities, from the certain and demonstrative principles of gravity.

From the observations we have of the fourth satellite, it is certain that its orbit is elliptical: and all our late observations are truly represented by supposing its greatest equation equal to that of the planet Venus, or 48′; and that its higher apsis was in ♓ 8° 00′, at the beginning of the year 1717. On comparing this hypothesis with the older observations of the years 1671, 1676, and 1677, the computations were found to differ greatly from the observations. But putting back the apsis to ♒ 14° 00′ at the beginning of the year 1677, those differences almost all vanished. Allowing it therefore an equable motion of 6° forward in ten years, the hypothesis was found to agree with the intermediate observations, for which reason we have followed it in our tables. And we find our numbers every where to agree with the heavens (except only two observations, both justly to be suspected) within the sixth part of a degree.

Our table of the equation of light is made on a supposition that the rays pass equably over a space equal to the diameter of the earth's orbit in fourteen minutes of time, and it answers to all distances of Jupiter from the earth, when Jupiter is nearest to the sun. But as the distance of this planet from the sun, when in its aphelion, is greater than its nearest distance by one fourth of the diameter of the earth's orbit, we found it necessary to add another table for the correction of these equations.

As to the latitudes of the satellites, it is certain from the late observations, that the nodes of the fourth are at this time in $11\frac{1}{2}$ degrees of Aquarius and Leo; and that those of the third lie very near them. We have therefore assigned the same places to those of the two inferior satellites, not having yet found any thing from our observations to the contrary. And if the nodes of the satellites were forty years ago in the fifteenth degree of Aquarius and Leo, where Cassini places them, (whose authority in this matter is of the greatest weight,) they will appear to have gone back about one degree in each revolution of Jupiter. We have retained Cassini's inclination of the orbits of the three inferior satellites to the plane of the orbit of Jupiter, 2° 55′; but we find the inclination of the orbit of the fourth to be somewhat less, that is to say, 2° 42′. It is certainly a very difficult matter to determine accurately the situation of circles so extremely small, nor is it to be undertaken without exquisite telescopes,

To Mr. Cleveland, Secretary of the Admiralty.

[Mayer's Tables, p. CIX.]

GREENWICH, Feb. 10, 1756.

SIR,

I HAD the honour of receiving your letter dated December 1, 1755, together with the theory and tables of the moon's motions and other papers relating to the method of finding the longitude at sea, which had been laid before the lords commissioners of the admiralty, by Mr. Professor Mayer, of the university of Gottingen.

In obedience to their lordships' commands I have examined the same, and carefully compared several observations that have been made (during the last five years) at the Royal Observatory at Greenwich, with the places of the moon computed by the said tables: in more than 230 comparisons which I have already made, I did not find any difference so great as $1'\frac{1}{2}$ between the observed longitude of the moon and that which I computed by the tables; and although the greatest difference which occurred is, in fact, but a small quantity, yet as it ought to be considered as arising partly from the error of the observations and partly from the error of the tables, it seems probable, that during this interval of time, the tables generally gave the moon's place true within one minute of a degree.

A more general comparison may perhaps discover larger errors; but those which I have hitherto met with being so small that even the biggest could occasion an error of but little more than half a degree in longitude, it may be hoped that the tables of the moon's motions are exact enough for the purpose of finding at sea the longitude of a ship, provided that the observations that are necessary to be made on shipboard can be taken with sufficient exactness.

The method of finding the longitude of a ship at sea by the moon hath often been proposed, but the defects of the lunar tables have hitherto rendered it so very imperfect and precarious, that few persons have attempted to put it into practice; but those defects being now in great measure removed, it may well deserve the attention of my lords commissioners of the admiralty (as also of the board of longitude) to consider what other obstacles yet remain, and what trials and experiments may be proper to be made on shipboard, in order to enable them to judge whether observations for this purpose can be taken at sea with the desired accuracy.

I am, Sir,

Your most obedient humble Servant,

JAMES BRADLEY.

To Mr. Cleveland, Secretary of the Admiralty.

[Mayer's Tables, p. CXI.]

GREENWICH, April 14, 1760.

SIR,

HAVING long deferred to make any report relating to the observations that were taken at sea by captain Campbell, in the year 1757, which you transmitted to me by order of the lords of the admiralty, I think it necessary to acquaint you, that, upon examining those observations, I perceived that they were not in all respects accompanied with such circumstances as are requisite for forming a right judgment of the accuracy and certainty with which observations proper for finding the longitude at sea by the moon can in fact be taken; for which reason I delayed giving my opinion upon this point till I could have an opportunity of comparing a greater variety of observations, made at different times, and with different instruments: such an opportunity having lately been given me by captain Campbell, who has favoured me with a copy of several observations that were made by him in 1758 and 1759, I now beg leave to lay before their lordships the result of the comparisons which I have made.

But before I proceed farther it may be proper to take notice, that since the time when I gave their lordships an account of the near agreement of Mr. Professor Mayer's lunar tables with the observations that had been then made at the Royal Observatory, I have compared several others, which concurred to prove that the difference between the observed and computed places nowhere amounted to more than about one minute and a half; and I find that the difference (small as it is) may yet be diminished by making alterations in some of the equations, whose true quantity could not be determined without proper observations: after making the needful corrections it appeared, by the comparison of above eleven hundred observations taken here since the new instruments were fixed up, that the difference did nowhere amount to more than a minute: it may therefore be reasonably concluded, that so far as it will depend upon the lunar tables the true longitude of a ship at sea may in all cases be found within about half a degree, and generally much nearer.

This fundamental and most important article being established upon such full evidence, it remained to be examined within what limits the errors arising from observations actually taken at sea could be contained.

In order to determine this point, I computed the ship's longitude from each of the observations made by captain Campbell, and, upon comparing the results of several that were taken near the same time and under the like circumstances, it appeared that in general the observer was not liable to err more than one minute in judging of the apparent contact of the moon's limb and the object with which it was compared. Now this being nearly the same error that would be found to obtain, if the like observations were to be made with the same instruments on land, it may hence be inferred, that in moderate weather the motion of the ship is no otherwise an impediment in this sort of observations, than as it renders the repetition of them more tedious and troublesome to the observer, which yet ought by no means to be omitted; because if each single observation be liable to an error of a minute only, by taking the mean of five or six, the error on this head may be so far diminished as to be of small moment.

But although the observations which I first received proved how nearly they could correspond with each other, yet they were not sufficient to determine whether the angular distances of the moon from the sun or stars were truly measured by the instrument. Observations, which were afterwards communicated to me, enabled me in some measure to judge of this particular; captain Campbell having, at my request, taken several distances between some of the principal fixed stars; but there still remained some doubt how far the observed distances of the moon's limb might be affected by the interposition of the dark glasses, which must be used in making those observations; and as this may be the source of considerable errors, not easily to be discovered or distinguished from others that may arise from the inaccuracy of the divisions of the instrument, or an improper adjustment, &c. I could only judge of the amount of those several errors, when blended together, by comparing the ship's longitude deduced from the observations taken on different days with the longitude per account, or, which was more eligible, with her longitude collected from her position with respect to some known point of land in view.

The observations which you sent me that were made by captain Campbell on a cruize near Cape Finisterre, between February 20, and March 2, 1757, (by the mean of above thirty taken on six different days or nights) gave the longitude of that Cape about nine degrees west from Greenwich, on supposition that the instrument measured the true angle; but if a correction was applied, which observations of the fixed stars that were taken afterwards seemed to require, then the longitude of that Cape would be about half a degree less: the books of navigation give its longitude about nine degrees and a quarter.

The observations above referred to were taken with the circular instrument constructed upon the plan proposed by Mr. Mayer, though they were not made in the manner he intended, captain Campbell thinking it too inconvenient to attempt to do it on shipboard; but as the principal use of this construction is to obviate the inconvenience proceeding from the inaccurate division of instruments, and as that might be sufficiently removed by the care and exactness with which Mr. Bird is known to execute those that he undertakes to make, a sextant of a radius twice as long as that of the circular instrument was made by him, and afterwards used by captain Campbell in taking several observations on board the Royal George in different cruizes near Ushant, in 1758 and 1759.

By the mean of twelve different days' or nights' observations, (great part of them being taken when Ushant was in sight, and the rest when the ship's bearings from thence might be pretty well ascertained,) I found the longitude of the middle of that island to be about five degrees and twenty-three minutes west from Greenwich.

In this deduction I supposed that the instrument shewed the true angle; but whether it did so or not, the mean result would be nearly the same, as the moon was sometimes eastward and at other times westward of the objects with which she was compared, especially if the error was not very great, and the number of observations taken on both sides was nearly the same.

It appeared by the near correspondency of the different days' observations that my supposition was not ill-founded, because the greatest difference between the mean result and that of any particular day amounted in one case only to thirty-seven minutes in defect, and the greatest difference in excess was but twenty-three minutes; so that the extreme results of these twelve days' observations differed from each other but about one degree of longitude.

This indeed is so near a correspondency as cannot reasonably be expected to obtain in general, and I would by no means attempt to infer from hence, that the longitude found by observations of this sort may in all cases be depended upon within one degree; but I beg leave to observe, that whatever extraordinary circumstances may have concurred to produce so near an agreement in this particular case, the event is such as may give reason to hope, however great the difficulties of finding the longitude by this method seem to be, that they are not insuperable, or such as ought to deter those whom it most nearly concerns from attempting to remove them.

I am, Sir,

Your most obedient humble servant,

JAMES BRADLEY.

The Result of several Observations of the Distance of the Moon from the Sun and Stars, taken by captain J. Campbell on board the Royal George, near or in sight of Ushant, with a brass Hadley's Sextant made by Mr. Bird; referred to in the foregoing Letter.

[Mayer's Tables, p. cxvi.]

Date of Observations.	Object, ☽'s dist. was observed from	☽	Long. of ship W. of Greenwich.	No. of Obs.	Ushant W. or E. of ship in longitude.	Longitude of Ushant.	
1758, Sept. 9.	⊙	E	5° 5′ W	6	W + 6′	5° 11′ W	
1759, June 20.	⊙	W	5 49	7	E – 4	5 45	
July 1.	⊙	E	5 55	5	E – 9	5 46	
18.	⊙	W	5 27	8	E – 5	5 22 ...	Ushant in sight.
19.	⊙	W	5 53	13	E – 23	5 30	
20.	⊙	W	5 31	4	E – 45	4 46	
29.	⊙	E	6 17	7	E – 32	5 45 ...	Ushant in sight.
31.	⊙	E	5 47	13	E – 11	5 36...	A great sea and strong gale. Ushant in sight.
Sept. 9.	Aldebaran	W	5 42	6	E – 32	5 10	
14.	⊙	W	5 40	7	E – 20	5 20	
15.	⊙	W	5 34	11	E – 10	5 24	
17.	⊙	W	5 39	10	E – 29	5 10 ...	Ushant in sight.

Hence the mean longitude of the middle of the island of Ushant is - - - - } 5 23

And the greatest error in excess 23′ by the observations of July 1, 1759; and the greatest error in defect 37′ by the observations of July 20, 1759.

MISCELLANEOUS PAPERS

AND

OBSERVATIONS,

NOW PUBLISHED FOR THE FIRST TIME.

MDCCCXXX.

MOLYNEUX'S INSTRUMENT.

A Description of an Instrument set up at Kew, in Surrey, for investigating the annual Parallax of the Fixed Stars, with an Account of the Observations made therewith.

[By Samuel Molyneux, Esq. F. R. S[a].]

THE manner in which it was intended to make this inquiry, was to repeat the observations related to have been made by Dr. Hooke, of the transits of the star Clara, in Vertice Draconis, over the zenith, especially when in conjunction with, and in opposition to the sun, viz. in the beginnings of December and June; and of such other stars as might conveniently be observed in the same manner: and it was proposed to make this observation with an instrument of as much exactness and accuracy, and as little liable to the difficulties attending Dr. Hooke's method, or to any other objections, as was possible. An account of his observations may be seen in his tracts, published in quarto, London, 1674.

This instrument was chiefly contrived and made by Mr. George Graham, watchmaker, in Fleet-street; it consists of a telescope, the focus of whose object-glass is 24 feet 3,15 inches, and the focus of the eyeglass about 4½ inch. The object-glass was very well centred, and a good glass. The tube which holds these glasses is made round, of strong firm tin plates, very carefully and closely soldered together, its diameter is about 4 inches, and within it was blacked, so as not to glare. At the object-glass end (see figure the first[b]) a ring of brass truly turned, about $\frac{3}{4}$ inch broad, was let on tight upon the tin tube, and well and truly soldered thereunto, and afterwards its face turned square unto (that is, at right angles with) the axis of the tube. The cell A which carried the object-glass was made very strong, and as thick as was necessary, about $\frac{1}{3}$ inch thick, and was carefully screwed on with six screws to the aforesaid brass ring. The object-glass itself was truly turned at its edges, and the groove of the cell which was to receive it was at first turned too wide for it; but afterwards, by lightly punching six or eight small holes in the inward surface of the groove, the glass was by these means brought to be truly steadied and fixed in its place, the periphery of the glass bearing in those places against the bur of the punched holes; the shoulder upon which the inward or lower surface of the object-glass rested was about $\frac{1}{10}$ inch broad, and the clear aperture which could be given to the glass was nearly 3,8 inch.

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[a] There are three copies of this description; the first is in Molyneux's own hand, and takes up about eight or nine small quarto pages of the observation book, marked K. 14; the second is a transcript of it by some amanuensis, with a continuation, of which the original has not been found in Molyneux's handwriting, but which he has made his own by the many additions and corrections he has inserted in it; these are incorporated in the third, which seems to have been written out fairly, and corrected by the author, for the press; it has therefore been followed in the present instance. There are a few pages at the end of the second, which have not been added to the third; these have however been printed in their proper place, and by a collation of the three copies, the text has in a few instances been either completed or corrected.

From this strong brass piece A, which carried the object glass, grew out two strong trunnions, or cylinders, BB, very truly turned, of the same size, so placed that they had one common axis, which passed through the centre of the object-glass, as near as possible, both as to the breadth and thickness thereof; and upon this axis the whole telescope hung suspended from above, as shall be now described.

A square hole having been made through the floors of the house, a wainscot tube was continued perpendicularly through from the garret to the ground floor, about nine inch. square without, and seven within. Perpendicularly over the centre of this tube, and independent entirely from it, was fixed some iron work, very strongly, by proper bolts, nuts, and screws, to an adjacent very large stack of brick chimneys, which were quite within the house, and scarce at all exposed to the weather, and were very strong old built chimneys, some part of the house being near three hundred year old; that part of the iron work which hung over the square tube was a strong iron hoop, C, above $\frac{1}{4}$ of an inch thick, set edgewise, of about seven inches diameter, the upper edge of this, which was set truly level, supported some brass work which carried two strong brass notches, DD, which notches were right angles, and in these the above-mentioned trunnions or axis was placed, and from them the whole tube depended. There was a provision made in this brass work in the first place by strong but fine screws, EEE, to bring this axis truly level, and to reduce it to be level again at any time, should the iron work give or yield any way so much as to require it: the said screws at EEE raising or depressing the brass plate dd which carried the notches DD in which the whole tube was suspended. The screw at the east end was of an exceeding fine thread, and is represented with a larger head than the other two. In the next place, provision was made so as to be able to give the whole brass work, with the tube depending from it, a motion horizontally round its axis, and to fix it in any given situation, so as thereby to set the axis, or trunnions, duly east and west as was designed. This was done by means of the screws at F, by which the whole plate of brass marked GHGGH, and consequently all the brass work above it, and the notches in which the whole tube was suspended, could be rotated round the axis of the tube. There were three screws which went through slits in this plate GHGGH and screwed into the iron hoop C, which hoop sustained the whole, as hath been said, and these fixed the whole brass work in its true situation when once found: the heads of two of these screws appear at HH. The west trunnion had its upper part chamfered off exquisitely to its centre, in an angle of about 45° with the axis; the lower half thereof was filed away about $\frac{1}{10}$th of an inch inward; exquisitely at the centre, an exceeding fine notch or scratch was made on the upper chamfered part, just large enough to receive a fine silver wire, whose diameter was about $\frac{1}{284}$ inch. and this wire was thus fixed. About $\frac{1}{4}$ inch above the notch was a round pin, one edge of which was fixed opposite to the notch for the wire, and the wire being brought round that edge was turned to a small screw, which pinched and sustained it; and for better security, a small thin bit of brass was laid on above the wire on the upper chamfered edge, so as to pinch the wire exquisitively in its notch in the very centre of the axis; and the utmost care was taken of this part, that the wire might not be possibly at any time displaced from this notch, but always perfectly truly delivered from the very same point, viz. the centre of the axis. To this wire a plummet hung, quite below at the eyeglass end, for adjusting the whole as shall be hereafter described: KKKK represents the tin tube, M the object-glass, NNNN the square trunk or tube of wood which went from top to bottom of the instrument[c], through the floors of the house, as hath been said; O represents a hole in the plate GHGGH whereby the wire and plummet were let down. At the lower end of the tin tube was also truly soldered another ring of brass, as at the top, about $\frac{3}{4}$ inch broad, and its face also turned truly square to the axis of the tube. To this was carefully screwed on with six screws a strong cell of brass, which was to carry the cross threads of silver wire. The wire made use of for these threads was less than $\frac{1}{310}$ inch, and great care was taken before the tube was put up that those threads were exactly at a proper distance from the object-glass. This cell had a groove truly turned to receive a plate or ring of brass, which carried these cross threads, and being placed in the groove intended for them, they were screwed fast with four screws; care was taken before the tube was put up, to bring one of the threads truly parallel to the axis of the trunnions at the other end, though this was not necessary to be exquisitely true. Flat against the lower face of this lower cell a plate of brass was screwed, which is represented in figure the second[d], DE the width of the ring that carried the cross thread, AB parallel to the north and south thread about $4\frac{1}{2}$ inch asunder, C at the east end of the east and west thread.

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[b] There is no such figure in the manuscript: between the leaves of it there were three loose papers, which seem to refer to the construction of the instrument, and which have therefore been reduced and copied, (see figs. A, B, C,) but they had no letters of reference affixed to any parts of them. To prevent ambiguity, the numbers 1 and 3 are avoided in the designations of the figures on the plate.

To the same stack of chimneys below, with proper bolts, nuts, and screws, some iron work was fastened in like manner as above, which carried a like iron hoop set edgewise, and truly level, in the same manner and of the same dimensions as above. This iron hoop carried some brass work, which was so contrived as that it was also capable to be turned round on its centre, and fixed in any situation; and the upper plate of this brass work made a sort of square frame, lying horizontally, and wherein the above delineated piece CAB, fixed, as hath been said, to the tube, could freely move, every way being about $\frac{3}{5}$ inch, or more, bigger than the square of AB. On this frame was fixed a fine tender spring, which pressing on C kept the side AB flat against one side, viz. the west side of this frame, and this spring could be discharged at pleasure, so as to leave the tube to hang freely by its own weight on the axis above. The side AB was filed a little hollow, so as to touch only in the points A and B. This frame also had a provision made to move it, and consequently to move the tube with it gently and steadily backwards and forwards, in the direction CDEF; that is to say, eastward and westward, which was necessary, as shall be hereafter explained, in order to bring the telescope to hang at a convenient distance as to east and west from the plumbline: a hole was left in this brass work, perpendicularly under the point of the western trunnion, from whence the plummet hung, to let the plummet and plumbline of silver wire go through. Upon the above described piece ABC, about midway between A and B, was screwed down a small thin piece of brass, about 2 inch. long, $\frac{3}{4}$ broad, and about $\frac{1}{10}$ thick, one edge of which overhung the edge AB, and also overhung the hole for the plumbline, and this edge was as near as could be at the same distance from the centre of the cross threads, as the notch which delivered the plumbline above was from the centre of the object-glass.

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[c] Smith, in his Complete System of Optics, §. 1200, says, "Having by me a memorandum "that I took [of the structure] of Mr. Molyneux's [instrument] when I saw it at Kew, I will "endeavour to give the reader a general idea of it;" to illustrate his description he gives a drawing (fig. 687 of his work). It is coarsely and imperfectly made; but, having been designed by one who had actually seen the instrument, it afforded the means, in some measure, of supplying from authority the want of the figure which was evidently intended to have been annexed to the MS. Fig. 5. at the end of this volume is a copy of Smith's drawing; and it may not be unnecessary to mention that the projection on the side of the bottom of the tube is intended to represent an eye looking through a microscope at the dot under the plumbline.

[d] Fig. 2. is taken from a rough outline in the observation book K. 14; it is wanting in the two other copies.

Upon the edge of this brass piece, midway between the upper and lower sides thereof, was made a very small mark, by drilling a conical hole therein with the point of a very fine drill, it was truly round at the out edge, and the outward diameter thereof was in respect to the diameter of the wire used for the plumbline nearly as 3 to 2. The diameter of the wire was, as has been said, about $\frac{1}{284}$ inch, and the hole might be $\frac{1}{200}$ of an inch. This hole or mark was filled with printer's black, to make it more distinct and visible, and that the edges might be more truly defined and distinguished. By this mark and the plumbline, the whole telescope was adjusted and reduced from time to time to the same situation, as shall be hereafter related. To the above-mentioned square frame of brass, which served to retain and guide the piece ABC and the telescope in their situation as to E. W. N. and S., to this frame, I say, just in the middle of the north side, was fixed a small socket horizontally, which carried a small brass screw, which was very truly turned, and touched with its point, which directed southward, the north point of the brass cell at the lower end of the tube, just at the height where the cross threads were placed, and the telescope was gently pressed against the point of this screw, by being drawn always northward by a small string tied round the tube about four inches higher than this brass work, which string was brought over a fine pulley, and had a small weight hanging to counteract against the screw; this screw had 42 threads in an inch, and the head thereof being divided into 17 equal parts, each part was equal to one second, on the radius 24 feet 3,15[e] inch. By turning of this screw the whole telescope could be moved very pleasantly and easily the whole range, truly north and south, as far as the square frame would allow the side AB to move, which was about 8′ or 9′. The spring at C in the mean time bearing the whole softly and pleasantly against the west side of the square frame, so that the motion of the telescope was then exactly in a meridian. To the same square frame, and at the same side, viz. towards the middle of the north side, just below this screw, there was placed in the same nature and direction a coarser screw, by which in like manner the graduated or second screw, but now mentioned, being discharged, the telescope might in the same manner as with that finer screw be moved its whole range north and south,

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[e] There are two loose papers in Graham's hand-writing; the first has been folded, and endorsed by Molyneux, "Mr. Graham's dimensions of part of the parallax instrument," it contains the following particulars:

"Radius=24 feet 3½ inch. nearly ,084797 + = 1′
"Screw 42 threads in one inch ,001413 + = 1″
"One thread ,0238 the plate to be divided into 17.
"$\frac{,0238}{17}$=,0014=to one second nearly, not differing one second in 100 divisions.

"If the eyeglass be 2 inches broad, it will take in an angle of 23′, which will be 2′ 28″ in time, "in passing the cross hairs at the zenith, the half of which will be 1′ 14″.

"If the screw at the top have 40 threads in one inch, and the plate fixed to the screw divided "into 100, one division will alter the tube at the lower end about ,011 of an inch."

On the other paper we have,

"From the middle of the object-glass to the cross hairs, 24 feet 3,15 inch.*

24	3,15
	,25
	,5
24	3,9
	1,5
24	2,4

	lb.	oz.
"The weight of the telescope, with all that is fixed to the tube - - - -	22	12
"The brass work upon the upper hoop of iron - - - - - -	4	9
"The whole weight upon the hoop - - - - - - -	27	5

Plummet

* Against these numbers Molyneux has written, "This is exact."

this coarse screw was made not only to save the finer screw, (by which the alterations of the situation of the telescope and of the star were proposed from time to time to be exactly measured,) but also was designed for this purpose, that is to say, that when the telescope had been brought by the fine screw, so as that the star was bisected by the east and west thread, and the mark also bisected by the plumbline in the beginning of December, the telescope might be bore off a little from the truth, that the star might become visible at its first entrance, without which provision, did the star come into the glass directly on the east and west thread, as it ought to do for several weeks, insensibly altered at that time of the year, it might perhaps not be at first perceived, the observation being at that time of year to be made about midday. The snout which carried the eyeglass was cocoa-tree, screwed on to the brass work; see figure the third. The instrument being thus described in itself, I shall now proceed to relate what occurred materially in the fixing it truly in its place, and adjusting it to be ready for observation.

1725, *November* 26.

In the first place, it was set up on the 26th day of November 1725, about 12 days before the sun and the star to be principally observed, Cap. Draconis, were in the same right ascension; and it should be here observed, that this star passing now about 3′ ½ north of the zenith, the centre of the iron ring in which the lower end of the telescope moved, was placed and fixed to the wall so much meridionally south of the centre of the upper ring, as that the axis of the telescope, when its lower end was in the centre of the said

	oz.	dwts.		
Plummet	2	8		
Little one	0	4	19	
	2	12	19	troy.

"The plumbline wire=284 in an inch, wound round a brass cylinder, mark as 3 to 2 of wire "nearly."

This last line is added by Molyneux; on the back of this second paper Bradley had written,

Rad.=24 feet ..3,15 inch.=291,15 inch. 2,4641168
7,5358832
1,7781512
−6,4637261
−1,6232493
1,2270690

1 Revol.=$\overset{\prime\prime}{1}6,868$

ring, and of the work below, might be directed to that star in its transit of the meridian[f].

The telescope being thus set up in its place, the next thing we did was to place the axis or trunnions by which it was suspended at the object-glass end, as near as we could, duly east and west, and this was done by deriving an east and west line from a meridian line, which was drawn by the sun above in the garret, while its transit over the meridian was carefully observed by a meridional instrument, made use of in the library below.

1725, *November* 27.

This day having made a proper kind of chair or couch for the observer to lie easily upon, while the eye was directed to the zenith, the chair being upon the ground-floor, and the snout of the tube about 3½ foot from the floor, the transits of some small stars were observed through the instrument, about seven o'clock that evening, giving light to the cross hairs, by a candle placed near the object-glass above, by which it was soon perceived that the axis was not exactly east and west, but that it had been set so as that the east end was too much northerly, and the west end too much to the south, a small matter, and this being the first observation made with the instrument, it is proper to take notice here, once for all, that the observer lay upon the forementioned couch always with the head eastward. Though the abovementioned error was perceived, yet no attempt was made to correct the direction of the axis, till there was provided a more convenient apparatus for enlightening the cross threads, as shall be hereafter described; so that the next thing which was done was to hang on the line and plummet; and this was performed as followeth.

The plummet consisted of two parts, the one smaller, of cylindrical shape, weighing about five pennyweight troy, the other part weighing about two ounces and eight pennyweight troy, both made of brass; of the annexed figures[g], truly and smoothly turned, the big one was so contrived as it could be readily hooked on and taken off from the bottom of the upper one, and both together weighing near two ounces and nine pennyweight troy, were about as much as the wire which was made use of for the plumbline could sustain in water; the smallest of these was first hooked on to a small loop made at the end of the wire, then softly unwinding the wire from the wooden cylinder upon which it had been originally drawn, it was let to descend gently down, until it came through the hole described to have been left for it in the brass work below, and from thence descended about a foot lower than the snout of the telescope, and consequently about 18 inches lower than the mark described to have been made on the brass piece fixed to the tube; then the wire was gently laid into the afore-mentioned notch in the centre of the west axis above. A little lower than this weight was firmly fixed into the adjacent brick wall a small wooden shelf, to hold very steadily a cylindrical vessel of water, about six inches diameter, and about six inches high, the centre of which was placed under the plumbline; then the small plummet was lifted up, and the larger one hooked on to it, and the cylindrical vessel being filled with water, the whole plummet was let gently down, and hung from above near about the middle of the water; then the small thin bit of brass above described was carefully laid on and pinched fast, to confine the wire exquisitely in the notch upon the chamfered edge above. It being absolutely necessary to move the plummet and wire out of the way of the observer, when there was occasion to place the chair for observing under the telescope, and to make use of the instrument, the same was performed very readily and easily in the following manner.

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[f] The first copy of the description ends here.

[g] See fig. 4. A is the small, and B the large plummet.

First, the plummet was carefully lifted out of the water, and then taking off the larger or lower part, the remaining small cylinder was carefully lifted up, and the wire from its elasticity, acquired in the manner of drawing it, coiling itself a little upwards, the bottom of the small plummet was placed in a little round socket of wood fixed in a convenient part under the iron ring, so as to remain there quite out of the way during the time of observation, and then the vessel of water was moved away to place the chair, and whenever there was again occasion to make use of the plumbline, the larger plummet could be again readily fixed to it, and immersed again in the water for examining and adjusting the situation of the whole as aforesaid. It is proper here to take notice that in all the observations from the first to the day of this plummet, weighing as hath been said, was always made use of; but on that day of the wire was broke by accident below, and ever since we made use of a thicker wire and a heavier plummet[h]. It hath been already mentioned, that the square brass frame through which the end of the telescope passed below, had a provision made by a fine screw, to move it gently; and also the whole tube with it, backwards and forwards, east and west; this was necessary, in order to bring the small mark on the little brass edge above described, to come to a due distance from the plumbline, when it hung steadily: for had this edge with the mark been too far east of the plumbline, it might have caused some error in the observer in judging when the mark was truly adjusted to and bisected by the plumbline; and had it come too much westerly, the plumbline might have borne upon it, and not reduced itself to so constant and true a perpendicularity as it ought to have done.

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[h] This sentence is an addition made to the third copy by Molyneux, who evidently intended to insert the dates and weight at some subsequent opportunity. The plummet was stated in

When these things were thus placed, a small apparatus was fixed to the adjacent wall, carrying a light which shone always from the same point west of, but somewhat above the height of the mark, and about 4 inch. from it; and the glare or reflection of this light on the bright edges of the brass, and of the mark, was taken off by the interposition of an oiled paper, in order to make the hole or mark appear more distinct. The same apparatus also carried a small tube, about 6 inch. long, placed horizontally due east and west at the height of the mark, having a convex glass of about 1½ inch focus at one end, truly directed unto the mark thus illuminated, and at the other end of this small tube was a small hole about the bigness of the pupil of the eye; and by this apparatus the situation of the mark, in respect to the plumbline, was always examined and adjusted from time to time, so that it is impossible that any sensible parallax or error could attend this adjusting thereof; the light, the glass, and the distance, and situation of the observer's eye, always being exactly the same, and perpendicularly directed to the mark in every examination.

The only difficulty which was found at first in this manner of examining and adjusting the mark by the plumbline, was from the motion of the plumbline, which, notwithstanding the assistance of the vessel of water below, would continue to vibrate in exceeding small arcs for a very long time, and hardly ever settle to our satisfaction; this made it necessary to make a cover to the water, in order to render it more steady, and also by pasting paper wherever it was possible on the fissures and joints of the

page 100, to have been "near 2oz. 9dwts. troy." In the account of the observations made at Kew, it will be found that the plumbline was broken on the 30th of May, 1726. This is probably the accidental fracture here referred to; on the following day, May 31, a new and thicker wire was put on, and on June 8, a heavier plummet was hung to it.

square wooden trunk, all current of the air was stopped in all parts thereof; and this also made it necessary to stop all the smallest passages of air at the door and windows of the chamber, and also to enclose, in a manner, the whole end of the room where the tube hung by thick and close curtains; by these methods it was found the plumbline would settle much readier than before, and would in the space of about two or three minutes make its vibrations so small, that they would not exceed above two or three second minutes of a degree; and these vibrations, from the length of the plumbline, being so very slow, and viewed with a glass of $1\frac{1}{2}$ inch focus, as hath been said, it was very easy to judge of their out limits, and, consequently, to judge where the true perpendicular lay, and to adjust from time to time the mark accordingly; and in about three or four minutes, and much sooner when we began to use the heavy plummet, after the day of it would come to settle absolutely at rest[i].

It was afterwards found, that all these precautions were in blowing windy weather scarce sufficient, so that at last a tube or trunk of wood was made down to the very floor, very close and tight, which could be readily put on and taken off, and the necessary passages were left in it to view the mark and plumbline, and to handle the screws; so that after this trunk was made, the situation of the plumbline and mark was generally observed before this trunk was taken off, and it was found that this contributed very much to the steadiness of the plumbline. However, after the day of it was seldom found necessary to use this case at all, the plumbline generally steadying itself without it[k].

When the weather was perfectly calm, the plumbline with either plummet would hang sometimes perfectly motionless, although the trunk was not on. The very breath of the observer would frequently put it in motion a small matter; and we observed that when two or three persons stood near the tube, it would never settle so soon nor so steady, although they stood perfectly still, as when the observer was alone in examining it: this might proceed from the different temper of the air in the garret, and in that room, on account of the warmth and breath of the persons, which caused a draught of air down the wooden trunk, and gave this exceeding small motion to the plumbline.

[i] See note [h].

[k] From what has been said above, the date here omitted is that of the putting a thicker wire and heavier plummet on the instrument; see note [h].

The next point, and upon which indeed the truth of the whole depended, was with all these precautions to examine how consistently it was possible to adjust the mark to the plumbline at different trials, moving it on each side away and bringing it to again, and also taking off the plummet frequently, and lifting up the plumbline as in a state of observation. And this we frequently examined by means of the graduated fine screw, which hath been described before, and on whose point the north part of the telescope gently bore; we repeated this examination several times over and over again, perhaps ten times in half an hour's time, and by all the different trials that were made at different times by different observers, it was very certain that this could be done to the certainty of less than one second, and in ten trials made immediately subsequent one of the other, seldom would differ half a second.

This extraordinary exactness may appear almost incredible, but certainly it was so in fact; and that so small a quantity was distinguishable, it is hoped will plainly appear to those who will duly consider what is above related of the nature of this instrument, and the manner of examining its consistency and accurateness; and, which perhaps is still more surprising, all our subsequent observations have not only confirmed the same, but also have shewn that the alterations arising in the situation of the instrument, from the yielding and motion of the stack of chimneys, to which the whole was fixed, were inconceivably little in long spaces of time, the same not having amounted in a whole month's time, from the 26th of November, when it was first fixed up, to above four seconds, although during that time it was almost every day frequently examined.

For the better understanding the subsequent observations, it must be here noted once for all, that the graduated or second screw, which hath been above described to have been divided into seventeen equal parts, each equal to one second on the rad. 24 feet 3,15 inch. was numbered so as that increasing the number unscrewed the screw, and brought the snout northward, and consequently brought the telescope to point more southward, and nearer to the zenith.

1725, *December* 2.

Having by this time provided somewhat a better apparatus for enlightening the cross thread from above, which shall be particularly described the day that it was brought to be perfect, and as it was to continue during the whole series of the observation; and having observed several stars pass this night, and particularly the star in Capite Persei, the axis, and consequently the threads below, were brought by frequent trial to lie sufficiently truly east and west; and it was found that the stars were passing through the whole glass about three minutes and two or three seconds in time; so that the glass took in about 28′ absolute space.

1725, *December* 3.

This day a clock was fixed up, going sidereal time, in the chamber where the lower end of the tube was, and about 18 minutes after 12 mean time, the sun at that time shining very bright, the star Clara in Vertice Draconis, or Caput Draconis, was seen for the first time passing the glass very visibly; however it appeared but weakly and indistinct, the whole surface of the object-glass, of near $3\frac{3}{4}$ inch. diameter, having been left open without a proper aperture. Then by turning the graduated screw, the telescope was brought so as that the star appeared to be duly bisected just on the cross, and leaving it in that situation, the index of the graduated screw was found to stand at 10,5, that is to say, the index pointed 10 and 11, about half way between the two divisions, the decimal 0,5 being parts of a division, was guessed at by estimation, as was done also in all the subsequent observations. Then immediately loosing the screws which fixed the small brass piece, carrying the mark on its edge, it was shifted by its own screws, touching no other part of the instrument, till the mark was brought to be exactly bisected by the plumbline, and then it was fixed down so, that it could not stir from that situation. This was all done and finished by about one o'clock; which is here taken notice of, because by the subsequent observations it will appear noways probable that the telescope altered its situation in any sensible degree during this hour, and consequently it is probable that the mark was fixed very near to agree with the plumbline, when the star was on the thread this 3d day of December, 1725. The mark hath never been in anywise altered in respect of the tube, since that day when it was first fixed, as hath been now related.

1725, *December* 5.

This morning proving very fair and clear, it was resolved to examine the situation of the mark as fixed on the 3d of December, so that about half an hour after 11 mean time, the instrument was adjusted as follows: the index was found unaltered as it had been left on the 3d of December at 10,5; then by the graduated screw the telescope was moved till the mark was truly bisected by the plumbline, and then the index stood at 9,5; the wind blew violently at north east, and the plumbline vibrated at least a second and a half on each side the mark, but now and then it settled as above, the index at 9,5; so that notwithstanding some very stormy weather, which we had on the 4th of December, the telescope was not altered above one second, by which the upper end of the telescope had gone southward since the 3d of December.

This day a pasteboard aperture was prepared, to cover the object-glass, of 2,75 inches diameter, but being still apprehensive that the covering the glass might make the star less readily visible, it was not at first put on; but having first supported the snout of the telescope upon the coarse screw, a small matter south of the fine screw, to the intent that the star at its entrance into the glass might be more readily seen; at about 56 minutes after 11, mean time, it was perceived to enter, and instantly discharging the coarse screw, so as that the telescope returned to be supported by the graduated screw, and the star seemed to be sufficiently true upon the thread. The greatest part of the time of this transit was taken up in putting on and taking off the aperture, which was easily done by calling to the assistant who attended above; and the star always appeared much distincter and better when the cover was on, than when it was off; for we found that when the whole glass was left bare, there came in so much light, that it very much effaced the small light, which diffused itself from the star on each side of the thread; and the star became quite lost and invisible, nor could it be perceived any ways sensibly to alter the situation of the star, even when held somewhat excentrically; upon the whole, it was judged that the mark had been so fixed on the 3d, that when the glass was brought true to the mark by the plumbline, the star would certainly at this time of the year appear upon the thread, and not expecting any greater accuracy to be possible, which however, in some tempers of the air, was afterwards found very practicable, nothing more was taken notice of in this observation; however, by farther experience it hath been found, that the star might appear upon the thread, and if its exact bisection, and also the part of the thread where it was so bisected, was not adverted to, its exact situation might differ above a second and a half, so that this observation must be considered as doubtful, though it is most probable that the mark and the star did very nearly agree with one another. In five or six minutes after the transit, the situation of the instrument was examined again by the plumbline, and it was found that during the observation, the glass had scarce sensibly altered its situation, notwithstanding the very high wind, for when the observation was begun, the index stood at 9,5. and now it was found at 9,8. so that it had scarce sensibly altered.

1725, *December* 10.

On Tuesday, December 7th, there was in the morning a very violent and unusual hurricane, such as hath not been known in many years, and from that time to this day a very great change in the weather, from cold, dry frost, to mild, open, rainy, warm weather. Notwithstanding all which changes, when the mark was adjusted this day again to the plumbline, the index which was found at 9,8. as it had been left on the 5th of December, was altered only to 11,5. so that the stack of the chimneys, with all this change of weather, had altered but about a second and a half, by which the top of them was come northward. Some sudden clouds prevented the observing the star this day, but about 11 o'clock at night, examining the instrument again, the index was found still to stand at 11,5.

1725, *December* 11.

This morning the apparatus was finished for enlightening the cross hairs, which is now to be described. In a thin piece of board, about 4 inches square, a round hole was turned truly in the centre, and of such a diameter as exactly to fit the upper part of the brass cell which carried the object-glass, and to steady itself truly horizontally thereupon; to two sides of this square piece of board were fixed on, truly square, two other pieces or sides, which stood upright above it, the upper edges of which pieces were cut so as to make an angle of 45° with the piece below, and consequently with the horizon; upon these two edges was nailed a thin piece of pasteboard, and about the centre of the said pasteboard, and consequently perpendicularly over the centre of the object-glass, was cut out an oval hole, whose short diameter was 2 inches and $\frac{5}{10}$, and the curve of the hole was the section of a cylinder of that diameter, at an angle of 45° with its axis; by this means an oval annulus of this pasteboard overhung the object-glass[1], near half an inch broad at each side of the short diameter, and proportionably all round. The inward or lower side of this pasteboard was strongly rubbed over with a black lead pencil, by which its reflective quality was very much increased; then this frame, with this aperture which it carried, the whole of which was very light, not weighing half a pound, was laid on in its place upon the cell of the object-glass, and it was so placed that the long diameter lay always east and west, and the short one north and south, the inward or lower surface respecting the east; then a bench or shelf was raised from the floor of the garret to the east of the object-glass, about five foot long, and pointing directly towards the object-glass, and the shelf was made of such a height as that a square lantern, containing a lamp and a very deep convex glass, to gather its light, being placed thereupon, the light thereof was thrown strongly and directly horizontally upon the inward surface of this pasteboard, and consequently was from thence reflected directly down the telescope, and by drawing back or approaching this lantern, it was very easy to give a weaker or more intense degree of light to the cross threads at pleasure; and at all distances and different degrees of light it appeared very equably diffused over the whole area of the cross threads. The above described aperture was from this time constantly made use of in every observation, the frame being always placed exactly in the same situation.

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[1] The description of this apparatus, which came afterwards into such general use, conveys the idea of its having been now originally constructed for the sector at Kew.

About the time of the star's passing, after a very violent stormy rainy night, Mr. Graham examined the instrument very carefully about 45 minutes after 11 mean time, and having adjusted it to the mark, he found the index stand at rather less than 11, so that it had altered since the day before but about half a second, by which the top of the chimneys had yielded southward.

At about 17h. 48′½ sidereal time the star Cap. Draconis entered, the snout being supported by the coarse screw; then discharging the coarse screw, and letting the telescope bear on the graduated screw, it seemed, as on the fifth instant, sufficiently well bisected; then Mr. Graham coming to the glass, it seemed also to him very truly upon the thread.

At 17h. 50′, he saw it pass the cross thread, and at 17h. 51′½ he saw it go out, and he judged that it went out rather a small matter to the left of the thread, that is, in appearance south, but really north, of the east end of the thread.

At 12 hours mean time, he again examined the instrument by the plumb-line, and, when adjusted, he found the index stand exactly as it had done before the observation, at a little less than 11.

During the time of the transit, the star appeared to him so true to the thread, that, by his estimation, one could hardly err 2 seconds in judging when it was true thereunto; and as to adjusting the instrument by the mark to the plumbline, he was very positive that there could never be an error of one second.

1725, *December* 12.

After an exceeding tempestuous rainy night, Mr. Graham again examined the instrument, and adjusting again the mark to the plumbline, about forty minutes after eleven mean time, he found the index stand at 10,2. whereas the day before it had stood at 11. so that it had not altered one whole second in that time, by which the top of the chimneys were come southward.

At 17h. 48′ 46″ sidereal time the star entered, the snout being supported by the coarse screw as before; then discharging the coarse screw, and letting the telescope bear on the graduated screw as before, it seemed again to be sufficiently well bisected; Mr. Graham then coming to the glass, as was done the day before, the star was so truly on the thread, that at first he could not perceive it, then he turned the graduated screw till he could perceive an alteration by his eye in the situation of the star on the thread, without knowing how much he altered the said screw; and an assistant standing by to observe how many divisions he had altered it, what he altered it did not amount to much above one second, when he said he saw the star distinctly adhering to one side of the thread, but still a sensible part of the glare appeared also on the other side of the thread; then he altered the screw, and brought it to be again bisected a little time before it went out, and then the index stood at about 10,5.

At 17h. 51′ 50″, he saw the star go out well on the thread.

About 12h. 8′ mean time, he examined it again by the plumbline, and then the index stood at 10,3. so that during the time of the observation it did not alter $\frac{1}{6}$th of a second.

1725, *December* 17.

This day, Mr. Bradley, one of the astronomy professors at Oxford, who had been acquainted with the whole contrivance of this instrument, and was present when it was first set up in its place, did with great care examine the situation thereof.

He found the index at 10,3. where it had been left on the 12th of December, and adjusting the mark to the plumbline, about 11h. 15′ mean time, he found the index stood at 9,7. so that from the 12th of December to the 17th, it had been altered but a little more than $\frac{1}{2}$ a second.

At about 17h. 48′$\frac{1}{2}$ the star entered, the snout being borne by the coarse screw as usual; then Mr. Bradley going to the glass and discharging the coarse screw, and letting the telescope bear on the graduated screw, about 15 seconds before it came to the cross, it seemed to him on the thread, but rather too much in appearance to the right hand; and it seemed to him to run thus along that edge of the thread till about 15 or 20 seconds, after it was got beyond the cross, and then seemed by degrees to recover the thread, and to appear pretty truly bisected about 8 or 10 seconds before it went out.

At 17h. 51′$\frac{1}{2}$ he saw it go out.

At about 11h. 50′ mean time, he examined the situation of the telescope again by the plumbline, and found it not sensibly altered during the time of observation. Then by very frequent trials, he tried within what limits of certainty it was possible to adjust the instrument, and was satisfied that one could be very sure to adjust it always within $\frac{1}{2}$ a second.

At about 12h. 10′ sidereal time, that evening, he observed a pretty bright telescopic star pass along the thread, being then illuminated in the manner as hath been above described. As to this star also, he thought that the star agreeing with the thread at the entrance, did, as it drew near unto the cross, bear off somewhat to the right hand, and seemed to return to the thread, by degrees, in the same manner as Cap. Drac. appeared to him in the morning.

At about 2h. 33′ sidereal time, a star, in Capite Persei, of the 5th magnitude, entered the glass.

At 2h. 34′$\frac{1}{2}$ it passed the cross, and then Mr. Bradley, by the graduated screw, exquisitely bisected it, and afterwards moved neither of the screws till it went out; and from the cross, it seemed to him to move rather to the left hand, but a very small matter, as it went out.

1725, *December* 18.

This evening Mr. Bradley observed several telescopic stars, as they run along the thread, and seemed to be confirmed that they all of them moved in the same manner; that is to say, that if they were truly bisected at the entrance, they seemed sensibly to edge off toward the right hand by the time they were got near the cross, and that they seemed by degrees to return to be pretty truly bisected again about the time of their going out; but this whole deviation of the apparent path of the stars from the thread was so small, and the fluttering of the star from the instantaneous variations of the air (very sensible in long glasses, and much more in reflecting telescopes) was so sensible, and so frequent in the time of the transits, that it was found impossible to determine at all satisfactorily to how much this deviation, at the very point of the cross, whereabout it was greatest, might possibly amount unto. It could only be judged in gross, that it did not perhaps amount to more than about a diameter of the thread. Upon this it was concluded, that in order to measure this deviation as near as was possible, it would be best, by the graduated screw, to bisect a small star as near as possible at the entrance; then the assistant to note exactly the number at the index, then exquisitely to bisect it again as near as possible to the cross, and again to note the number at the index, and to do the same things again, as near as possible, at the going out; and accordingly the star in Capite Persei was this night observed for this purpose.

At about 2h. 33′ sidereal time, this star entered, and the glare of it being as truly bisected as possible, the index stood at $0\frac{1}{4}$. When it came to the cross, it was again carefully bisected, then the index stood at $2\frac{3}{4}$. About 2h. 36′ it went out, and then being truly bisected again, the index stood at $1\frac{2}{3}$. This was found to be the only method of judging at all nearly of the quantity of this deviation; for till this method was taken, the estimations that were made thereof were very different and inconsistent, and as wide from the truth as could be, in a quantity so small; and from hence it was at first apprehended that the wire was itself not perfectly straight, and that this bending thereof had not before been taken notice of in the observations of the 3d, the 5th, the 10th, and the 12th of December, it having happened in those four observations that the bisection of the star on the thread had not been so exactly taken notice of as it might have been. The most of the time of those transits having been taken up in trying of the apertures, or in moving the screws, so that the situation of the star just at the cross, had not before been strictly taken notice of.

This apprehension of the thread being crooked continued some time, till upon consideration that possibly the apparent path of the stars seen through this glass in our zenith might sensibly deviate from a straight line, this matter was computed; and it was found, that if the star were truly bisected at each end of the cross east-and-west thread, it would appear near the cross, about $2\frac{1}{4}$ seconds distant south of the cross, which is about a diameter of the thread; and it was further considered, that this deviation must seemingly be as Mr. Bradley observed these stars do go, viz. edging off by degrees on the right hand toward the middle part of the glass; for as in reality they deviate to the south, and the north was in reality upon the observer's right hand in the inverting glass, this deviation must therefore appear toward the right hand.

And this is a very strong confirmation, not only of the great exactness to which it was possible for the eye to determine in some tempers of the air, when the star was exquisitely bisected by the thread; but also of the accuracy with which these small angles could be measured by this instrument, since it is plain by these observations that the deviation resulting from the measure of the instrument, at a time when no deviation whatever was looked for, as is aforesaid, do very nearly coincide with the deviations which must happen in reality[m].

And it must here be noted once for all, that the measuring of the same deviations of the apparent paths of the stars, in the same manner, have been since frequently repeated when the state of the air would permit, and have been found to agree very nearly with these observations. Although it must be confessed that the stars, especially in summer, were found to flutter so much, that it was hardly possible sometimes to determine this bisection to any sort of exactness.

And it must be further from hence noted, that it was necessary in this instrument to observe the bisection of the star, always as near as possible about the same part of the thread; and it being convenient, especially on account of the fluttering of the star, for the observer to have some time to judge distinctly of its running truly bisected some way on the thread, it was therefore concluded, from the considerations of this day, that henceforward the star should be brought to be truly bisected about ½ minute before it came to the cross, since from that time to about as much after the passing the cross, there could be no sensible deviation of the apparent path of the star, and this would give more than sufficient time for the observer to be as accurately certain of the bisection of the star on the thread, as the state of the air then would allow of.

And according unto this, the subsequent observations of the bisection of the star are strictly and always to be understood; that is to say, when the star is said to be bisected by the thread, it is to be understood that the star was bisected in that space, unless it be otherwise expressed.

Considering this day the observation of yesterday, where the star Caput Draconis seemed even at the entrance to be to the right hand, or on the south side of the thread, consequently, that when it was near the cross, it must have been some two or three seconds south thereof, and this being a deviation directly contrary to that of the annual parallax of the earth, it was at first mistrusted that the mark had not been so truly fixed as it should have been; but subsequent observations proved the contrary; and it was afterwards concluded that the mark and star did nearly agree when first fixed, and this alteration was a real alteration in the apparent place of the star, as will be seen hereafter.

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[m] The third copy ends here; what follows is printed from the second.

Having thus given an account of this instrument, and the whole process of fixing and adjusting it, as is aforesaid, what follows is chiefly the series of observations from time to time made therewith; but before these are begun, in strict regard to truth, it is necessary here to take notice, and repeat it again, as hath been hinted before, that upon the whole it did not appear possible to observe nearly so accurately the bisection of the star upon the cross thread, as it was to observe the bisection of the mark by the plumbline. There is one very obvious reason for this: the plumbline and the mark were observed by a glass of 1½ inch focus, the star and the cross thread were observed by one of 4 inch. focus; it might therefore have been better in this case to have made use of a deeper charge. The other reason is less obvious to persons who are not used to astronomical observations, and it is this: while the mark and the plumbline were observed through but 1½ inch of an equable, steady air, the star was viewed through the whole height of the atmosphere, and, as it hath been hinted before, those who have used long glasses do very well know that there frequently happen such sudden, and almost instantaneous little alterations in the refractive quality of the air, as will make objects to undulate and flutter about very sensibly, as much as may be of some consequence in so accurate an observation as this; and in this instrument it did happen so sometimes in fact; the stars would flutter on the thread, so as that their glare would sometimes appear to be considerably more on one side, and instantaneously afterwards as much on the other; however, in this vertical situation these flutterings were never observed to amount to above two or three seconds, and very seldom to near so much; and sometimes the air was so still and undisturbed that there appeared no fluttering at all; and generally, if the observer was to estimate the bisection for any considerable space on the thread, when the air was the most disturbed, he could hardly be liable to so great an error as two seconds; and, for the most part, it seems probable there could not indeed be an error upon all accounts of near that quantity; and, with sufficient care and practice in the observer, it generally might come nearly within less than half thereof, at least as to the determining the absolute quantity of the whole variation of the star's place at different times of the year, where different observations might be made on different days, near the two extremes, to verify and correct one another by taking the medium of them all; for this purpose at least, it was hardly possible to err half a second by this instrument; to render which the more credible, it must be here observed, as may be drawn from what is above said, that in an undisturbed air, it may be much more accurately judged when the light of the star is truly bisected, than it can be to judge how far its centre may be from the middle of a thread, to which it seemingly adheres; and nobody can be a judge of the difference of accuracy in these two cases, but those who shall have an opportunity to examine the matter with an instrument as fit for the purpose as this.

1725, *December* 21.

This day was very fair and clear, Mr. Bradley, about 11h. mean time, very carefully examined and adjusted the telescope by the plumbline, and the index stood at 8,0.

At 17h. 48′ 22″ Cap. Draconis entered.

At 17h. 49′ 52½″ star at the cross.

At 17h. 51′ 24″ star went out.

As soon as he could discharge the coarse screw, the star seemed to him to be too much to the right hand, and so it continued till it passed the cross thread; and in less than a quarter of a minute after it was passed he turned the graduated screw, till he saw the light of the star perfectly bisected, and then the index stood at 11,7. He took this to be a very exact certain observation, having seen the star very distinctly; but if there was any error, he thinks it was that he rather unscrewed the screw too much, and if so, that the number of the index should be rather less than 11¾; however, it hath been here set down exactly as it was then found.

Then he examined the telescope by the plumbline again, and about 11h. 30′ he found it stood at 8,3.

Let 8,2 be the number the index shewed when the mark was true to the plumbline, and consequently when the instrument was in the same situation which it had when the mark was fixed on the 3d of December.

Then deducting 8,2
from 11,7
remains 3,5 whereby this star appeared this day south of the mark.

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⁂ "The third figure" is referred to in p. 99; but none such is to be found in either of the MSS. Neither this possibly nor the first were ever executed: see note [b] in p. 94.

OBSERVATIONS[a]

MADE AT KEW.

1725, *November* 27.

6h. 37′ A STAR brought to the hairs within $\frac{1}{4}$ of an inch of entrance went out near $\frac{1}{4}$ of an inch to the left, that is, south of the east end; so that the east of the hair must be brought about by this star, half so much southward, and the west end half so much northward.

6h. 51′ 20″. About this time another not so bright in the same way.

7h. 06′ 26″ A very bright one touched at the entrance at going out; I turned seven divisions of the second screw, to make it touch again. The cross threads are at a very good distance from the object-glass; for in moving the eye I could not perceive that the stars had any motion at all.

N. B. In observing, we always lie with the head eastward.

Divided and numbered the second wheel containing 17 seconds.

This wheel is numbered so that increasing the number unscrews the screw, and brings the snout northward, and therefore brings the telescope to point nearer to the zenith; wherefore the numbers must be decreased, and the screw screwed, to the end that the snout may be pushed southward from

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[a] These observations are printed from the 4to book marked K. 14. With a few exceptions, which will be particularly noted, the first part as far as April 22, 1726, inclusive, is in Molyneux's handwriting; the rest is written by Bradley.

On the inside of the cover of the book Molyneux has written,

h. ′ ″
" Drac. 17 50 10
" Pers. 2 34 59

" $1'' = \frac{10}{7076}$ of an inch exact on radius 24 feet 3,15 inches.
" Dubiè. The thread under $\frac{1}{300}$ of an inch $= 2\frac{1}{4}$ seconds.
" Exact. The first plumbline $\frac{1}{284}$ of an inch $= 2\frac{1}{2}$ seconds.
" When numbers lessen, top of stack of chimneys hath gone southward."

December to June, for this brings the telescope to point farther and farther from the zenith; and the star, from this period to the next, if it hath any sensible parallax, will go farther and farther from the zenith, and more and more northerly; so that the alterations I am to expect are to be, that the star should go off from the thread to the left hand. To increase the number, it must be turned from the west by the zenith to the east; for this unscrews.

The number shewn by the index when the glass is restored from time to time by the mark to the plumbline, I say that number, being compared with the number shewn by the index, when by this graduated screw the star is at any time again brought to be bisected to the observer's eye looking through the glass; I say, the difference between these two numbers will shew the number of seconds by which the star hath moved from the situation it had at the time of fixing the mark.

Stopped the door close, and made good the tube; and now the plumbline settles exquisitely in about three or four minutes from first putting the plummet in water.

It will move and vibrate by moving in the passage, but steadies again in less than one minute and a half.

One half division will sensibly throw the line from the centre toward the north or south side of the mark.

At 8h. 59′ set it, and the mark is at 6½.

9h. 31′ 08″ Pers. Glass takes in 28′ by this star.

9h. 32′ 41″ went out almost as much as the work will reach; snout southward.

9h. 47′ 00″ tried plumbline again, and it came to 9¾.

N. B. Between 8h. 59′ and 9h. 47′ the weight that moves the sash was let to fall violently against the wood that supports it, and yet it altered no more than 3¼ seconds in the direction of the instrument.

1725, *November* 28.

Put back the moveable clock 6′ 35″.

h.	′	″	
9	31	08	
	6	35	
9	24	33	
	3	56	
9	20	37	it ought to come this night, the 28th.

$$\begin{array}{lrl} & \text{h. } {}' \ {}'' & \\ \text{P. M.} & 2\ 05\ 00 & \text{library.} \\ & 2\ 03\ 38 & \text{moveable.} \\ \hline & 1\ 22 & \text{moveable slower than library.} \end{array}$$

$$\begin{array}{lrl} & \text{h. } {}' \ {}'' & \\ \text{P. M.} & 11\ 15\ 30 & \text{library.} \\ & 11\ 15\ 27 & \text{moveable.} \\ \hline & 3 & \text{moveable slower than library.} \end{array}$$

1725, *November* 29.

Went to London for the winter.

1725, *December* 2.

At 11 h. 45′ ante mer. came from London.

Mr. Graham here again. Found the mark at $9\frac{3}{4}$. I was obliged to bring it to $8\frac{1}{2}$ when true to plumbline.

Then brought the east end above southward about ,025 of an inch, which is about one revolution, and one eighth of a revolution of the rotating horizontal screw above; and now I judge the hairs nearly east and west. Pinched it there, and adjusted the work below accordingly, and pinched that too.

The same day at 5 h. 30′ we found it true to the same mark; the plummet on since the morning.

The star in the head of Perseus having been observed on the 27th of November, as is there related, I this night brought the snout of the telescope southerly into the same situation, as near as I could guess, which it had that night when Pers. run along the thread, and supported it there by the graduated screw; then I turned the graduated screw so as to bring back the snout northerly 15 revolutions and 12 parts, being = to 4′ 27″ the present difference of declination of Cap. Pers. from Cap. Drac. in Flamsteed's Tables, and in that situation we found the mark about $\frac{1}{30}$ of an inch north of the plumbline.

Then we shifted the piece of brass which carries the mark by means of its own screws $\frac{1}{30}$ of an inch southward, the plumbline being our direction, and not touching either of the screws that move the telescope; and thus we brought it so that the mark now agrees with the plumbline again, and the graduated or second screw is at 5; and we now judge that the mark can shift a little more than $\frac{1}{20}$ of an inch northward, and a little less than $\frac{1}{20}$ of an inch southward, in case of necessity.

Then fitted up the apparatus for the glass to view the plumbline.

Left it this night exactly at 5.

Clouds all day and evening.

1725, *Friday, December* 3.

9h. a. m. This was a rainy, blowing, tempestuous night; however this morning trying the instrument again, we found the index as we left it last night at 5, and we were obliged to alter it only to 6; so that with this blowing, sudden change of weather, it alters but one second to bring it to plumbline.

Mr. Graham went to London.

This day I saw the sun pass the meridian, and the library clock was then about 1′ 59″ too fast for mean time, and the clock in the black room was 6′ 47″ faster than the library, so that the clock in black room was 8′ 46″ faster than mean time.

I saw Cap. Draconis pass the glass this day for the first time. It passed the hairs at 12h. 26′ 32″; deduct 8′ 46″ clock too fast, leaves 12h. 17′ 46″. It should have passed this day, by the Table, at 12h. 17′ 30″, so that the glass directs 16″ in time too much westward.

The star doth not yet run along the thread. The east end of the axis must be still altered, and brought about southward, but not so much as we altered it last night.

I saw the star but very weakly and indistinct. It is quite covered and lost when on the thread; I brought it just on the thread in the cross by the graduated screw, and left it there. Then the index stood at 10½, or rather more, and the mark was north of the plumbline above $\frac{1}{20}$ of an inch. In hazy weather, and in the day-time, one may err 3″ or 4″, but hardly more.

N. B. No cover, but the whole aperture was bare. See December 5th, 1725.

Then I tried to shift the brass piece that carries the mark southward so much as was necessary to reduce it to be true to the plumbline, by its own screws, and touching nothing else of the instrument; but it would not shift enough, so that I was obliged to take out the brass piece on which the mark is, and to make a new mark about $\frac{1}{20}$ of an inch or more south of the first mark hitherto used. I made the mark much of the size as the first; then I put on the brass piece so marked anew, and I shifted it by its own screws, and touching nothing else till I brought the mark to the plumbline; and when it was true I pinched it pretty tight, but not so hard but that it will shift still by the north and south screws.

This was all done and finished by a quarter after one, and I am sure I cannot err 4″, and, I believe, not 2″ from truth.

At 3h. 30′ p. m. found it still true to plumbline, the mark at 10½, or a little more.

Then I loosed the screws of the work below and above, and I rotated the east end of the axis toward the south; not so much as last night, perhaps near ¾ of that. This time we turned just ¾ of a revolution of the screw above, and last night we turned above a whole revolution; then pinched all fast above, and having adjusted it again as to the rotation below, pinched it fast there too. Then adjusted it to plumbline again, and index stood at 8½, and there left it.

Tried it again at 8h. 30′. Adjusted it to plumbline, and then the index stood at 7⅓. A violent stormy night.

I saw some small stars this night about 8h. which seem to run very true, but I cannot be sure.

This evening I screwed up bob of moveable to go sidereal time, viz. to upper mark, and set it near to sidereal time.

	h.	′	″	
P. M. at	5	08	0	by library.
	10	38	15	moveable.
	5	30	15	

	h.	′	″	
P. M. at	8	33	0	library.
	2	03	57	moveable.
	5	30	57	

57″
15
Gains, in 3h. 25′, 42 which should be but 34″, near 3″ per hour too much, which is a minute a day.

Then I let down the bob of the moveable about $\frac{1}{20}$ of an inch, and it stands now at 20.

	h.	′	″	
At	8	50	30	library, p. m.
	2	19	06	moveable.
	5	28	36	

	h.	′	″	
At	10	36	30	library, p. m.
	4	05	26	moveable.
	5	28	56	

1725, *Saturday, December* 4.

h. ′ ″
9 00 00 library, a. m.
2 30 51 moveable sidereal.
5 30 51

By this the sidereal gains about 35″ too much in one day.

Then I let down the bob of sidereal one whole revolution. It now stands at 20.

h. ′ ″
And at 9 39 30 library.
3 13 55 sidereal.
5 34 25

h. ′ ″
At 11 24 00 library.
4 58 43 sidereal.
5 34 43

At 11h. 45′ found index at 7⅓, altered it to the mark, and then it stood at 7⅕.

It grew very cloudy and dark about this time, so that I could neither take the sun nor the star.

h. ′ ″
At 12 10 30 library.
5 45 21 sidereal.
5 34 51

So that now the sidereal clock shews sidereal time, and will keep it very nearly.

At 12h. 30′ found index at 7⅕, altered it to the mark, and then it stood at 7.

The object-glass was found a little dampy, and the board that lies on it was so too. I now have put a bit of wood under it, that the air may blow freely through.

I find the plumbline must be some way clear of the edge, or otherwise it will not settle true to two or three seconds. I fancy the edge attracts it.

East and west the telescope alters vastly more than north and south. I believe in a night or so sometimes 10″ or 15″ in space, as I guess.

At 7h. 35′ p.m. found index at 7; adjusted it to the mark, and then index stood at 7¾.

h. ′ ″
At 7 37 00 library.
1 13 07 sidereal.
5 36 07

Soon after this pretty clear night.

Sidereal time. The snout north as much almost as it will go.

1h. 23′ 15″. Two telescopics. This the preceding passes mer. and runs exactly true along the thread.

1h. 27′ 12″. A brighter telescopic passed.

1h. 56′ 14″. A bright star north quite out of reach.

1h. 59′ 16″. Another south, as I judge, out of reach.
The snout now quite south.

2h. 10′ 42″. A bright one very north out of reach.

2h. 14′ 10″ One not very bright run along the thread.

2h. 16′ 15″. Another smaller, but just north of the thread; one passed south not 3″ before this.

2h. 22′ 20″. Another exactly where the last was.

2h. 35′ 53″. Cap. Pers. passed mer.

2h. 37′ 24″. Went out. Hazy now.

h. ′ ″
2 35 53
2 34 59
54

It runs exactly well along the thread.

While I observed this star, the snout was supported by the graduated or second screw; and to bring the mark true again to the plumbline, I was obliged to turn the screw 18 rev. and 2″ = 5′ 00″ nearly, which is the difference of declination between Cap. Pers. and Cap. Drac. by our observations with this instrument, differing but ½ a minute from Mr. Flamsteed's Tables, and Pers. is so much north of Drac.

While it run along the thread I altered the screw twice, and brought the star to be bisected again, looking through the glass; and I was very consistent, not differing a second at any time, and when it was very sensibly from the truth, looking through the glass, the variation of the number shewn by the index was not greater than 3″.

I think one may be very sure to 2″ or 3″ at most if the weather is very clear, otherwise one may err 3″ or 4″. I used no cover, but left the glass quite open. Query, Will it not do better by night and by day also with proper aperture? See Dec. 5th, 1725.

Left index at 7⅓.

1725, *Sunday, Dec.* 5.

h. ′ ″
At 8 31 00 library.
2 09 21 sidereal.
5 38 21

Still sidereal gains too much about 15″ or 18″ a day.

Let down the bob one whole revolution more, and now it stands at 20. In doing which the sidereal clock lost 20″.

	h.	′	″	
At	9	22	30	library.
	3	00	31	sidereal.
	5	38	01	

At 5h. 40′ sidereal time, which was about 12h. 02′ 00″ mean time, I found the index at $7\frac{1}{3}$, as I had left it last night.

I reduced the mark to the plumbline, and then the index stood at $9\frac{1}{2}$.

The wind blew violently at north-east, and the plumbline vibrated at least $1\frac{1}{2}$ second at each side the mark, but now and then it settled as above, (the index at $9\frac{1}{2}$.)

The weather exceeding clear, hard frost, not a cloud in the sky.

At 5h. 47′ sidereal, I began to wait for the entrance of the star, but first with the coarse screw I eased off the snout from the second screw about $\frac{1}{20}$ of an inch.

Then my servant counted the sidereal clock, and another attended above in the garret.

At 5h. 49′ 12″ or may be 1″ sooner, I saw the star Cap. Drac. enter distinctly a little to the right hand, or north in appearance of the east and west thread; then I instantly discharged the coarse screw, and let the telescope return to be supported by the second or graduated screw, the index continuing unaltered, as is above said at $9\frac{1}{2}$, and the mark, as settled December 3d, true to plumbline. And I found the star was exquisitely upon the east and west thread, so as to be almost lost under it: then I called to my servant, who attended above, to cover the object-glass with a pasteboard aperture I had prepared, of 2,75 inch diameter, and I found I then saw the star much distincter. When with the coarse screw I brought it from under the thread, it appeared very distinct a pale lucid point, with scarce any sensible diameter, I am sure not $\frac{1}{2}$ of the diameter of the thread, which is the 284th of an inch; and when I returned it again to rest on the graduated screw, I could sometimes see it bisected. But when the aperture was taken off again, and the whole glass left bare, there came in so much light it quite effaced the small light that diffused itself from the star on each side of the thread, and the star became quite lost and invisible. I repeated more than once this taking off and putting on the aperture, and always found it much distincter and better, whether the thread was on the star or off it, and perhaps a smaller aperture might do still better; nor could I perceive it any way altered the situation of the star, not even when held somewhat excentrically, as I ordered my servant sometimes to hold it. The star run very exactly true along the east and west thread, from the entrance to the going out; so true, that sometimes I fancied it most on the one side, and sometimes most on the other side. I am very certain I cannot err in the situation of the glass this day not 3″, and I think not 2″. I moved it with the coarse screw till the star was above a diameter of the thread clear of the thread, which was a very sensible distinguishable space, and this alteration I afterwards found amounted scarce to 6″, and I think I may be confident I could not err not one third, I am sure not one half of that quantity.

At 5h. 50′ 42″ the star passed the meridian.

h.	′	″	
17	50	10	
17	50	42	
		32	too fast.

At 5h. 52′ 14″ the star went out of the glass.

At 5h. 54′ sidereal time, I again hung on the plumbline, and bringing the mark to it, I found that during the observation the glass had scarce sensibly altered its situation, notwithstanding the very high wind; for when I lay down the index stood at 9½, and now it was at 9⅞, so that it had altered scarce half a second in its direction in this quarter of an hour.

The sky was so clear and fine I could not have a better nor surer observation, nor can I find that it is possible at all to mend the situation of the mark made and fixed as is related on the 3d of December 1725, so that I have now no doubt but that for this period I may certainly depend upon having fixed my mark truly to its place and exact limits within 2″ or 3″; and now we have no farther corrections to make, and are only to verify and repeat the truth of this day's observation for some days to come, and about the middle of January, or sooner, to look out for what alterations may then begin to be visible.

Left the mark at 9⅞.

	h.	′	″	
At	12	26	30	library.
	6	05	00	sidereal.
	5	38	30	

Went to London.

1725, *Friday, December* 10.

With Mr. Graham.

h. ′ ″
A. M. 11 00 30 library, which now gains 6″ a day.
4 57 32 sidereal.
5 57 02
5 38 30 difference on the 5th of December.
18 32 gain of sidereal above library in 5 days,

60
18
480
60
32
5) 1112 ($222\frac{2}{5}$ per diem = 3′ 42″
10
11
10
12
10
2

60) 222 (3
180
42

which is 3′ 42″ per diem, but library gains 6″ per diem of mean time, which added to 3′ 42″ makes 3′ 48″, and this the sidereal clock now gains per diem of equal time; it should gain 3′ 56″, so that it loses of sidereal time 8″ a day.

On Sunday, December 5th, it was 32″ too fast. The loss in five days we compute 40″, so that this day, the 10th of December, it is 8″ too slow.

We found the index as we left on the 5th, at $9\frac{7}{8}$.

On Tuesday the 7th there was a violent and very unusual hurricane, such as hath not been known in many years, and from that time to this day a very great change in the weather, from cold dry frost, to mild, open, rainy, warm weather; notwithstanding all which changes, when we adjusted it this day the index stood at $11\frac{1}{2}$, only $1\frac{1}{2}$ second different from what we left it at on Sunday, December 5th.

Cloudy. So we saw neither star nor sun.

Took up the bob of sidereal seven of the large divisions, viz. from 20 by 2 to 7, and there we left it, losing, as is above computed, about 8″ per diem of sidereal time.

h. ′ ″
6 57 00 p. m. library.
12 55 07 sidereal.
5 58 07

Clouds all night.

h. ′ ″
11 48 30 p. m. library.
5 47 25 sidereal.
5 58 55

Left the index at 11½. It seems not to have altered all this day.

1725, *Saturday, December* 11.

h. ′ ″
9 19 30 library.
15 19 57 sidereal.
6 00 27

h. ′ ″
9 24 30 library.
15 24 57½ sidereal.
6 00 27½

The sidereal now seems to gain 8″ per diem, whereas yesterday it lost as much of sidereal. Wherefore Mr. Graham let down the bob about four divisions of the great divisions, whereof we altered seven yesterday.

h. ′ ″
9 58 30 library.
3 59 07 sidereal.
6 00 37

h. ′ ″
10 04 00 library.
4 04 37 sidereal.
6 00 37

The weather proving unexpectedly clear after a violent stormy rainy night.

We examined the instrument, and found the index as we left it last night, at 11½.

This morning before the star passed we made the new frame for an aperture, and cut the hole by a turned cylinder cut at an angle of 45°. The short diameter 2,5 inches.

This was on while we observed the star, the short diameter north and south.

Then at 5h. 42′ sidereal time, about 11h. 42′ mean time, Mr. Graham adjusted it very carefully to the mark, and the index stood at rather less than 11.

At 5h. 48′ 49″ I saw the star Cap. Draconis enter, the snout being supported by the coarse screw; then I discharged the coarse screw, and let it bear on the graduated screw, and to my eye it seemed as on the 5th instant, very sufficiently exact bisected; then I supported it again by the coarse screw, and Mr. Graham lay down about 5h. 49′, and instantly saw the star; and then discharging the coarse screw, to let the snout bear on the graduated screw, he found it also exactly bisected. He staid at the glass till it went out, and found it run very well along the thread; he says, if any thing it runs off to the south of the east; but as to the direction of the threads, and also as to the situation of the mark, as now fixed, he thinks they cannot be mended, and are sufficiently exact and true. And he thinks that in judging when the star is truly on the thread bisected, one can hardly err 2″, if so much; and as to adjusting the instrument to the plumbline, he is positive there cannot be an error of one second.

At 5h. 50′ 20″ he saw it pass the meridian.

At 5h. 51′ 50″ he saw it go out.

h. ′ ″
5 50 20
5 50 10
―――――
10

At 5h. 57′ he again examined the instrument by the plumbline, and found it exquisitely as it was before the observation, at 5h. 42′; the index standing exactly as before, at a little less than 11, the mark being true to plumbline.

h. ′ ″
12 15 00 library.
6 15 58 sidereal.
―――――
6 00 58

h. ′ ″
6 15 00 library, p. m.
12 16 56 sidereal.
―――――
1 56

So that now I judge the sidereal gains of sidereal time about 3″ per diem.

And was 10″ too fast for sidereal time when the star Cap. Drac. passed this day, December 11th.

h.	′	″	
10	04	00	library, p. m.
4	06	31	sidereal.
	2	29	still as above, gains 3″ or 4″ per diem.

This night we fitted up the enlightening lantern, which seems to do very well.

An exceeding stormy, rainy night.

1725, *Sunday, December* 12.

Fair morning, wind laid.

h.	′	″	
9	37	00	library.
15	41	22	sidereal.
6	4	22	

By this sidereal seems now to keep exact sidereal time.

h.	′	″
6	4	22
		3
	4	25
		37
	3	48

At 17h. 22′, sidereal being about 11h. 18′ mean time, found the index at almost 11, as we left it last night; brought it to mark, and then index at a little more than 10.

At 17h. 44′ sidereal, about 11h. 40′ mean time, Mr. Graham carefully examined the instrument again, and adjusting it afresh to the plumbline, the index stood at $10\frac{1}{6}$.

Then I lay down and waited for the entering of the star, the snout discharged by the coarse screw as yesterday.

At 17h. 48′ 46″ it entered. Then I let back the snout to bear on the graduated screw, and when so to my eye I found it as yesterday, exquisitely bisected. I left it so, and then Mr. Graham lay down, and at first could not perceive the star, it was so truly on the thread. Then he turned the graduated screw till he could perceive an alteration by his eye through the glass, without knowing at all himself how much he had altered the said screw; and I stood by to observe by the divisions how much he altered it, and what he altered it did not amount to one second, when he said he saw the star distinctly adhering to one side of the thread uncovered; and he turned it so as to drive the snout southward.

At 17h. 50′ 18″$\frac{1}{2}$ he saw it pass the meridian. Then I desired him, still looking through the glass, to bisect the star again, by turning again the graduated screw which bore the snout. He did so, and when he said it was again bisected, the index stood at 10$\frac{1}{2}$, or rather less, whereas at first it stood at 10$\frac{1}{6}$, and was well bisected by my eye; so that it is demonstration he could adjust it then, the weather being extreme clear and favourable, to less than $\frac{1}{3}$ of a second.

At 17h. 51′ 50″ he saw the star go out well on the thread, and is confirmed that the direction of the threads, and the truth of the mark, as I fixed it December 3d, do require no manner of farther correction, but are sufficiently exact.

Returned this morning to London.

At 18h. 08′ sidereal, about 12h. 08′ mean time, examined it again by the plumbline, and then the index stood at 10 and less than $\frac{1}{3}$; so that it did not alter one sixth of a second this day pending the observation, viz. between 11h. 44′ and 12h. 08′.

1725, *Friday, December* 17.

With Mr. Bradley.

h.	′	″	
10	00	30	library.
16	23	50	sidereal.
6	23	20	
6	00	37	Their difference Dec. 11th.
	22	43	Sidereal gains of library in 6 days=3′ 47″ per diem.

By the sun's transit this 17th of December, it appears that the

	′	″	
Library gains	0	7	per diem of mean time.
Sidereal gains	3	47	per diem of library.
	3	54	sidereal gains per diem of mean time.
	4	00	it should gain at this time of year.

Found the mark at 10$\frac{1}{3}$, where it was left December 12th.

Mr. Bradley brought the mark to the plumbline at 11h. 15′ mean time, and then the index stood at 9$\frac{3}{4}$. He thinks the instrument may at any time be securely adjusted to $\frac{1}{2}$ a second by the plumbline.

At 5h. 47′ sidereal, I lay down to wait for the coming in of the star.

At 5h. 48′ 32″ it came in, but I did not see it so distinct as on the 12th instant. Then I discharged the coarse screw, and found it as on the 12th, to my eye very truly bisected. Then I bore it off with the coarse screw again. Then Mr. Bradley lay down, and discharging the coarse screw about 15″ before it passed the meridian, it seemed to him on the thread, but rather too much to the right hand. It seemed to him to run thus till about 15″ or 20″ after it had passed, and then seemed by degrees[b] to recover the thread, and to continue truly bisected from that time till he bore up the snout with the coarse screw again within a few seconds, perhaps 8″ or 10″ afore it went out; which at 5h. 51′ 34″ he observed exactly. It appeared very faint and indistinct to him.

I do not now know how to account for this change in the running of the star along the thread, as it appears to Mr. Bradley, but from some alteration in the refraction of air, which I have found before would make the star flash sometimes towards one side, and sometimes towards the other side of the thread; but I never saw those alterations continue so long as this.

The zenith was covered with thin, hazy, whitish clouds all the time of this observation, which drove slowly from the north to south.

At 11h. 50′ mean time, examined it by the plumbline, and found it not sensibly altered.

At 5h. 30′ mean time, examined it again, and it was still the same.

Fitted oil-paper to enlighten the mark, and it does mighty well.

At 12h. 10′ 17″½ Mr. Bradley observed a pretty bright one pass along the thread.

12h. 11′ 49″ it went out.

h.	′	″	
0	10	17½	
0	11	49	
	1	31½	
12	08	45	came in.

As to this star also, he thinks if any thing, that when it draws near the centre of the cross threads, it bears off rather more to the right hand than otherwise, and returns as Cap. Draconis did this morning.

At 1h. 30′ sidereal, and 7h. mean time, screwed up the bob of sidereal from 3 by 4 to 8, and left it at 8. It lost 20″ in doing this.

At 2h. 33′ 07″ Mr. Bradley observed Cap. Pers. enter.

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[b] “by degrees” is an interlineation made in the MS. by Bradley.

2h. 34′ 38″$\frac{1}{2}$ it passed, and then he set it exquisitely bisected on the cross.

2h. 36′ 10″ it went out, and seemed to him to move rather to the left hand, but so little it is scarce sensible. He thinks it cannot amount any thing near to one diameter of the thread. However, all these stars to-day having gone the same way, it looks as if the wire was not exquisitely straight, but it is so small a matter it is scarce worth notice.

At 3h. sidereal, which is about 8h. 30′ mean time, examined it again and found it exactly at the same as in the morning, viz. at 9$\frac{3}{4}$, where we left it.

1725, *Saturday, December* 18.

With Mr. Bradley.

Found it at 9$\frac{3}{4}$, where we left it last night.

Adjusted, and then it was at 8$\frac{4}{10}$.

Clouds.

$$\begin{array}{rrrl} \text{h.} & ' & '' & \\ 11 & 28 & 30 & \text{library.} \\ 17 & 55 & 33 & \\ \hline 6 & 27 & 03 & \end{array}$$

11h. 48′. About this time one not very bright. Mr. Bradley thinks it runs as last night.

11h. 53′ 16″. One along the thread passed; snout in middle; still runs as yesterday.

12h. 04′ 26″. Another passed the same way.

12h. 8′ 29″. A brighter one seen last night came in bisected.

12h. 9′ 23″. It began to yield to the right.

12h. 10′ 02″. It passed almost clear of the thread to the right.

12h. 11′ 05″. It was bisected again.

12h. 11′ 30″. It was to the left.

12h. 11′ 33″. It went out.

These taken by Mr. Bradley.

2h. 33′ 06″ Cap. Pers. entered, and I observed it, and bisecting the glare of it at entrance, Mr. Bradley found the index at 0$\frac{1}{4}$.

When it came to the centre I bisected it again, and then he found the index at 2$\frac{3}{4}$.

2h. 36′ 10″ it went out, and then I bisected it again, and the index then stood at 1$\frac{2}{3}$.

To my eye it seems when it is well bisected, at first to tend, as he says, rather to the right hand when it draws near the centre; but I never lost the light on the left hand, nor ever saw the body of the star on the right. It fluttered about sometimes to one hand and sometimes to the other, and sometimes even near the centre I saw more light on the left hand than on the right hand, but for the most part there was rather more on the right hand; it is plain, whatever causes these appearances, whether the wire is somewhat bent or not, or that the alterations of the air cause it to flutter, or somewhat of both, which I judge to be the case, or whatever else is the cause, the whole effect does not amount to more than $1\frac{1}{2}$ second.

The apparent path of the stars seen through this glass in our zenith, must deviate sensibly from a straight line, and just at the cross they may appear about 2″ distant south of the cross, in case they be truly bisected at each end, and the deviation will seemingly be as these stars do go, viz. towards the right hand near the centre; for in reality they deviate to the south, which is on the left hand, the north pole being on the right hand; but if it be adjusted so as that the star be truly bisected about a minute after it enters, that is, about half a minute before it passes, then it will not differ above $\frac{24}{100}$ of a second on the cross, and scarce sensibly all the rest of the thread, so that certainly it will be best to adjust it to the star always about that time.

Now as to adjusting it in fact.

If the wire was a mathematical line, and the apparent path of the star was so too, as it would be if we lived under the equator; yet still if the star fluttered, as it always has done to my eye, I could not be sure to set it much nearer than this, viz. to about 1″ or 2″, and this is much nearer than I expected; for December 3d, looking with the whole glass bare, I thought I could not come nearer than 3″ or 4″, except in a very clear undisturbed air.

At 3h. sidereal examined it, and brought mark to plumbline, and found the index then stand at $8\frac{6}{10}$. So that this whole day it hath altered but $\frac{1}{5}$ of a second.

A calm, gentle day, wind north, but very little.

1725, *Sunday, December* 19.

Dark cloudy day.

The library clock gains about 8″ a day of mean time

h.	′	″	
6	18	15	library.
12	50	14	sidereal.
6	31	59	

At 2h. 36′ 12″ Mr. Bradley saw Cap. Pers. go out.

h. ′ ″

Pers. R. A. 2 34 59

Passed this night 2 34 40

+ 19 clock too slow.

h. ′ ″

Perseus this night 2 36 12

Perseus last night 2 36 10

2 sidereal loses of sidereal time.

A fire-pan in the black room to dry the blacking. I believe it will now rather gain than lose of sidereal time.

This night we considered how much the star would deviate from the line in moving through the glass; and from these considerations, and my observation of the star's passage, December 17, I conclude that the mark is about $\frac{1}{2}$ a second south[c] of the situation it ought to have in respect of the cross. So that as the mark now stands, (if there be no sensible variation,) the glare of the star at the cross must always appear most to the right hand. J. B.

1725, *Monday, December* 20.

11h. a. m. found it at $8\frac{6}{10}$.

Adjusted mark to plumbline, then it stood at $7\frac{1}{2}$.

It was a little hazy, but no thick clouds, and the star could not be seen.

h. ′ ″

6 34 41 diff. now.

6 31 59 diff. $17\frac{1}{8}$ hours past.

2 42=162″ in 17 hours.

17) 162 ($9\frac{1}{2}$ per hour=3′ 48″ a day.

153

9

But library gains about 8″ a day of mean time 8″

Total gain of sidereal 3 56 in a day, with the pan of coals in the room.

h. ′ ″

11 25 00 library, a. m.

17 59 41 sidereal.

6 34 41

h. ′ ′

8 00 00 library, p. m.

2 36 04 sidereal.

6 36 04

---

[c] South has been substituted in this place for north, on which Molyneux has noted, "This "should be north as it was first wrote."

This paragraph with J. B. subscribed to it is in Bradley's hand-writing.

h. ′ ″	
6 34 41	9) 85 (9
6 36 04	81
1 23=83″ in 8¾=85″ in 9h.	4

This day the pan of coals was not lighted, the room being dry, and yet sidereal seems not to gain of the library more than it did yesterday.

So that I reckon I leave it this night about 20″ too slow, and going nearly sidereal time.

1725, *Tuesday, December* 21.

h. ′ ″	
9 00 00	library, a. m.
3 38 11	sidereal.
6 38 11	difference.
6 36 04	difference yesterday at 8h. p. m.
2 07	gain in 13 hours,

which is what it should gain of mean time; but library now gaining about 7″ or 8″ a day of mean time, it seems now sidereal will gain also thereabout of sidereal time; at least I think it should not now be altered. I suppose it did not gain yesterday, the effect of the warmth not being over.

Mr. Bradley found index at 7½, adjusted it, and then it stood at 8; and this at 5h. 40′ sidereal.

5h. 48′ 22″ Cap. Drac. entered.

5h. 49′ 52″½. Star at the cross.

5h. 51′ 24″. Star went out.

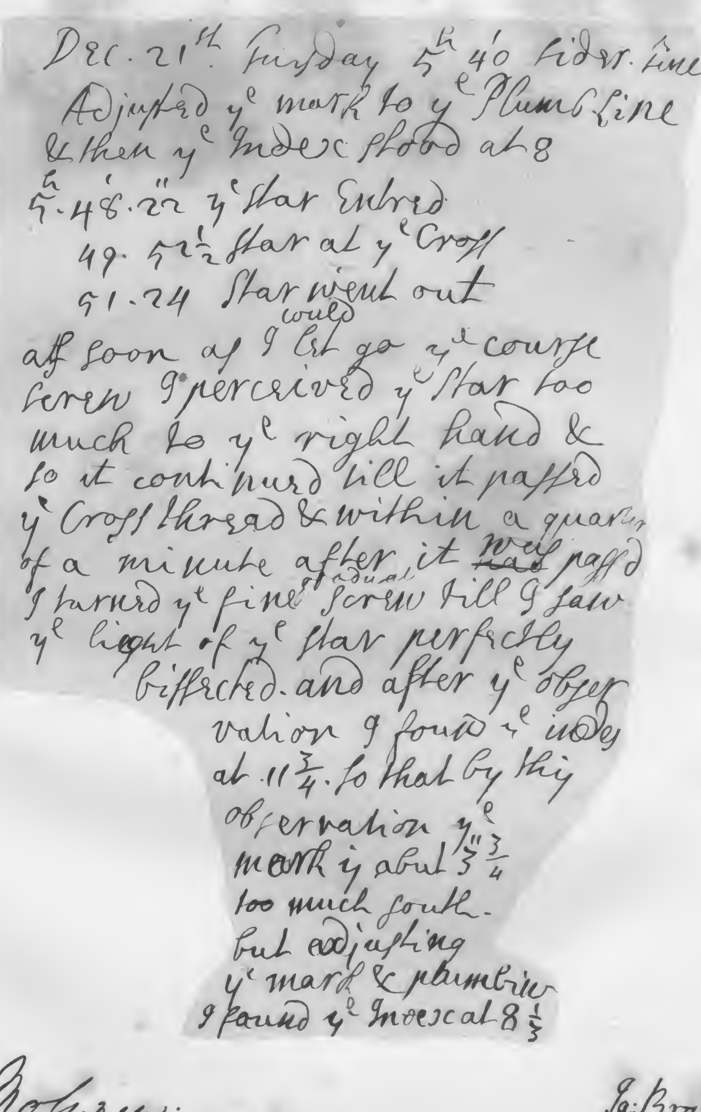
h. ′ ″	
5 49 52½	
50 10	
17½	too slow.

As soon as he could let go the coarse screw, he perceived the star too much to the right hand, and so it continued till it passed the cross thread; and within a quarter of a minute after it was passed, he turned the graduated screw till he saw the light of the star perfectly bisected; and after the observation I found the index altered to 11¾, so that by this observation the mark is about 3″¾ too much south.

But examining it by the plumbline again at 6h. sidereal, he found it stood at 8⅓, where we left it, which will diminish this to 3″½.

He takes this to be a very exact, certain observation, and prefers it to that of the 17th instant.

125.



Dec. 21th. Tueſday $5^h\,40'$ ſider. time
Adjuſted ye mark to ye Plumb Line
& then ye Index ſtood at 8
$5^h.48'.22''$ ye ſtar Entred
49. $52\frac{1}{2}$ ſtar at ye Croſs
51.24 ſtar went out
aſs ſoon as I would let go ye courſe
ſcrew I perceived ye ſtar too
much to ye right hand &
ſo it continued till it paſsed
ye Croſs thread & within a quarter
of a minute after it was ~~had~~ paſsed
I turned ye fine gradual ſcrew till I ſaw
ye light of ye ſtar perfectly
biſsected. and after ye obſer
vation I found ye index
at $11\frac{3}{4}$. ſo that by this
obſervation ye
mark is abut $3''\frac{3}{4}$
too much ſouth.
but adjuſting
ye mark & plumbline
I found ye Index at $8\frac{1}{3}$

S: Molyneux

Ja: Bradley

1725, *Friday, December* 24.

Cloudy morning.

h. ′ ″
1 31 00 library, post mer.
8 21 21

6 50 21
6 38 11 difference December 21st, at 9h. a. m.

12 10 gain in 3 days and 4 hours = 3′ 50″ per diem.
But library gains about = 7 per diem.

3 57 total gain.
3 56

1 daily gain of sidereal.

17″ sidereal too slow Dec. 21st.
3 = 3 days gains.

14 now too slow.

I saw Cap. Pers. pass this night.

Bisected at entrance 15$\frac{6}{8}$.

Bisected at centre 17$\frac{1}{2}$.

Bisected going out 16$\frac{7}{8}$.

Not very clear. As far as ever the snout will go south.

2h. 36′ 20″ nearly it went out.

1725, *December* 25.

Cloudy morning.

Wound up the clock at 2h. sidereal time, and stopped it about 5″.

I saw Cap. Pers. pass.

Bore it on the coarse screw, bisected at entrance, it went to the right most beyond the cross, when it was almost clear of the thread, and soon returned by degrees, and went out rather to the left.

2h. 36′ 17″ exactly, it went out.

It passed along the thread when the snout quite bears southward.

h. ′ ″
2 36 17
2 36 12 it went out the 19th.

5 gain in six days.

1725, *December* 27.

Hazy night, the air full of vapours.

2h. 33′ 18″ Cap. Pers. entered.

2h. 36′ 22″ it went out.

I bisected the star by the graduated screw near the cross as well as I could, but it fluttered about so with the vapours I could not distinctly determine when it was bisected exactly; wherefore I chose to leave the screw, so as that if there was any error the star appeared rather too much to the left than to the right hand, and consequently was in reality rather north of the thread than on it.

Then the index stood at 2,6.

Then I brought back the snout 18 revolutions and 2 seconds, (see December 4th,) and then it stood at 4,6; and then letting the plumbline settle, the line's south edge just touched the north side of the mark.

Then I brought it true to the plumbline, and at a medium of five trials, all within a quarter of an hour of the observation, no one differing more than $\frac{1}{2}$ a second from any other, I then found the index stood at 7,5.

So that the distance by which this star is now north of the mark, is more

	rev.	″
than	18	5
whereas on the 4th of December we found it	18	02
Difference		03 at the least,

the star being rather to the north of the thread than bisected this night.

Star true, it stood at		2,6
Mark true, it stood		7,5
		4,9 difference.
Besides rev.	18	
Star north now	18	4,9
Star north Dec. 5.	18	2
Change		2,9

At 3h. 35′ 00″ a bright one passed on the right hand, mark true to the plumbline.

3h. 36′ 09″. It went out.

This star bore a sufficient light to see the thread.

3h. 57′ 30″. One not so bright quite to the right hand, snout as in last star.

3h. 58′ 10″. It went out, scarce bore any light.

1725, *December* 28.

10h. 30′ mean time, found the index at $7\frac{2}{3}$.

Brought it to - - - 7,1.

Hazy, star not perceived.

1725, *December* 29.

11h. mean time, found index at 7,1.
Brought it to - - 6,6.
5 trials.

Hazy as yesterday.

1725, *December* 30.

11h. mean time, found index at 6,6.
Brought it to - - 6,9.
4 trials.

The weather was still cloudy, and I did not perceive the star till just it was going out.

At 17h. 51′ 41″ or 42″, as near as I could judge, it went out.

$$\begin{array}{lrrr} & \overset{\text{h.}}{17} & \overset{\prime}{51} & \overset{\prime\prime}{41} \\ \text{December 21st,} & 17 & 51 & 24 \\ \hline & & & 17 \text{ gain in 9 days.} \end{array}$$

Very calm evening.

This evening I fitted up the deal trunk below, so as I could leave the plummet always hanging on in water, and made two holes therein so as I could observe the mark and line, and also turn the graduated screw so as to adjust the whole without taking off the trunk; and I stopped the edges of it so with list as to exclude all manner of air, and the consequence I found as follows:

The plumbline would settle dead still in about two minutes.

I could put in my hand and alter the graduated screw without setting the plumbline in the least in motion.

Opening all the doors and moving the curtain would hardly put it in motion.

I could perceive a sensible alteration in moving the graduated screw less than $\frac{1}{6}$th of one second, and could reduce it to a truth again 10 times, one after another, and never vary $\frac{1}{6}$th of a second.

As the plumbline hung, I dusted on upon the surface of the water some fine hair-powder, and a good deal of it, and I did not find it produced the least alteration.

Then I fixed a bit of woollen cloth at one side of the plummet, about an inch long, and this had no sensible effect at all.

Then I threw in some chips and sawdust at one side of the line, and this made no alteration.

The line settled always so exquisitely truly to the same situation, that I was apprehensive it bore upon the brass edge where the mark is; but this was not the case, for moving it above the thickness of a sixpence clear of the edge, I tried all the same things over again, and always with the same success, as well as to the steadiness as to the consistency of it.

Then I got quite fresh clean water, and still the plumbline came perfectly true to the mark, and it stood at 6,9 at a med. of four trials.

So that from henceforward I shall always leave the plummet on, and shall adjust the telescope before I take the bottom part off.

1726, *January* 1.

All this day and yesterday we had very damp, rainy weather.

And afternoon this day, violent, stormy, rainy weather, south-east wind.

At 5h. p. m. I tried the plumbline, and I found the wind to-night would make the line vibrate near as much when the trunk below was on as when it was off.

Trunk on	6,3	Trunk off	5,3		
	5,6		5,7		
	6,3		5,4		5,9
	5,5		5,2		5,4
	23,7		21,6		11,3
Med.	5,9	Med.	5,4	Med.	5,6

1726, *January* 2.

5h. 30′ p. m. violent, stormy, rainy weather, with snow since last night, and at this time wind at north-east.

Found the index at 5,6.

Trunk on 5,8
6,4
6,0 } At a medium mark true at 5,9.
5,4
5,9
5)29,5(5,9

Plumbline vibrates as much as last night, arches of 3″ or 4″.

Then Mr. Graham examined and adjusted it immediately after, and the index stood as follows.

Trunk on 5,9
5,4
6,1 } At a medium mark true, by Mr. Graham, at 5,7.
5,4
5,6

5)28,4(5,7

1726, *January* 3.

10h. a.m. mean time, a very sudden change in the weather, wind quite laid, a clear, dry air, and violent hard frost; barometer had risen since last night near $\frac{3}{4}$ of an inch.

Mr. Graham this morning examined the instrument.

Found index at 5,7.

Brought it to 8,9. trunk on, 4 trials, none differing $\frac{1}{2}$ a second.

This is the suddenest and greatest change we have yet observed in the fixedness of the chimney. Close, warm, or damp weather, seems to bring their tops southward, and dry, clear weather, brings them back northward.

He says the plumbline remains now perfectly fixed, steady, and motionless, and that the instrument might have been now adjusted surely to much less than $\frac{1}{2}$ a second.

17h. 48′ 42″ Cap. Drac. entered, then I instantly discharged the coarse screw, and the star appeared about two diameters or more of the cross thread wide on the right hand. It did not appear with its usual lustre and brightness, but looked very dim and small; it moved extremely steadily and without the least fluttering, as I have seen objects in a horizontal line look through a reflector in a calm day, and a shower of rain.

N. B. Much snow fell yesterday, and some again this evening, and a good deal on January 4th.

Then at about a minute after its entrance I bisected it with the fine screw, and then the index stood at 1,6.

Then Mr. Graham lay down, and in less than $\frac{1}{2}$ a minute after it passed the cross he saw the star, and found it well bisected.

He staid till it went out, and it seemed to him to go out a small matter rather to the left hand of the thread, at 17h. 51′ 44″.

At 10h. 30′, examined it, and adjusted mark to the plumbline again as before.

Then index stood at 9,1.

Suppose index at 9 in the time of the star's passing.

17
9
8
1,6
9,6 by which the star seems now south of the mark.

At 2h. 30′ sidereal, examined

Trunk on	10,6	
	10,8	10,7
	10,8	

At 2h. 33′ 32″ Cap. Pers. entered distinct, brought it on the thread with fine screw.

Then I bisected it near the cross, and index stood at 5.

Then I brought back the mark to plumbline, and the index stood at

Trunk off	10,0		
	10,2		
	10,2	10,2	10,5
Trunk on	10,2	10,7	5
	10,2	9	5,5 + 18 rev.

Let at a medium 10,5 be the number at which the index stood in this observation.

Then the distance by which this star is now north of the mark is 18 rev. 5″,5.

This night being very clear, I observed from about 5h. 30′ sidereal, to 6h. 20′; glass true to the plumbline.

I saw no stars pass bright enough, but as follows; and of them but one that I believe would bear a good light.

There was no assistant above, so I did not try what light any of them would bear.

5h. 36′ 38″ went out not very bright on the right.

5h. 41′ 18″ a very good bright one comes in a little to the left of the thread. Telescopica in aurigâ.

5h. 44′ 21″ it went out.

5h. 46′ 13″ one not so bright came in a little to the right of the thread.

5h. 49′ 16″ it went out. This I believe would bear a small light.

6h. 02′ 49″ a very bright one, quite wide to the left.

6h. 4′ 16″ it went out.

1726, *January* 7.

At 2h. 15′ p. m. Mr. Graham here.

A hard frost ever since January 3d, with frequent snows.

The wind now north-east, hazy air, thin white clouds, and there seems to be still much snow in the air, very calm.

The plumbline exceeding still.

Adjusted mark to plumbline by my eye, trunk off, and then index at 9,3.

Adjusted again with the eyeglass apparatus, and then index stood at 9,1.

Then we put on the trunk and adjusted it again, and then index at 9,0.

Adjusted it to the mark again about 6h. 30′ mean time, index stood at 8,6.

At 2h. 33′ 41″ Cap. Pers. entered.

Then I bisected it near the cross, and the index stood at 2,4. Mr. Graham then lay down and saw it well bisected.

At 2h. 36′ 44″ it went out, a small matter to the left as he thought.

Then immediately examined and adjusted it again, and the index stood at 9,4.

Let at a medium 9 be the number at which the index stood in this observation.

$$\begin{array}{r} 9{,}0 \\ 2{,}4 \\ \hline 6{,}6 \end{array}$$

Then the distance by which this star is now north of the mark is 18 rev. 6″,6.

About 10h. mean time adjusted it to the mark again, then index at 9,7.

5h. 41′ 28″ a bright telescopic in auriga, Anti-Draco entered to the left hand.

Then I bisected it near the cross, and the index stood at 5.

Mr. Graham also saw this star truly bisected; at 5h. 44′ 30″ it went out a little to the left.

Then brought back the mark to the plumbline, and it stood at 9,0; and to bring it thus back, I turned it one whole revolution + the difference between the numbers.

Let 9,3 be the number of the index in this observation.

9,3
−5,0
4,3 + one revol.

Then the distance by which this star is now north of the mark is one revolution 4″,3.

1726, *Sunday, January* 9.

Went to town.

Wound up sidereal clock, and it lost 4″ or 5″.

1726, *Saturday, January* 15.

Barometer rising fast since the last night.

2h. 33′ 50″ Cap. Pers. entered sensibly to the left hand, but seemed well bisected at the cross, and then went off sensibly to the left again, index at 1,8.

It twinkled and fluttered somewhat, but not much.

2h. 35′ 22″ it crossed.

2h. 36′ 53″ it went out.

h. ′ ″
2 34 59
0 35 22
23 too fast.

A calm, clear, frosty night. Then immediately examined it by the plumbline, and index stood at 10,1.

10,1
1,8
8,3 +

So that this star seems now north of the mark 18 rev. 8″,3.

When the Telescopica in Auriga passed this night, it was cloudy.

This night at 5h. 35′ sidereal, adjusted the mark to the plumbline, and then index stood at 9,3 the trunk on, and the same water as had been used since January 3d.

Then I changed the water, and at 6h. 15′ I found the index would then stand at 6,0. I took off the plummet and put it on again, and still index stood at 6,0.

Then I took off plummet again, and I took out some of the water, so as the plummet was but just covered, and trying it again, still the index stood as before at 6,0.

I note this to shew how sudden an alteration may happen in about half an hour's time, for I judge this alteration not produced by changing the water, but rather by a change of the weather, which was in effect very sudden about this time, it having been a very clear star-light frosty night at 5h. sidereal, and before 6h. the weather was suddenly very sensibly warmer and quite overcast.

The frame that carries the aperture, with the late damp weather, went on this night too stiff and tight, possibly therefore some alteration may have been made in putting on the aperture this night, as well as by the sudden change of the weather above mentioned[d].

1726, *February* 2.

Alone, p. m.

h.	′	″	
3	49	30	library.
1	12	51	moveable.
1	23	21[e]	

h.	′	″	
4	16	00	library.
1	39	25	moveable.
1	23	25	

Twice wound up sidereal since I was here. Loss about 5″ each time.

h.	′	″	
12	18	00	Giles took sun's centre dubiè.
	14	47	
	3	13	library too fast.

See December 30th, sidereal gains 17″ in 9 days.

January 15th, it was too fast	23	
Gain in 18 days about	34	
	57	now probably too fast.

At 4h. 50′ mean time, 2h. 30′ sidereal, hazy or so light I could not see Cap. Pers. Found index at 6.

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[d] This paragraph is in the handwriting of the amanuensis who copied out the description of the instrument at Kew.

[e] In this part of the book there are some errors in calculating the differences of mean and sidereal times, but it seemed best to leave the numbers as they are in the MS. Likewise in page 128, line 3, 2′ 29″ has not been altered to 2′ 31″, because Molyneux connected the rate of the clock with the numbers which he has there given.

Trunk off 7,0
7,1
7,4
7,2
Trunk on 5,6
5,4
5,2
5,3
Trunk off 6,7

Then I twisted plumbline four or five times round and found no alteration.

These differences may in some measure arise from altering the east and west screw, by which at various distances, I this night, for the first time, perceive a sensible parallax may be made, unless at all times the axis of the eyeglass is exactly truly directed to the plumbline.

Library clock stopped this evening at six o'clock; wound it up and set it again going. And then

h. ′ ″
9 17 30
6 53 05
1 24 25

h. ′ ″
9 20 30
6 56 06
1 24 06

1726, *February* 3.

Hazy, foggy morning.

I put forward library clock about 5′ at noon this day.

This night I tried again to make a parallax sensible by various distances of the plumbline, and I found I always could perceive it sensible, but scarce sensible when the spot and plumbline appeared exact in the centre of the eyeglass.

Hazy at 5h. p. m. no Caput Pers. to be seen.

About 7h. 45′ p. m. mean time, I examined the instrument by the plumbline carefully, the trunk off, it being a very clear, fine, calm night, freezing, but not very hard, barometer rising. Index stood at 5,0.

At 5h. 34′ 46″ the Telescopica in Auriga entered on the left, and moved along without twinkling, very steady, and rather methought dimmer than when I saw it before.

5h. 36′ 17″ it passed the cross, and thereabout I distinctly bisected it, and then index stood at 13,4.

5h. 37′ 48″ it went out.

Then I examined it again both with the trunk off and on, and then at a medium of six trials, none differing half a second, the index stood at 5,5.

Let 5,3 be the number of the index in this observation, then

5,3	or 17
−13,4	−13,4
8,9 plus one revolution	3,6
	+5,3
	8,9

so that the distance by which this star seems now north of the mark is one revolution 8″,9.

My servant will not own it, but I believe he let the clock go down in the black room, and set it again falsely.

This night I have put it forward about 7′, and now it shews very nearly true sidereal time.

1726, *Friday, February* 4. a. m.

h.	′	″	
8	33	00	library.
6	16	15	moveable sidereal.
2	16	45	

N. B. Library is now 2′ 28″ too slow.

Sidereal nearly shewing sidereal time.

This correction arises from having mistaken the day of the month, and must be made in the other book as well as here.

1726, *Saturday, February* 12. p. m.

The mean or library clock, Giles found at noon this day 1′ 16″ too slow, and it gains now about 10″ per diem.

By his account the sidereal gains about 3′ 50″ every day of the mean clock, wherefore it gains every day of sidereal time about 4″.

On the 4th of February sidereal shewed very nearly sidereal time, so that it is now about 32″ too fast.

About 7h. 15′ mean time, 5h. 15′ sidereal: this night a strong north wind blowing, I adjusted the instrument as well as I could as follows:

The trunk off 5,3	The trunk on 5,9
5,6	5,8
5,5	5,7
14	24
24	
6) 38 (6	Medium 5,6
36	

At 5h. 30′ 10″ it was exceeding bright and clear, a pretty bright one passed on the right hand.

5h. 31′ 21″ this went out.

About 3 or 4 minutes after, several pretty bright ones to the left, and some on the right hand.

Then clouds often.

5h. 42′ 10″ Telescopica in Auriga entered.

5h. 43′ 43″ it passed well bisected at the cross dubiè through thin clouds. So that before and at the time of the transit, and after it, I could not see it distinctly, and frequently lost sight of it quite. So that I judge this to be no very good observation, though I think it cannot err much. At the transit, index stood at 13,5.

Then I examined it again as follows; the wind blowing hard, so that I could not make the plumbline settle very steady by any means.

Trunk off 5,4	Trunk on 5,5
5,8	5,7
6,0	5,5
22	17
17	
6)39(6½	Med. 5,6

Let 5,6 be therefore the number of the index in this observation, then

5,6
−13,5 the star appearing to the left of the thread.
9,1 + one revolution.

So that the distance by which this star seems now north of the mark is one revolution 9″,1.

1726, *Sunday morning, February* 13.

Cold morning, wind strong, north-east, clear, but some thin flying dirty clouds, 7h. 00′ mean time.

Trunk on	4,8	Trunk off	4,5	
	4,9		4,7	
	4,7		4,6	
	24		18	
			24	
			6)42(7	Med. 4,7

At 5h. 49′ 43″ Cap. Draconis came in quite wide to the right hand, seemingly north of the thread, but really south thereof. It appears but dull, and sometimes the thin clouds flying over the zenith took it quite from my sight. It fluttered a little, and twinkled sensibly at times.

At 5h. 50′ 45″ I bisected it, and it was very clear and distinct.

5h. 51′ 14″ it passed, and seemed well bisected.

5h. 52′ 46″ it went out very dull and cloudy.

When it was bisected the index stood at 7,1; but to bring it there I had turned the screw above one whole revolution, increasing the numbers, the snout being to be brought northward.

At 7h. 45′ mean time, examined it again as follows:

Trunk off	6,4	Trunk on	6,0		
	6,2		6,0	4,7	
	6,3		6,0	6,1	
	9		0	10,8	½=5,4
			9		
			6)9(1		

Let 5,4 be the number shewn by the index in this observation, then the star appearing to the right hand of the thread, deducting 5,4 from 7,1 remains 1,7, whereby this star appears now south of the mark over and above one whole revolution.

From this day's observations, and that of the 3d of January, of this star, I suspect that there is an extraordinary diurnal refraction in the air, all the observations of the other stars, viz. Cap. Persei and the Telescopica in Auriga, which have been made in the nighttime, agreeing very well, and being very consistent among themselves. But this must be determined by farther observations, and whether it may not depend on the sun or moon's place, or any other regular cause.

At 7h. 10′ mean time, p. m. I examined the instrument and brought it true to the plumbline.

Then I brought it one revolution and 9″ northward, as the instrument stood last night when the Telescopica in Auriga passed, and I waited for the entrance.

It was so cloudy I could not see it pass, nor entering, nor going out. When it was about half-way passed the cross I saw it for an instant bisected, as I thought, but not long enough to make any exact observation; however, I am sure I cannot err not even in this coarse observation more than 2″ or 3″.

1726, *February* 20.

A clear evening, freezing, north-west wind.

Trunk off 9,8	
9,7	
9,4	
3)28,9(9,6	9,6 index afore transit.
27	
1,9	

At 5h. 36′ 15″ a bright one passed to the left hand.

At 5h. 44′ 22″ Telescopica in Auriga passed well bisected at the cross, index at 16,6.

The star glared and fluttered much, and sometimes I fancied it looked to have a double glare, as if there were two.

At 5h. 45′ 53″ it went out towards the left side of the thread.

Then immediately examined it again, and found the index thus:

Trunk off 8,5	
8,3	8,3
8,1	9,6
3)24,9(8,3	2)17,9(8,9
	14
	1,6[f]

Let then 8,9 be the number of the index in this observation, then

$$\begin{array}{r} 8{,}9 \\ -16{,}6 \\ \hline 9{,}3 + \text{one revolution,} \end{array}$$

by which this star now seems north of the mark.

---

[f] The second and third lines have been corrected, the numbers originally were 14 and 1,6 ought, therefore, to have been erased.

8,3
7,3
2)15,6(7,8
14
1,6

1726, *Saturday, March* 5.

Mr. Bradley here.

The sidereal clock was too fast about 1′, February 13th; in 18 days I judge it hath gained about 1′ 12″, so it may be now about 2¼ minute too fast.

1726, *Sunday, March* 6.

Mr. Bradley rose, and at 5h. 30′ mean time, adjusted the instrument, a very calm, fine, clear, frosty morning, north wind.

Trunk on 11,3	Trunk off 11,2
11,2	
11,2	

At 5h. 51′ 12″ Cap. Draconis entered quite wide to the right hand.

At 5h. 52′ 44″ it passed well bisected at the cross, and to bring it there Mr. Bradley was obliged to turn the screw one whole revolution, and from 11,2 to 15,3. It twinkled much and looked very bright.

At 5h. 54′ 16″ it went out a small matter to the left as usual.

Then adjusted it to the plumbline again, and the index stood at

11,2 Trunk off.
11,3 Trunk on again.

Let 11,3 be the number of the index in this observation. Then deducting 11,3 from 15,3 remains 4,0; whereby this star appears now south of the mark + one whole revolution.

He reckons this a very true and exact observation.

h. ′ ″
17 52 44 Time of passage March 6th.
17 50 12 R. A. Cap. Draconis.
— 2 32 Error Hor. now too fast, and it now gains about five seconds a day of sidereal time.

1726, *Saturday, March* 19.

3h. p. m. came down alone.

′ ″
2 32 sidereal too fast, March 6th.
1 05 gain in 13 days.
3 37 clock now probably too fast.

At 3h. 30′ stopped it 4′. So now it is about 20″ too slow.

I found a scum on the inward surface of the glass, and Giles says since the 6th it never hath been without one; and that sometimes it hath stood as it were in drops of rain, the snout and eyeglass on since the 6th always.

Then I took off that snout and put on the sarsnet snout immediately, and left it so.

Half an hour after there seems to be no alteration, but the scum or dew on the inside looked as before.

In an hour it grew smaller, and a white circle round it.

In less than two hours it was almost quite gone, the spot not being above $\frac{1}{4}$ of what it was at first, and what remains looks much more diluted, thinner, and scarce visible. The barometer altered in this time not above $\frac{1}{50}$ of an inch.

In two hours and a half the glass was quite entirely clear and black, not the least dew or sign thereof remaining.

1726, *Sunday, March* 20.

8h. 11′ sidereal time. About this time a pretty bright star passed near the centre of the glass, just bears the light.

None after till

8h. 45′ 10″ } a double star wide to the right hand, the last the brightest
19″ } bears light well.

8h. 46′ 15″ one not so bright, but bears light; to the left.

8h. 51′ 44″ a very bright one, Telescopica in Ursa Major, bears the strongest light. I bisected it at the cross, and index stood at 7,1; and the mark was got to the right hand, that is, south of line.

Then immediately I brought it back to the plumbline, and then

index stood at 12,0
from which deduct 7,1
Remains 4,9 by which this star is now north of the mark.

While I observed this star, which appeared very distinct, strong, and bright, with the utmost light we could give, I caused the north and south half of the object-glass to be alternately covered, and still the star seemed to move perfectly true along the thread.

At 9h. 31′ sidereal time, I began again to observe.

9h. 39′ 48″ a bright one to the right hand.

9h. 44′ 25″ another bright one somewhat more to right hand.

9h. 47′ 47″ a small one but just visible, with weakest light at the centre, can scarce be useful.

10h. 05′ left off observing.

1726, *Monday morning, March* 21.

I rose before day break, it being a clear morning. It grew somewhat foggy before the star came. About 16h. 50′ mean time, or a little before 5h. in the morning, I adjusted the instrument as follows; trunk off, very still, calm morning.

10,0 10,1
10,1 then I took out plummet and trying it again found 10,1
10,4 10,0

The medium of these 6 is 10,1.

At 17h. 47′ 48″ sidereal time, Cap. Draconis came in very refulgent, I observed it without a light, the hairs being sufficiently illuminated by the strong twilight. It fluttered much. It came in to the right hand of east and west thread. I bisected it at about a minute after its entrance, and it passed well bisected at the cross, index at 14.

As soon as it was passed, I caused my servant above to breathe upon the object-glass, and I could see no other effect from thence, but that the star looked a little fainter and dimmer. I directed him to blow again, and then it looked still dimmer and weaker, but it run still exactly true along the thread. He breathed a third time, and this almost quite effaced the star, so that for a second or two I could not see it at all; then I spied it again, and he breathing no more, I found it begun to recover its light and grew brighter and brighter, still running truly bisected on the thread; when it was about 12″ or 15″ from going out, it looked as bright as at first.

At 5h. 50′ 51″ it went out rather to the left hand.

Then I instantly examined it again, and found the mark was quite wide of the plumbline to the north, that is, to the left hand.

To bring it true again, I was obliged to turn the screw one whole revolution, and from 14 by 13 to

11,5
11,7
11,0
Med. - - 11,4
Med. before the transit 10,1
1,5

Let 10,7 be the number of the index at this observation, then deducting 10″,7 from 14″,0 remains 3″,3; whereby this star appears now south of the mark plus one whole revolution. If there was any error in this observation, it was that the 14 should be rather less than more, which would bring the star still less south of the mark.

	h.	′	″
Time of stars passing this day was	17	49	19
Star R. Ar. is - -	17	50	10
			51 clock too slow this 21st of March.

1726, *March* 22.

Observation made by Mr. Graham[g].

The morning very clear, I found the index at 11,7. After I had taken the plummet out of the water and put it in again, the index stood as follows:

12,5
12,5
12,6
12,7

The last of these was about 10′ before the star entered the glass; the star entered at 5h. 47′ 48″, it was very bright, and soon after its entrance I bisected it, and when it wanted about a minute of the cross, I desired Giles to observe what the index stood at, and then moved the screw sometimes one way, sometimes the other, to try within what limits I could determine an alteration in the bisection; from which trial I judge there may be an error of near a second arising from the light of the star diffusing itself on each side the thread. A few seconds before it came to the cross I bisected it as exactly as I could; it moved along the thread without any fluttering, passed the cross at 5h. 49′ 21″, left the glass at 5h. 50′ 52″ inclining sensibly to the left hand. The index I found was at 15,3 when it was bisected at the cross. The observation being over, I hung on the plummet again, and the index stood as follows:

11,3
11,2
11,1
11,1

The medium before the transit is 12,6
After the transit - - 11,2

The medium at the transit - 11,9 the difference between this and the star, 1 rev. and 3,4 or 20″,4.

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[g] This day's observation is in the handwriting of the amanuensis.

1726, *Saturday, March* 26.

Very clear and calm. Mr. Hadley here with me.

Trunk off	14,6	
	14,5	Med. 14,5
	14,5	

At 8h. 50′ 23″ Telescopica in Ursa Major came in to the left, very dim. A haze upon the glass, but not great.

8h. 51′ 55″ it passed well bisected. Index at 9,5.

Trunk off	15,5	
	15,3	Med. 15,2
	14,7	14,5
	15,2	29,7
		14,8

Let 14,8 be the number of the index in this transit, then deduct from 14,8 this number 9,5. remains 5,3 by which this star is now north of the mark.

1726, *Sunday, March* 27.

Very clear and calm fine night.

Trunk off	15,4	
	15,8	Med. 15,6
	15,7	

At 8h. 50′ 25″ Telescopica in Ursa Major entered to the left of the thread, it looked brighter than last night, and moved very steady.

8h. 51′ 56″ it passed well bisected. Index at 10,7.

8h. 53′ 27″ it went out a little to the left.

Trunk off adjusted again	15,5	
	15,4	Med. 15,6
	15,8	

Let 15,6 be the number of the index in this transit, then deducting 10,7 from 15,6. remains 4,9 by which this star is now north of the mark.

This day and yesterday a very great dew was observed on the inside of the object-glass, though the bottom of the tube was opened, and left so some hours. The heat of the sun which now commands the roof is, I fear, the reason of it. I believe we must make holes above.

The warmth of one's hand applied near the object-glass, yet not so as to touch it, will take off this dew in a few minutes.

At 9h. 05′ 07″ a bright star passed to the right, bears light well.

At 9h. 19′ 27″ another not so bright to the right hand, hardly visible with light.

9h. 33′ left off observing.

1726, *Saturday, April* 2.

With Mr. Bradley.

Found a great dew on the inside of the glass, and Giles says it hath been on and off several times since I was last here. A linen cloth warmed will take it off at any time, being laid on the outside of the glass, but not so soon as the hand. I took it off in 5 or 6 minutes, holding my hand over the glass, the muslin snout hath been on and the water under ever since I was here.

We were desirous to examine whether there might not some cobwebs adhere to the plumbline; to try this, it being a very calm day, we let down by a fine silk a small bit of lead put round the wire about 2 tenths of an inch diameter, and about an inch long; it came down very easily; but when we went to draw it up again, the silk had twisted round the wire, so that it would not go up again; this obliged us to cut the silk above, and draw it downwards. It was so twisted round the wire it stuck very much to it, and we could hardly get it down again; however, pulling it gently, and giving it time to loose and unwind itself, it came down at last without any damage done; we examined it immediately afterwards by the mark, as we had done before, and we could not perceive the least sensible alteration.

1726, *Wednesday morning, April* 6.

Before day I rose, at 3h. a very clear calm fine morning.

Trunk off 16,3
16,3 } Medium is 16,2 at 3h. 40′ mean time, about 15′ before the star passed.
16,0

At 17h. 43′ 41″ a bright star passed wide to the right hand.

17h. 44′ 20″ this star went out, bears light well.

Then came in a small star, as I judge about 20″ to the left of the thread, and it passed about the time as Cap. Draconis entered.

17h. 48′ 13″ Cap. Draconis entered on the right, very bright.

17h. 49′ 43″ it passed well bisected, index at 1,3.

17h. 51′ 15″ it went out, inclining as usual to the left hand.

Examined[h] it immediately after, and found the mark wide to the left or north of plumbline.

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[h] The edge of the paper is torn off here; but the word which is lost appears to have begun with *ex*, and it certainly ended with *d*.

To bring back I turned one whole revolution, and from 1,3 by 0 to

16,0
16,1 Medium as before the transit 16,2
16,3 which deducted from 17,0
16,1
leaves 8
Add 1,3
2,1 by which the star appears now south of the mark + one revolution, being just 19″,1.

The twilight was not sufficient to shew the threads, so that this was the first observation made in the night, and by the light of the lamp above.

1726, *Thursday, April* 7.

At 3h. 30′ a very clear calm morning.

Trunk off 0,4
0,6 —— 0,5
0,5

17h. 43′ 04″ bright star wide to the right hand entered.
17h. 43′ 41″ it passed.
17h. 44′ 20″ it went out.
17h. 49′ 44″ Caput Draconis passed well bisected, index at 1,8.

Trunk off 0,2
0,0 Med. - - 0,0
16,8 Med. before transit 0,5
Med. 0,2 which deducted from
1,8
leaves 1,6 by which this star seems now south of the mark + one whole revolution, being 18″,6.

1726, *Tuesday, April* 12.[1]

10h. 18′½ sidereal time, began to observe. From which time, until 10h. 34′ no stars appeared.

At 10h. 35′ 17″½ a bright one passed quite wide to the left, and bears light well.

10h. 36′ 12″½ it went out.

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[1] The entries for April 12 and 14 are written by the amanuensis.

At 10h. 49′ 23″ one passed on the left hand just out of reach, and bears a small light.

10h. 50′ 43″ it went out.

10h. 57′ 40″ a small one passed wide to the right hand, and just bears light.

10h. 58′ 35″½ went out.

11h. 04′ 33″ one went out to the right hand just out of reach, and hardly bears light.

11h. 32′ 50″ a very small one passed quite wide to the right hand, and scarce bears the weakest light.

11h. 33′ 45″ it went out.

At 11h. 48′ 00″ left off observing.

1726, *Thursday morning*, *April* 14.

Before day light about 3h. 10′ mean time.

The air seemed this morning to be very damp and hazy, with thin white watery clouds overspreading the whole sky.

This morning I made use of the small aperture for the first time, the short diameter about an inch.

$$\text{Trunk off } \left.\begin{matrix} 1{,}1 \\ 1{,}8 \\ 1{,}3 \end{matrix}\right\} \text{ Med. } 1{,}4.$$

What with the cloudiness of the sky and the smallness of this aperture I could hardly perceive the small stars which precede Cap. Drac. as may be seen in former observations of the 6th and 7th instant.

At 5h. 48′ 22″ Cap. Draconis entered sensibly to the right hand, as usual, though now sensibly less than before.

5h. 49′ 52″ it passed well bisected, and then I found I had turned the graduated screw near one whole revolution, and the index stood at 0,7.

5h. 50′ 23″ it went out.

The star in its transit danced and fluttered about vastly more than ever I saw it, insomuch that I could sometimes see the whole body of the star on the one side of the thread, and instantly afterwards upon the other side; so that I look upon it there may be an error of a second at least in this observation, if not more.

Immediately after I examined it again,

Trunk off 0,8
1,0 Med. 0,9
0,8 1,4 Medium above.
2,3
1,1 Medium of both.
−0,7 Index when star on cross.
0,4
which added to 17,0
17,4 by which this star appears now south of the mark.

1726, *Monday morning, April* 18.

Calm morning, about 3h. mean time. I was called too late, and had but just time to lie down under the glass and found the star already entered, and almost arrived at the cross; I made the following observation hastily and in a hurry. Star moved very true, and

At 5h. 49′ 44″ it passed, and I bisected it as soon after as I could, and index stood at 1,0.

5h. 51′ 16″ it went out to the left.

Examined immediately the instrument, trunk off, and found at a medium of four trials,

index at 0,5
deducted from 1,0
leaves 0,5
which deducted from 17,0
leaves 16,5 by which this star is now south of the mark.

1726, *Friday night, April* 22.

I sat up this night, for the first time, until the time of the transit of Caput Draconis; so that this is the fifth revolution of the star from the 18th instant. I used the small aperture.

I kept at the glass from about 3h. 31′ sidereal time, till Draco came in; there were frequent thin flying clouds, so that I am not sure I saw all the stars which passed, but two small ones I thus observed.

4h. 25′ 08″ sidereal time, a small star (ad boreum pedis Herculis informium præcedens telescopica) entered on the left. I bisected it on the cross, and index stood at 6,0. To bring it again true to plumbline, I was obliged to turn it two revolutions, and to 16,6; so that this star is now 44,6; two revolutions 10,6 north of mark.

4h. 42′ 10″ another (ad boreum pedis Herculis informium sequens telescopica) came in wide to the left. I bisected this also at the cross, and index stood at 14,7. To bring it back to plumbline, turned it 14 revolutions, and from 14,7 to 16,6; so that this star is now 14 rev. 1″,9 north of the mark.

4h. 45′ 11″ another brighter star passed south of the last, I believe within reach.

About 5h. 22′ sidereal time, two bright ones passed, but I did not observe them.

5h. 44′ 20″ quæ præcedit Cap. Drac. went out south.
5h. 48′ 13″ Cap. Draconis entered on the right hand.
5h. 49′ 43″ it passed well bisected, index at 14.

Index before the transit 16,6
Index after - - 17,1
Med. 16,8
17,0
0,2
14
14,2 by which this star is now south of the mark.

[This is the original journal of the observations made at Kew with the parallactic instrument; but the next page having been before wrote upon, as some of the following are likewise, Mr. Molyneux got a new book and had the contents of this transcribed, and then gave me this, together with his description of the instrument, with leave to publish it as I thought proper. J. BRADLEY[k].]

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[k] Rather more than half the book, marked K. 14, is occupied on one side of the pages by a catalogue of fixed stars, and some tables of reduction. Molyneux began at the other end to enter his observations at Kew, but when he had come to this part, where most of the left hand pages were occupied by what had been formerly written on them, it appears that he procured and used another observation-book. This has not been met with; but Bradley alludes to it in the beginning of some memoranda which he has written in the second and third pages of K. 14. These memoranda are no longer of any importance in themselves, but they are inserted here that the present publication may be more complete. The remarks on the velocity of light are curious

[The following account of the observations made at Kew with the parallactic instrument, is transcribed from the book (wherein they are entered as they were made) which is a copy of this as far as to April 22, 1726.]

1726, *April* 23.

Mr. Graham fixed on the new steel piece, for the end of the graduated screw to bear upon.

Before we put it on, the index stood at	2,5
after it was on we found it thus - -	2,1 2,3 2,3
then moved plumbline only; - -	2,3
moved both plumbline and screw -	2,4
lifted up plumbline and moved the screw	2,2
the same repeated - - - -	2,3

Finding the pasted paper above too stiff, we eased it this day from the tube; and we raised the whole tube by the screws above about $\frac{1}{20}$ inch, the irons

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as connected with the present inquiry: but they do not indicate that the writer was at all aware of the conclusions which Bradley deduced from it. The whole is as follows:

" In the two first pages of the book, which the observations entered in this till April 22, 1726, " were transcribed into, I find the following memoranda:

	h.	′	″
" Draco.	17	50	10
" Pers.	2	34	59

" 1″=$\frac{10}{7076}$ of an inch exact on a radius 24 feet 3,15 inches.

" Dubiè. The thread under $\frac{1}{300}$ of an inch=2½ seconds.

" Exact. The first plumbline $\frac{1}{284}$ of an inch=2½ seconds.

" The new one of June 6th, about $\frac{1}{205}$ of an inch; above 3 seconds.

" When numbers lessen, top of stack of chimneys hath gone southward.

" Parallax 30″. Light coming 71 days from the fixed stars.

" Diameter of Mag. orbis 75 millions of miles.

" Diameter of Saturn's orb 700 millions of miles.

" Distance of the fixed stars 13750 times the mean distance of the sun; 6875 times the diameter of the Mag. orbis; above 700 times the diameter of Saturn's orb. So that Saturn is always above 1400 times farther from the nearest fixed stars than from the sun. And the influence of such star, in respect to the influence of the sun at Saturn, cannot be above one in " two millions.

" Distance of the fixed stars in miles 1031000000000, which is above a million of million of " miles. A number, which to count from unity at 3 units in a second minute, and 12 hours in " a day, would take above one and twenty thousand years to tell.

" The diameter of the earth 7846 miles.

above having by degrees settled a little. We concluded this day to make the pulleys go easier[1].

Mr. Graham observed the small star, ad bor. pedis Hercul. informium præcedens telescopica, which entered about 4h. 25′ sidereal time. At the time of the transit index stood at 7; having altered the screw about 2 revol. and examining it after the transit, he found it stood at 2,2; this was one of the stars I observed last night.

$$\begin{array}{l} 17 \\ -7 \\ \hline 10 \\ \ \ 2{,}2 \\ \hline 12{,}2+2 \end{array}$$

rev. by which this star is north of the mark.

At about 4h. 42′ sidereal time, the other small star observed last night, Herc. infor. sequ. was in the glass, index at transit stood at 15,9; and after transit, having turned the screw above 14 revol. it stood at 2,0; so that this star is 3″1 and 14 rev. north of the mark.

At about 5h. 49′ sidereal time, Caput Drac. entered.

Index before transit at	2,0
At transit - -	15,1
After transit - -	1,9

The medium before and after is 2,0; which deducted from 15,1 leaves 13″,1; by which this star is now south of the mark.

" By diurnal rotation it moves about 1000 miles in an hour, and about 23540 miles in a day.

" By annual about 616434 in one day, which is about 27 times as fast as diurnal motion, and " is about 25601 miles in an hour, and about 427 miles in a minute. [This is wrote in Mr. " Gray's hand.]

" Light goes in a second about one diameter of the earth, and in about 7 or 8 minutes one " diameter of Mag. orb.

" The velocity of light to the velocity of the earth's annual motion is as 135500 to 6, which is " about 20000 times faster.

" Light of stars may not arrive at the atmosphere with so great velocity, for this is probably " greatest, being deduced from velocity of sun's light.

" If there be no sensible parallax in the fixed stars, they must be above 30 millions of millions " of miles off. A number, which to count twice as fast as above, would take up above 300 thou" sand years to tell. [This is wrote in Mr. Molyneux's hand.]"

[1] This first paragraph had been previously inserted by Molyneux; but having placed it immediately after April 18, without leaving space for the observations of the 22d, he drew his pen across it, and added as a memorandum, " transpose this to its proper date."

All these observations of this night were made with the large aperture; and the glare of this last star being now very considerable, and the other two being very small, one cannot exactly depend upon them, perhaps not to a second or a 1″½.

1726, *Sunday night, April* 24.

I observed the two small stars mentioned in the last night's observation. The first entered at 4h. 25′ 2″ sidereal time, I could but just discern it; by the observation it seemed to be two revol. 12″,8 north of the mark.

At 4h. 42′ the following small star entered, and this also was scarce discernible; it appeared to me 14 rev. 5″,2 north of the mark.

At 5h. 48′ 6″ Cap. Drac. entered, index before was 1,7; at the transit 16,6; after 1,7; hence star 14″,9 south.

N. B. All made with the large aperture, and therefore very doubtful.

The inconsistencies which appear in the observations of these three last nights of Cap. Drac. may arise either from that the small aperture diverts the appearance of the star from its true place, or from that the great glare renders it very difficult to bisect it, when the large aperture is made use of. I rather judge it to arise from the latter of those causes, for that I found upon the 22d instant, that the bisecting the star with only the large aperture on, the star would, upon putting on the small aperture, appear rather to the right hand of the thread; whereas this night, having bisected it with the large aperture, and afterwards putting on the small one, it made it appear sensibly to the left hand. It will therefore be necessary carefully to examine this point, and thereby to correct, if necessary, the observations of the 14th, 18th, 22d, 23d, and 24th; but if, as I suspect, only the uncertainty of the large aperture is in fault, then the observations of the 14th, 18th, 22d, will be to remain uncorrected, and those of yesterday and this day will only remain doubtful, differing each from other near 2″ from the difficulty of a true bisection.

1726, *Friday, April* 29.

Came down with Mr. Whitson[1].

It was cloudy weather till the night between Sunday, May 1st, and Monday, May 2d. Sunday night I sat up; a very calm fine night, but before the star came, somewhat foggy.

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[1] So in MS. probably Whiston.

Trunk off	4,5	
	4,7	Med. 4″,5
	4,3	

At 17h. 47′ 55″ sidereal time, Cap. Drac. entered on the right hand, somewhat dim on account of the fog, I used the small aperture, star fluttered somewhat, so that having bisected it by the small aperture, taking off that aperture, I could not well determine whether it was still truly bisected or not. I thought putting on the large aperture, it seemed to go off to the right hand, if any thing.

At 4h. 48′ 25″ it passed well bisected, index at 17″,0.

after, trunk off, index	″ 4,0		med. before transit	4,5
	3,9	Med. 4,0	after	4,0
	4,1		med. of all	4,2

Hence the star is 12″,8 south.

N. B. Lifting up the plummet sometimes, and once only making it vibrate, I found this day an inconsistency of 2″ arising, as I judged at first, from the loop of the wire of the plumbline being just in the surface of the water. For when I filled it higher with water it did not happen, but having at the same time drawn off the brass edge from the plumbline, perhaps it was not so free before as it should be. It may arise from the different situations of the volutions of the wire, or perhaps the plumbline was too near the edge. On Friday when I came down, I put up the new pulleys, they do very well, but I think the lead weight is now too heavy.

1726, *May* 5.

This day I considered the matter of the different apertures, as stated April 24th, and I find that the numbers will come out thus:

	If the small aperture causes a variation of the rays of 1″.	If the doubtful glare of the large is in fault.
April 14	″ 18,4 south	″ 17,4 south
18	17,5	16,5
22	15,2	14,2
23	13,0	14,0
24	14,9	14,0
May 1	13,8	12,8

or the same number minus 2″ each.

So that it seems most probable as yet that the small aperture is not in fault.

1726, *Wednesday night, May* 11.

Came down.

It was exceeding clear and calm this night, and very cold. I sat up however to observe Cap. Drac.

Trunk off	12,4	
	12,4	Mean 12″,4
	12,3	

At 17h. 47′ 12″ sidereal time, Cap. Drac. entered on the right hand. It fluttered exceedingly, and so much, that after I had bisected it as exact as I could, I saw the body of the star sometimes at one side of the thread, and sometimes at the other; I made use of the small aperture. But this observation I think doubtful to at least half, if not a whole second. When bisected, index stood at 4″,2.

At 17h. 50′ 14″ it went out sensibly to the left.

Adjusting it again after the observation, I found it as follows:

Trunk off	12,0	
	11,9	Med. 12,0
	12,1	

Med. of both before and after is 12,2.

Hence the star is 9″,0 south.

1726, *Saturday night between the* 14*th and* 15*th of May.*

Trunk off	12,0	
	12,0	Med. 12″,0
	11,9	

At 17h. 47′ 5″ Cap. Drac. entered sensibly to the right hand, and fluttered very much. In clear dry weather I find this fluttering is very great, from the continual descending vapours and dew of the night. When it passed I bisected it, but very uncertainly, index 4,2.

17h. 50′ 7″ it went out to the left hand.

Trunk off	11,8	
	12,0	Med. 11,8
	11,7	

Mean of, before and after 11,9.

Hence the star 9″,3 south.

This observation is doubtful to at least ½ if not a whole 1″. I used the small aperture, as I do now constantly.

1726, *Monday night between the 23d and 24th of May.*

Trunk off 15,6
15,5 Med. 15″,5
15,3

At 17h. 46′ 27″ Cap. Drac. entered, very dim, through thin clouds. It moved very steady and was very distinct near the cross; when I saw it well bisected, index 5″,3.

At 17h. 49′ 27″ it went out to the left.

Trunk off 15,8
15,6 Med. 15″,7.

Hence the star is now 6″,7 south. This I reckon a good observation.

1726, *May* 24.

When Cap. Drac. passed last night the clock was 17h. 47′ 57″, it should have been 17h. 49′ 50″. Hence clock too slow by 1′ 53″.

So that this day I put the index of the sidereal clock forward 3′, and it is now about 1′ too fast, as I judge.

1726, *May 25th, Wednesday night between 25th and 26th.*

″
Trunk off, before 17,0
16,7 Med. 16,6
16,0
After 16,2
16,5
16,7 Med. of all 16,5

At 17h. 49″ 9′ Cap. Drac. entered fluttering much, but seemed at times to touch the thread before I stirred the graduated screw. I bisected it very doubtfully, and index stood at 4″,0. Hence the star is 4″,5 south. This errs in defect, and I think this is a very doubtful observation. The number 4″,5 should be more.

1726, *Friday night, May* 27.

Index before 17,0
17,2
16,7
16,8 Med. 17′,0
after 17,0
17,1
16,8 Med. 17″,0.

At 17h. 49′ 1″ Cap. Drac. entered, the glare touching the thread, it fluttered, but not so much as in the last observation. I bisected it near the cross, and index stood at 4″,6. Hence the star is 4″,6 south of the mark. I reckon this a pretty good observation.

1726, *Sunday night, May* 29.

Trunk off	14″,9
	15,0
then lifted up the plumbline, index	15,8
	15,6

the inconsistency of these numbers made me conclude immediately that somewhat was wrong in the plumbline; however, I took the transit of the star as follows.

At 17h. 48′ 50″ Cap. Drac. entered, when it was near the cross I bisected it, and

index was at	4″,0
examining it again after the transit, index	7,0
lifted up plumbline	13,0
lifted up again	16,5
again lifted up	15,6

Being confirmed by these inconsistencies that there was some fault in the plumbline more than usual, I do not here enter the result of this observation.

1726, *Monday, May* 30.

This morning I examined the plumbline above, and taking off some of the pasteboard, I found a very strong, thick, fresh cobweb, which adhered to the tin tube, and to the side of the wooden one, and involved the plumbline quite round for several inches, adhering also to it in twenty different places, so that near two inches of its length stuck to this cobweb. I removed the cobweb, and then examining the instrument, I found it as consistent as it used to be, returning within less than half a second always to the same situation upon its being lifted up, which I frequently repeated; but I observed one particular which I never took notice of before. If the plummet is rotated round its axis, so as to twist as it were the wire about 10 or 20 times, and then lifted up and let alone, lifted up for a minute or two and then let down into the water again, it will rotate of itself so as to untwist, till at length the friction of the water stops it; and then examining the instrument, this will sometimes, but not always, make a sensible difference of a second or more, sometimes less and sometimes more. Rotating it again the contrary way as much, lifting it up and letting it rest a little while as before, and then examining it again, this will sometimes cause an inconsistency on the other hand of about as much. The situation of the coils of the wire, it not being stretched sufficiently truly into a straight line, as I suppose, make these alterations; but the utmost extremes I could not make to amount to above 2″ or 3″. And if the water is stirred, so as the wire may be less resisted in untwisting itself, I generally found that it would return truly to the same number again, as if it had not been twisted. But this should be a caution to us not to rotate the plummet in taking it off and on.

1726, *May* 30.

Endeavouring this morning to let down a board through the wooden tube to cleanse it from cobwebs, if any more should possibly remain, I broke the plumbline.

1726, *Tuesday evening, May* 31.

Mr. Graham came down and put on a new plumbline, somewhat thicker than the last. When it was fixed in the notch, and that screw which holds the plumbline pinched, we adjusted the instrument,

Trunk off ″2,8
then lifted up as high as for an observation, index 3,0
then put on the little brass cover above, and lifted up as before, index 3,0
lifted up again 3,0

The wind was pretty high, and the top of the tube uncovered,

lifted up again 2,4
again 2,4
then tied a thread to the plumbline, and let it hang to it, index 2,5

This day we also put up the white string, which we can move round the plumbline from top to bottom to clear away any more cobwebs, if they should come.

1726, *Tuesday night, May* 31.

Before	1,4	
	1,9	
	1,9	mean 1″,7
after	1,7	
	1,3	
	1,7	
	1,2	
	1,2	mean 1″,3

At 17h. 48′ 36″ Cap. Drac. entered very steady, touching the east and west thread, bisected near the cross, index at 4,4.

At 17h. 51′ 38″ it went out not so much to the left as usual, if any error, therefore, 4″,4 should be increased. Let 1″,5 be index at transit, then deducting 1″,5 from 4″,4 there remains 2″,9; whereby the star now appears south of the mark.

I reckon this a pretty good observation, and it demonstrates the consistence of the new plumbline with the old one; for on the 27th of May the observation was 4″,6, differing from 4″,8 in the table of sines only by 0″,2; and this of 2″,9 is in the same table but 3″,4; and if 4″,4 the number in this observation, is increased a small matter, the regularity and consistency will still be greater.

1726, *Wednesday night, June* 1.

Before 16,6
16,5
16,7
after 16,5
16,4
16,2
16,6
16,4

At 17h. 48′ 28″ Cap. Drac. entered on the thread, somewhat fluttering, bisected near the cross, index at 2,4.

At 17h. 51′ 30″ it went out sensibly to the left hand. Hence the star 3″,0 south.

1726, *Monday night, June* 6.

Index 16,3; then lifted the plumbline and made use of the white line, then

index 15,8
15,7
15,5
15,6
15,5
after transit, index 15,8
15,9
15,8 mean of all 15,8.

At 17h. 48′ 2″ Cap. Drac. entered on the thread, but more to the left than the right; when it came near the cross, it seemed rather too much to the right, I bisected it as well as I could, on account of the fluttering, index 17″,1. Hence star 1′,3 south. If any error, this should be less.

1726, *Thursday night, June* 7.

Before 17,0
16,7
16,4
after 16,4
16,6
16,5 mean of all 16,6.

At 17h. 47′ 58″ Cap. Drac. entered, fluttering much, it touched the thread, but was sensibly to the left hand near the entrance. When it came to the cross I thought it rather to the right, but I could not be sure. I bisected it as well as I could, and index stood at 17,3. Hence the star 0″,7 south.

1726, *Wednesday night, June* 8.

This night Mr. Bradley came down and brought with him the heavy plummet, it weighs in air 8oz. 16dwts. and in water 7oz. 14dwts. troy weight. He tells me Mr. Graham finds 206 diameters of the new plumbline in an inch; and that it will bear 14oz. troy in air. Before we put it on we examined the instrument with the old plummet, as follows:

″
0,6
0,5
0,6
then we put on the new one, index 0,0
0,0
0,1

then we put on the small one again, and dropped a piece of split quill down the wire, which was about half an inch long; it fell readily, so we were secure there were now no cobwebs. Then with the small plummet on,

″
index 16,8
16,6
16,5
then put on the large one, index 0,2
0,2
0,2

The new plummet is longer settling than the small one hitherto used, but to an amazing degree consistent and steadier. It vibrates about 5′. We conclude to use it always. Mr. Bradley made this following observation, lifting the plumbline each time.

Before 0″,1
0,1
0,2
0,2
0,0
after transit 0,2
0,2

Caput Drac. entered at 17h. 47′ 53″ sensibly, as he said, to the left hand, but touches the thread. As it drew near the cross it seemed bisected, however he thought it necessary a small matter to alter the graduated screw to bisect it more exactly, and then index stood at 0″,7 : the fluttering, he says, made it more doubtful than in the day, as I have found it. Hence the star 0″,5 south.

1726, *Thursday, June* 9.

Before 2″,3
2,1
2,2
after 2,2
2,2

At 17h. 47′ 48″ Cap. Drac. entered on the string, seemingly well bisected, but fluttered exceedingly. It had been a very hot day, and the air at this time was very thick and foggy. At the cross I bisected it very uncertainly, index at 3″,5; there may be an error of 2″. Hence star 1″,3 south.

1726, *Friday night, June* 10.

A violent hot day, a great dew.

Index before 3″,5
3,4
3,5
after transit 3,5
3,5
3,4

At 17h. 47′ 43″ Cap. Drac entered, and seemed to me well bisected on the thread, but fluttered exceedingly, so that I could not make any thing certain of the observation, nor can I be sure to two seconds or more; bisected at the cross with great uncertainty, index at 5″,0. Hence the star is 1,5 south.

1726, *Saturday, June* 11.

Still violent hot weather, great dews, thick air.

Before, index 6″,4
6,3
6,3
6,3
after 6,2
6,2
6,3

At 17h. 47′ 40″ Cap. Drac. entered fluttering, but not so much as the two last nights. Bisected at the cross, index 7,2. Hence the star 1″,0 south.

1726, *June* 12.

The weather still hot, and very bad air.

Before 6″,8
6,8
after transit 7,2
7,3

At 17h. 47′ 38″ Cap. Drac. entered, fluttering very much as usual in this kind of weather. It seemed to me almost detached from the thread on the left hand at its entrance. Bisected at the cross doubtfully, index 6″,8. Let 7″,0 be the mean number, then deducting 6,8 from 7,0 there remains 0″,2; whereby the star appeared this night, for the first time, north of the mark, as fixed December 3d.

1726, *Monday night, June* 13.

The barometer fell this day, and we had some rain, the weather much cooler and changing.

Index before 8″,6
8,7
8,5
after 8,5
8,4
8,6

About 17h. 47′½ Cap. Drac. entered almost detached from the thread on the left hand; it moved much steadier than it hath done since the hot weather, and I think this observation pretty good, index 7″,8 at passage. Let 8″,6 be the mean, as above, then by this observation the star is 0″,8 north of the mark.

1726, *Tuesday, June* 14.

Before $\overset{\prime\prime}{5}$,3
5,2
5,2
after transit 5,4
5,5
5,5

At 17h. 47′ 23″ Cap. Drac. came in clear of the thread to the left, or but just touching. It moved very steady, but frequently I lost sight of it by the clouds. Bisected at cross, index 3″5. Hence the star is 1″,9 north of the mark.

1726, *Thursday night, June* 16.

Before $\overset{\prime\prime}{10}$,4
10,6
10,5
after 10,6
10,6
10,7

At 17h. 44′ 21″ Cap. Drac. entered on the left just clear of the thread, pretty steady, bisected at cross, index 8″,5. Hence the star 2″,1 north. A very sure observation.

1726, *Sunday, June* 19.

Before $\overset{\prime\prime}{4}$,5
4,3
4,4
after 4,5
4,4
4,4

At 17h. 47′ 6″ nearly, Cap. Drac. went out, index at cross 1″,2. It moved exceeding steady, a very good observation. Hence the star 3″,2 north.

1726, *Saturday, June* 25.

Before 1″,3
1,3
1,4
after 1,4
1,4
1,5

At 17h. 43′ 29″ Cap. Drac. came in.
46′ 32″ went out, index at cross 13,6. Hence the star 4″,8 north.

1726, *Monday, June* 27.

Before 2″,6
2,7
2,8
after 2,7
2,6
2,7

17h. 48′ 21″ Cap. Drac. entered and moved pretty steady, so that I reckon this a pretty good observation, index at cross 14,0. Hence star 5″,7 north.

1726, *Wednesday, June* 29.

Before 0″,4
0,6
0,6
after 0,5
0,6
0,4

17h. 48′ 11″ Cap. Drac. entered and fluttered a little, no very certain observation, index at cross 11″,7. Hence star 5″,8 north.

1726, *Thursday, June* 30.

Before 1″,5
1,6
1,4
after 1,7
1,6
1,5

17h. 48′ 5″ Cap. Drac. entered, and moved pretty steadily, but frequent clouds, index at 12,1. Hence star 6″,5 north.

1726, *July* 1.

″
Index before 2,0
2,1
2,1

At 17h. 48′ 0″ Cap. Drac. entered, and fluttered much in passing, so that I could hardly make any thing of this observation. I bisected it as well as I could, index 13,2. Hence the star 5″,9 north.

1726, *Saturday, July* 2.

[J. Bradley observing, entered the observation.]

″
Index before 3,2
3,2
3,3
3,2
3,3
3,3
3,3
3,4
after, index 3,2
3,0
3,2

At 17h. 47′ 54″ Cap. Drac. entered pretty bright; soon after I began to bring the thread to the star, and some time before it passed the cross I bisected it, index at 11″,4. Clouds after that made the star appear so faint that I could not see it near the cross when the threads were illuminated, so that I had not the opportunity of judging whether it passed the cross truly bisected; but if it was not, the star must really have been more southerly than by the observation, because it seemed truly bisected before the transit. It did not flutter much. Before it went out it grew bright again, and went out rather to the left of the thread at 17h. 50′ 55″. Hence the star 8″,8 north.

1726, *July* 3.

″
Index before 4,7
4,8
4,7
after 4,8
and 5,0 the mean 4″8.

At 17h. 47′ 47″ Cap. Drac. entered. About a minute after I bisected it, and then Mr. Gray found index at 13,0; then I discharged the screw a pretty way, and bisected the star again about the cross, index 13″,5; then I moved the thread from off the star, and bisected it again as soon as I could, at least within ½ minute after it passed the cross, and after it went out I found index at 13,2.

I ordered Giles above to give a very strong light, which perhaps contributed a little to the steadiness of the star's motion, by taking off the glare. Suppose the mark 4,8. star 13,5. then the star was 8″,3 south of the mark. I take this to be a very good observation.

[N. B. These two observations were made by myself, and entered into the Journal by me. J. B.]

This was a clear night.

1726, *Wednesday, July* 6.

Mr. Bradley again, [entered by Mr. Molyneux.]

Before 5″,6
5,5
5,7
5,6

At 17h. 47′ 28″ Cap. Drac. entered pretty steady, bisected at cross, index 14,3. Hence star 8″,3 north.

1726, *Friday, July* 8.

Before 4″,9
4,8
5,0
after 4,9
4,9

17h. 47′ 15″ Cap. Drac. entered, cloudy. I could not see it distinctly, index 13,4; a doubtful observation. Hence star 8,5 north. Wound up the clock.

1726, *July* 9.

Night very clear, star moves pretty steady.

Before 5″,4
5,4
5,3
after 5,5
5,4

At 17h. 46′ 54″ Cap. Drac. entered, index at the cross 13,4. Hence star 9″,0 north.

1726, *July* 10.

Before $\overset{\prime\prime}{3}$,7
3,6
3,5
after 3,4
3,5
3,5
at transit 11,4 the mean 3,6.

Hence star 9,2 north.

1726, *July* 12.

Before $\overset{\prime\prime}{4}$,5
4,3
4,4
after 4,6
4,5
4,5 mean 4″,5.

At cross 12″,1. Hence star 9″,4 north.

1726, *Friday*, *July* 15.

Before $\overset{\prime\prime}{2}$,4
2,4
2,4

At the cross 10″,3; thin hazy clouds, star fluttered much. Hence star 9,1 north.

1726, *July* 18.

Before $\overset{\prime\prime}{3}$,5
3,3
3,3
after 3,5
3,4
3,3

At the cross 9″,7. It moved pretty steady; a good observation, star north 10,7.

Moved the white string round the plummet line.

1726, *Friday night, August* 5.

Before 5″,9
5,7
5,8
after 5,7 mean 5″,8.

Cap. Drac. moved but indifferently, somewhat fluttering, bisected at cross, index 8,9. Hence star 13″,9 north.

1726, *Tuesday night, August* 9.

Very clear calm night.

Before 8″,0
8,1
8,0
after 8,0 mean 8″,0.

17h. 45′ 6″ Cap. Drac. entered. I observed it this night by twilight, being the first time since March 21st. It moved exceeding steady. I called to the servant to put on the small aperture, which I used all this summer in the night observations, and it made no alteration in the bisection of the star: then having it on, I altered the graduated screw, and bisected the star again, and then taking it off, I found no alteration.

Bisected with the large aperture at cross, index 10,2.

I take this to be a very certain observation, star 14,8 north.

1726, *Wednesday, August* 17.

This night Cap. Drac. moved exceeding steady, I changed the aperture frequently, as on the 9th, without the least sensible alteration. Some cobwebs disordered the plumbline, so that I could make no observation this night.

1726, *Thursday, August* 25.

Mr. Bradley here.

This day I made the wheel and cloth to keep the plumbline from cobwebs, and we found it did mighty well. We could let it down and draw it up without the least sensible alteration in the plumbline. A very clear calm night.

Index before 11″,0
11,0
11,2
11,3
11,2
11,3

″
after 10,8
10,9
10,8 mean 11″,0

17h. 43′ 33″ Cap. Drac. entered on the left hand, Mr. Bradley observing it. Moves pretty steady; bisected at cross; index 11″,3: it went out as usual sensibly to the left, and takes this to be a very good observation, star 16″,7 north.

1726, *Friday*, *August* 26.

Dropped down a piece of packthread.

″
Before 12,2
12,2
12,2
12,2
12,5
12,3
after 12,1
12,2 Med. 12″,2.

Mr. Bradley observing.

At 17h. 43′ 31″ Cap. Drac. entered; bisected at cross, 10″,8 or rather more, for it did not go out so sensibly to the left as usual. Hence star 18″,4 north. If any error, this 18″4 should be less.

1726, *Sunday*, *August* 28.

″
13,4
Let down the cloth, then index 13,2
13,3
13,2
12,9
after obs. 12,9 mean 13″,0.

At 17h. 43′ 28″ Cap. Drac. entered, and moved pretty well; bisected at cross, 12″,1; went out a little to the left. Hence star 17″,9 north.

1726, *August* 29.

″
Let down cloth 14,0
14,2
14,2
after transit 14,0 mean 14″,1.

17h. 43′ 25″ Cap. Drac. entered; moved pretty steady, and went out as usual sensibly to the left; index 13″,6. Hence star 17″,5 north.

1726, *Friday, September* 2.

″
Let down the cloth 6,7
6,7
6,8
after 6,6 Med. 6″,7.

17h. 43′ 11″ Cap. Drac. entered; hazy air, but star moves steady, though not so distinct as usual; index 6″,4. It went out as usual sensibly to the left. Hence star 17″,3 north.

1726, *Monday, September* 5.

″
Before 6,1
6,0
6,3
6,2 med. 6″,1.

17h. 42′ 59″ Cap. Drac. entered; moved pretty steady, but hazy, and not so distinct as usual; went off sensibly to the left as usual; index 5″,0. Hence star 18″,1 north.

1726, *September* 7.

″
4,7
Then let down the cloth, before 5,8
5,8
after 5,8
5,8 Med. 5″,8.

17h. 42′ 52″ Cap. Drac. entered; very hazy; I can just discern it; goes off sensibly to the left as usual; index 6″,0. Hence star 16″,8 north.

1726, *September* 8.

″
Before 3,9
4,3
4,4
after 4,2 mean 4″,3.

17h. 42′ 49″ Cap. Drac. entered, scarce discernible; very hazy, but very steady; I saw it well near the cross, and bisected it; index 4″,0. Hence star 17″,3 north.

1726, *Friday, September* 9.

″
2,9
Then let down the cloth 3,6
3,6
3,5

″
after 3,2
3,2 mean 3″,3.

At 17h. 42′ 45″ Cap. Drac. entered; hazy, but very steady; went off to the left as usual; bisected at cross; index 3″,0. Hence 17″,3 north.

1726, *Sunday, September* 11.

″
Let down cloth 3,6
3,6
3,7
3,7
after 3,7 mean 3″,7.

At 17h. 42′ 36″ Cap. Drac. entered; very hazy; indistinct uncertain observation; index at 3″,7. Hence star just one revolution, or 17″,0 north.

1726, *Wednesday, September* 14.

″
1,2
then cloth 1,3
then changed the water 1,2
after transit 1,1 mean 1″,2.

At 17h. 42′ 25″ Cap. Drac. entered; I could but just discern it, the weather was so hazy and cloudy; index 0″,8. Hence star 17″,4 north.

1726, *September* 15.

″
Mr. Graham observing. 0,0
Let down cloth 0,0
0,0
16,9
after 16,8
16,8 mean 16″,9.

At 17h. 42′ 20″ Cap. Drac. entered; bisected at cross; index 15″,6: if any error, this should be more. Hence star 18″3. After the observation Mr. Graham wound up the clock; it lost about 12″ or 15″ in winding.

1726, *Sunday, September* 18.

″
15,9
Then let down the cloth 15,9
15,9

At 17h. 41′ 54″ Cap. Drac. entered; very bright, more fluttering than usual of late; index 15″,2. Hence star 17″,7 north.

1726, *Sunday, September* 25.

	″
	14,1
Cloth	14,1
	14,1
	14,2

At 17h. 41′ 28″ Cap. Drac. entered; very hazy and indistinct; bisected at cross; index 14″,4. Hence star 16″,7 north.

1726, *Tuesday, September* 27.

	″	
Mr. Bradley observing.	14,2	
then cloth let down	13,9	
	13,9	
	13,9	
after	14,0	
	13,8	mean 13″,9.

At 17h. 41′ 21″ Cap. Drac. entered; it appeared very faint, being very hazy weather. I bisected the star ½ a minute before it came to the cross;

	″
index	14,9
at the cross, index	14,4
after	14,4

Hence star 16″,5 north.

1726, *Wednesday morning, September* 28.

Mr. Bradley rose before day, to observe the Telescopica in Auriga, observed the last time the 20th of February last, quod vide.

Before	13,6	
after	13,8	
	13,7	
	13,5	mean 13″,7.

5h. 33′ 56″ Telesc. in Aurig. entered; bisected about ½ minute after it passed the cross; index 4″,8. Hence the star 8″,9 north.

1726, *September* 28.

	″
Mr. Bradley observing.	15,3
then cloth	15,5
	15,5
after passage	15,5
	15,5

At 17h. 41′ 19″ Cap. Drac. entered; pretty distinct, and moved very steady; index 16″,2. Hence star 16″,3 north.

1726, *Saturday, October* 1.

″
15,4
then cloth 15,6
15,4
15,4
after 15,5 mean 15″,4.

17h. 41′ 14″ Cap. Drac. entered; scarce discernible, it moved steady; index 16″,4; went out as usual. Hence star 16″,0 north.

After the observation I put the sidereal clock forward nine minutes. Look next time for star at 17h. 50′ 4″ if no error.

1726, *Friday, October* 11.

″
14,8
Cloth 15,0
15,0
15,0
after 15,0 mean 15″,0.

17h. 49′ 47″ Cap. Drac. entered; very steady, but faint; index 1″,1. Hence star 13″,9 north. Wound up the clock.

1726, *Monday, October* 17.

″
Before 13,3
13,3
13,3
after 13,3

At 17h. 49′ 41″ Cap. Drac. entered; moved pretty steady; index 0″,0. Hence star 13,″3 north.

1726, *Wednesday, October* 26.

Wound up sidereal clock before observation.

″
11,0
then let down cloth 11,2
11,1
after transit 11,1 mean 11″,1.

At 17h. 49′ 26″ Cap. Drac. entered; it appeared exceedingly distinct, but fluttered somewhat; went out as usual to the left; index 0″,4. Hence star 10″,7 north.

1726, *Sunday, November* 13.

Wound up sidereal clock.

1726, *Sunday, November* 20.

″
Before 3,6
3,9

At 17h. 49′ 50″ Cap. Drac. entered; dubiè for clouds; bisected dubiè at cross.

″
Index 17,1
after transit 3,8 mean 3″,8.

Hence star 3″,7 north. This is a doubtful observation, but cannot err above 1″½, if so much.

1726, *Saturday, November* 26.

[This observation is entered by J. Bradley.]

Clouds hindered me from observing Caput Persei, but the sky became very clear again before Telesc. in Aurig. passed.

″
Index 4,6
then cloth 4,8
4,7 mean 4″,7.

At 5h. 42′ 55″ circ. Telesc. in Aurig. entered to the left of the thread, and appeared very well. I bisected it just before it came to the cross, index 6″,2. As it passed on, the thread quite hid it when 'twas pretty strongly enlightened, but it appeared to the left when it went out; so that I judge it to have been pretty truly bisected; if there was any fault, the thread was too near the wall: after transit, index 4″,7. Hence the star is 15″,5 north. I take this to be a very good observation. J. B.

1726, *Sunday night, November* 27.

″
Before 4,9
4,8
4,8
after transit, let down the cloth, and found index 4,5 mean 4″,7.

At 2h. 35′ 10″ Caput Pers. entered to the left of the thread, which was moved 18 revolutions from the plumbline. Then I brought the snout as near the wall as the work would admit, but that was not quite far enough to bisect the star truly at the cross; for though it appeared upon the thread, yet 'twas more to the left than the right, the index at the passage was 6″,5; which substracted from 4″,7 leaves 15″,2 above 18 revolutions, by which this star is now north of the mark.

[N. B. This observation was made and entered in the book by J. Bradley.]

1726, *Tuesday, December* 13.

Mr. Bradley was here; and though it was a clear day, he could not see the star.

> [Memorand. I could not see the star this day, because the clock had stood, and we could not tell the exact time when to expect the star in the glass. J. B.]

1726, *Saturday, December* 17.

I was here, but could not see the star, though it was tolerably clear. It was occasioned, as I judge, by a damp on the inside of the eyeglass; and perhaps the same when Mr. Bradley was here. I wiped it very clean and dry this day. Some rain hath beaten in, and run down the telescope.

This day I wound up the sidereal clock, and set it by guess as near as I could.

1726, *Sunday, December* 25.

	″
Before	3,6
	3,6
	3,7

then I moved the snout southwards 18 revolutions.

At 2h. 30′ 50″ Cap. Pers. passed. I moved the snout southwards as far as I could, forcing it rather too much, and still the star was distinctly too much on the left or north of the thread, but adhering to it; then index was 3″,6; after transit, index 3″,1. Let 3″,4 be the mean, then the star is 16″,8 + 18 rev. north; and to this is to be added about 2″ or 1″$\frac{1}{2}$, on account that the star was still to the left of the thread considerably.

1726, *Monday, December* 26.

	″	
Before	2,3	
	2,4	
	2,4	
after	2,6	mean 2″,5.

17h. 44′ 28″ Cap. Drac. entered, very dim, scarce visible, but moved very steady; it was distinctly clear of the thread on the right hand. When on the thread I could scarce perceive it. Bisected well at the cross; index 9″,4. Hence star 6″,9 south.

N. B. By a medium of last winter's observations of the 20th of December and the 2d of January, it should have been this day 6″,4; it is by this observation 6″,9; difference 0″5.

1726, *Monday*, *December* 26.

	″	
Before	3,4	
	3,5	
after	3,3	mean 3″,4.

5h. 37′ 8″ Telesc. in Aurig. entered, and moved very steady; bisected well at the cross, index 15″,4.

At 5h. 40′ 09″ it went out, yielding as usual to the left of the thread. Hence the star is 5″,0 + 1 rev. north of the mark.

1726, *Saturday*, *December* 31.

	″	
Before	1,6	
	1,6	
	1,7	
after	1,7	
	1,7	mean 1″,7.

5h. 37′ 23″ Telesc. in Auriga entered, and moved steady, but the night was so foggy that I could scarce discern the star when the threads were sufficiently enlightened; bisected at cross as well as I could; index 13″,7. Hence star 5″,0 + one whole revolution north.

1727, *Sunday*, *January* 15.

	″	
Before	16,0	
	16,0	
after	15,9	
	15,8	
	16,0	mean 16″,0.

5h. 37′ 49″ Telesc. in Aurig. entered; very bright and clear, and moved very steady; a windy night; bisected at cross, 8″,1. Hence the star 7″,9 + one whole revolution north. It went as usually, yielding distinctly to the left hand; which I began to perceive when it was scarce half way between the cross and exit. S. M^x^.

1727, *Sunday*, *January* 29.

	″	
	17,0	
	16,8	
then cloth	17,0	
after	16,7	med. 16″,8.

5h. 38′ 26″ Telescop. in Aurig. entered on the left, and moved very steady; a calm night, clouds now and then; bisected pretty well on the cross; index 6″,7. Hence the star 10″,1 + 1 rev. north. It went out as usual to the left.

1727, *Sunday, January* 29.

″
Before 16,9
16,8
17,0
after 16,7
16,8 mean 16″,8.

At 8h. 48′ 20″ Telescop. in Urs. Majore entered, on the right hand of the thread. I bisected it well on the cross, and the index stood at 1″,7; then having brought the snout of the glass northwards above one revolution, deducting 16″,8 from 1″,7, there remains 1″,9 + one revolution whereby the star is north.

See March 20th, 26th, 27th, 1726, then about 5″,0 north.

1727, *Sunday morning, February* 5.

About eight in the morning; clouds prevented me seeing the entrance and transit of Cap. Drac.: but about 20″ afore it went out I saw it for a few seconds, and brought it then on the thread, so as I judge it was bisected when it passed the cross; but cannot be sure to nearer than 2″ or more, having scarce seen it long enough to have time to turn the graduated screw.

About 17h. 46′ I judge it entered; the index as I left it from the above estimated bisection stood at 14″,2; examined it by the mark after the observation,

″
index stood at 14,0
13,7
13,8

Suppose the mean 13″,8, then the star was 17″,4 south. But this is a doubtful observation, I guess the 17″,4 should be more.

1727, *Sunday night, February* 5.

″
Before 14,4
14,3
14,4
after 14,4

5h. 38′ 42″ Telesc. in Aurig. entered on the left, very bright and clear and steady, bisected at the cross 4″,0. Hence the star 10″,4 + 1 rev. north.

The instrument above is gone so far eastward, I fear we must take it down. Memorandum, to speak to Mr. Graham about it.

1727, *Saturday night, February* 11.

Mr. Bradley observing.

	″	
Before	15,0	
	15,2	
	15,1	
after	15,2	mean 15″,1.

At 5h. 38′ 47″ Telesc. in Aurig. entered, and moved pretty steady, but looked very dim and hazy; index at 1″,7. Hence star 13″,4 + 1 rev. north.

1727, *Monday morning, February* 19.

	″	
Before	11,7	
	11,8	
	11,9	
after	12,0	
	11,9	mean 11″,9.

At 17h. 46′ 13″ Cap. Drac. entered quite wide on the right hand.

At 17h. 47′ 43″ it passed.

At 17h. 49′ 14″ it went out very distinct and steady; good observation; bisected at cross; index 16″,9. Hence the star is 5″,0 + one revol. or 22″ north of the mark.

[Quære whether south—it should be said south of the mark.]

1727, *Sunday, February* 26.

Mr. Bradley observing.

	″
Before	12,1
	12,2
after	12,2

8h. 48′ 56″ Telesc. in Urs. Maj. entered on the right hand, and moved very steady; a good observation; bisected at cross; index 9″,3. Hence the star 14″,1 south of the mark.

1727, *Thursday, May* 25.

I cleaned the parallax instrument this day from several cobwebs which had gathered about it while I was away at London; then I brought the mark to the line,

″
index at 8,3
8,5
8,5
8,4
then I let down the cloth a second time, index 8,5
8,6
8,4
let it down a third time, index 8,6
8,5
8,5
fresh water and the plummet taken off, index 8,4
8,5

1727, *Sunday night or Monday morning, May* 28.

″
Index 11,5
11,9
11,3
and 12,0

then suspecting some cobwebs from these inconsistencies, I let down the cloth, but had not time to examine the instrument again before the transit.

″
After the transit, index 11,3
11,5
suppose the true mean 11,6

At 17h. 42′ 54″ Cap. Drac. entered, fluttering much. Bisected pretty well at the cross; index 17″,4; it did not go out so much to the left as usual. Hence the star 5″,8 south.

The sidereal clock being now about six minutes too slow, I put it forward so much, and now it shews nearly true time, and is in a temper to lose about 6″ per diem.

1727, *Tuesday, June* 6.

″
Before 12,6
12,7
12,6
after transit 12,5
12,7 mean 12″,7.

17h. 47′ 32″ Cap. Drac. came in pretty steady, though not so well as I have seen it; bisected at cross; index 16″,7. Hence star 4″,0 south.

Several cobwebs were cleared before the observation, but I doubt not sufficiently. This is therefore a very doubtful observation. One cobweb was so thick it bore the cloth; formed in nine days' time.

1727, *June* 7.

For better preventing the cobwebs, and more readily drawing up the cloth, I this day altered the apparatus for that purpose. It now lets down at one side of the tube, not about the wire; it is fixed round the wire below, and cleanses all upwards.

	″
Before we used the cloth, index	11,5
	11,4
then lifted up the plummet and used the cloth	12,4
	12,3
	12,4
lifted plummet	12,4

about 10h. sidereal time.

1727, *Wednesday, June* 7.

	″	
Before	11,8	
	11,7	
	11,9	
	12,0	
after	12,0	mean 11″,9

At 17h. 47′ 27″ Cap. Drac. entered, very steady; bisected at cross very well; 14″,7; a good observation. Hence star 2″,8 south.

1727, *Friday, June* 9.

Before	11,8	
	11,9	
	11,8	
after	11,7	
	11,6	mean 11″,8.

At 17h. 47′ 13″ Cap. Drac. entered, fluttering pretty much; a doubtful observation. Bisected pretty well at cross; index 13″,8. Hence the star 2″,0 south.

1727, *Monday night, June* 12.

	″
Before	12,9
	12,7
	12,9

after 12″,7
12,8 mean 12″,8.

At 17h. 46′ 53″ Cap. Drac. entered, pretty steady; a good observation; bisected at cross, 14″,4. Hence star 1″,6 south.

1727, *Wednesday evening, July* 19.

I found the instrument very foul with cobwebs; index when the hole was bisected, was at 1″,9; then let down the cloth,

index 2″,7
2,8
2,7
after transit, index 2,8
2,9 mean 2″,8.

At 17h. 4′ 56″ Cap. Drac. entered, very steady and distinct, wide to the left hand, that is, north of the thread; bisected very well at the cross; index 8″,8. Hence the star 11″,0 north of the mark.

I take this to be a very good observation. After the observation I let down the cloth and changed the water, and found no alteration.

1727, *Thursday evening, July* 20.

Before, index 3″,8
3,8
3,9
after the transit, let down the cloth, then index 3,8
3,7

At 17h. 41′ 49″ Cap. Drac. entered to the left, fluttering pretty much; no very certain observation; bisected pretty well on the cross; index 10″,0. Hence star 10″,8 north.

1727, *Monday evening, July* 31.

[J. Bradley observed, and entered the observation.]

At 5h. 18′ sidereal time, let down the cloth,

index 9″,0
8,8
then lifted up the plummet, index 7,8
and 9,5

about 30′ sidereal time.

At 5h. 43′ 25″ Cap. Drac. went out; I bisected it very well at the cross, and index was 14″,4.

At 5h. 53′ index 5″,0.

At 5h. 56′ index was 4″,7.

At 5h. 59′ 'twas 4″,7.

At 6h. 1′ index was 4″,5. Then I let down the cloth, and

At 6h. 12′ index was 9″,4.

At 6h. 15′ 'twas

″
9,3
9,3
9,3

till 6h. 25′. Suppose therefore 9″,3 to be the index for this transit, then deducting 14″,4 therefrom, 11″,9 will be the star from the mark, north.

1727, *Tuesday evening, August* 1.

About 5h$\frac{1}{4}$ sidereal time, I let down the cloth, and then index stood at

″
9,8
9,7

after that I lifted up the plummet, then index stood at 10,0.

At 5h. 40′ 13″ Cap. Drac. entered, I bisected it well at the cross, and found

index at ″ 15,6
afterwards I brought the mark to the plumbline, and index stood at 8,6
and 8,8
then we let down the cloth, and index was 9,5
9,6

Let 9″,7 be index at transit, then deducting 15″,6 from 9″,7, there remains 11″,1; by which the star is north of the mark.

The differences between the several trials in bringing the mark to the plumbline in both these nights' observations, are greater than I have before observed, and seem to have arisen from the cobwebs, with which the trunk was very full. For on both nights the trials agreed well enough after the cloth was let down, both before and after the transits; but upon lifting up the plummet, the cobwebs seemed to have been again entangled with the wire.

[N. B. Both the last observations were made and entered by
J. Bradley.]

1727, *Sunday, August* 6.

	″	
Index	13,8	
then let down the cloth	13,5	
then lifted the plummet, index	13,5	
after transit	13,6	mean 13″,6.

At 17h. 39′ 20″ Cap. Drac. entered, somewhat fluttering, bisected at the cross; index 16″,8; a pretty good observation. Hence star 13″,8 north.

1727, *Tuesday, August* 22.

	″
Index	2,5
let down the cloth	2,5
lifted plummet, index	2,5
	2,4
	2,7
after transit	3,0
	2,8

At 17h. 37′ 12″ Cap. Drac. entered, pretty steady; bisected at cross, 4″,1; let 2″,7 be index at passage, then the star is 15″,6 north. Now Mr. Bradley's instrument is set up, and we go on comparing note from this time.

[Memorandum. My instrument was set up August 19, 1727. J. B.]

1727, *Sunday, September* 3.

Thin clouds; a doubtful observation.

	″
Index	11,5
let down cloth	12,4
lifted plummet	12,0
	12,1
after	11,8
	12,0

17h. 35′ 55″ Cap. Drac. entered, steady, but very visible through the thin clouds; at cross, index 14″,2. (by twilight.) Hence star 14″,8 north. Supposing index at passage to be 12″,0.

1727, *Monday, September* 4.

	″	
Index	7,2	
then let down the cloth, index	11,0	
let down again, index	11,1	
after transit, index	11,2	
	11,0	mean 11″,1.

At 17h. 35′ 49″ Cap. Drac. entered, very steady, and distinct all the way; a very good observation; well bisected at cross; index 12″,1. Hence the star is 16″,0 north of the mark.

1727, *Saturday, September* 23.

″
Index 7,8
7,6
then Mr. Gray let down the cloth, index 6,8
6,9
then the cloth let down again by John Haynes 6,8
then let down again by J. Haynes, 7,0

At 17h. 33′ 37″ Cap. Drac. entered, pretty steady, but not very distinct; bisected at cross;

″
index 6,9
index after passage 6,5
6,6

Suppose 7″,0 index at transit, then the star is 17″,1 north.

1727, *Wednesday, September* 27.

″
Index 0,6
let down cloth, index 7,0

then lifted the plummet and cleaned the plumbline above and below,

″
index 5,0
let down cloth, index 5,6
after transit, index 5,5
5,0 mean 5″,5.

At 17h. 33′ 9″ Cap. Drac. entered, very dim, and somewhat fluttering, at cross doubtfully it was 6″,2. Hence the star is 16″,3 north. A doubtful observation in all respects; I fear the tube, now full of cobwebs, must be cleaned, by taking down the plumbline and drawing cloths through it.

1727, *Sunday, October* 1.

Mr. Bradley here to clean the tube from cobwebs, we proceeded thus; first without touching the inside of the tube,

″
index 1,1
1,2
then let down the cloth as usual, index 1,6
1,6

then I took off the plummet and let the line coil up as much as I could, as for an observation; then fixing on the plummet again;

index 1″,9
1,9

then I let down the cloth and pinned a towel to it, and drew it up quite through the tube, and let it down again, and drew it up again; then

index was 1″,8

repeated the same again at another side of the tube,

then index 2″,0
and 1,9.

1727, *Friday*, *December* 29.

Index 5″,3
5,3
5,4

At 17h. 43′ 32″ Cap. Drac. entered, exceedingly dim, scarce visible; bisected at the cross doubtfully; index 13″,2. Hence star 7″,9 south of the mark.

[This is the last observation made and entered at Kew.]

MEMORANDA

RESPECTING THE INSTRUMENT AT WANSTED[a].

MARCH 28, looked at an object with the $12\frac{1}{2}$ feet glass, and when the object was distant from the object-glass 3 chains 80 links $=$ 250 feet $9\frac{1}{2}$ inches, the respective focus was 13 feet 2,75 inches. Hence the true focal length for distant objects is 12 feet 6,8 inches.

March 31. I put the glass into a turned cell, and found that the centre of the glass was not exactly in the middle of the cell; but turning it side for side, the difference occasioned an alteration in the place of the object about $\frac{1}{8}$ of a minute.

I then made a mark on the rim of the glass at that place where the centre lies directly in a diameter, so that if that line lies in the line of the axis of the tube or the wire the star moves along, no alteration in the aperture can affect the motion of a star north or south.

N. B. The centre of the glass lies nearer that side of the glass where the mark is made than the other.

April 1st, I tried, by looking at a fixed object, whether by placing an aperture not true to the centre of the object-glass, there would arise any sensible difference in the place of the object when the aperture was on different sides of the centre, but I could not perceive any; so that any apertures may safely be put in the cell of the object-glass, without fear of causing any alteration in the place of a star. I likewise tried by looking at a fixed object through the $12\frac{1}{2}$ feet glass with my eyeglass of $1\frac{3}{4}$ inch, to what certainty I could bring the thread of the micrometer to the same object; and by many repeated trials, I found that the extremes did not differ above one division or

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[a] The original, from which these are printed, is in Bradley's handwriting, and where nothing to the contrary is noted, the same is to be understood of all which follows in this part of the collection.

$\frac{1}{40}$ of a revolution of the screw, and therefore we may suppose that the eye can distinguish about half that quantity, or about $\frac{1}{80}$ of a revolution through that eyeglass, or one of 2 inch.

100 revolutions of the micrometer being 2,46 inches, one revolution is 0,0246 inch, and $\frac{1}{80}$ of that will be 0,0003075, or about $\frac{1}{3252}$ part of an inch. If the focus of the object-glass be 12 feet 6,5 inches, then $\frac{1}{3252}$th part of an inch subtends about 25‴, or not quite half a second, so that it seems that one might observe through this object-glass charged with a two inch eyeglass, so as not to differ from the truth above half a second, supposing the object fixed; and a star moving along a thread may perhaps be as exactly observed as a fixed land object, when it does not flutter.

$\frac{1}{3252}$ part of an inch in the focus of the eyeglass, supposing it $1\frac{3}{4}$ inch, is about 35″ seen through it: according, therefore, to this experiment, my eye can discern an angle of about half a minute, so that in the telescope which will magnify between 70 and 80 times, I shall be able (as above) to perceive any difference that exceeds $\frac{1}{2}$ a second.

April the third. I tried again what alteration was sensible through the 12 feet tube, and found that with the $2\frac{3}{4}$ inch eyeglass, I could always bring it to the same object within about a division of the micrometer, so that according to this, my eye discerns an angle less than $\frac{1}{2}$ a minute, or about $\frac{1}{3}$ of a minute.

Memorand. Land objects appear rather too dark through the 12 feet tube, charged with the shortest eyeglass of 1,8 inch. An eyeglass of about 2 inches or $2\frac{1}{4}$ may perhaps be better, because objects will appear more distinct and brighter.

The distance between the two floors is 7 feet 6 inches, the cross hairs must be 2 feet 6 inches above the lower floor, that is, 5 feet below the uppermost; and that part of the beam which will first touch the tube as 'tis moved towards the south, is 4 feet above the second floor, or 9 feet above the cross hairs. The focal length of the object-glass is 12 feet $6\frac{1}{2}$ inches, therefore the beam is 3 feet $6\frac{1}{2}$ inches from the axis of the tube. We may suppose the eye just 13 feet from the axis of motion, and 5 inches from each wall when the snout is farthest north; and if we suppose the telescope then to be six degrees from the zenith, there will then be room to take six degrees on each side the zenith, without touching the beam.

Memorand. The breadth of the object-glass is 2,85 inches.

April 5th. I tried an eyeglass which I had of [b]Scarlet, whose focus is 2,15 or 2,2 inches, and objects seem very distinct through it in the 12 feet object-glass.

April 5th. I cut away the beam; and then by a plummet took its distance (at 23 inches from the corner) from the plane of the bricks, and found it 16,7 inches at the height of the cross threads, or 30 inches from the floor.

I afterwards hung a plummet out at the top of the chimney, and took its distance at top and below, and found that the plane of the chimney, where the upper iron work must be fastened, hangs over about 0,15 of an inch, the wall at top being so much nearer the plumbline, than the wall where the lower fastening will be. I measured the distance also of the south-east corner of the wall, from the line above and below, and found that the top part did not project so much as the bottom by 1,5 inch exactly.

Memorand. 4½ inches will be enough from each wall for the snout at bottom.

Memorand. The cross hairs must be put just 2,1 inches from the vertex of the eyeglass, if I make use of that which I had of Scarlet, whose focus is 2,2 inches; for they will appear most distinct and clear in that situation, especially at the cross.

Memorand. If the aperture of the eyeglass is 1,35 inch, the telescope will take in an angle of just half a degree.

April 8. I tried the focal length of the 12½ feet object-glass, and found it (by help of a near object) for parallel rays to be exactly 12 feet 6 inches, or half an inch shorter than is supposed above.

Memorand. The distance between the wall at the bottom of our garden, and inner edge of the broad stone at the fore-door, is 2 chains 95½ links.

The arch of one second in a radius of 12½ feet, or 150 inches is $\frac{1}{1375}$th part of an inch.

April 14. I tried the focal length of the 12½ feet glass by several fixed stars, and found that the true focal distance is 12 feet 6,3 inches, so that the cross hairs must be 12 feet 6¼ inches from the inner vertex of the object-glass. This was done with care.

May 10th. The beam may be cut away till the lower part of it is 19 inches from the plane of the lower fastening. Suppose the diameter of the tube to be just four inches, and the snout to be five inches from the wall of the chimney which is to be broke into to let the arch move through it, and to be then 6° from the zenith, then the snout will be 19½ inches from the same wall when the telescope is vertical.

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[b] Scarlet was an optician, and lived near St. Anne's church, London. *Smith's Optics*, p. 378.

Suppose the tube to be just free of the beam when the snout is 6° towards the south.

Memorand. Mr. Graham in his draught (according to which he designs to proceed in making all the parts of the iron work, &c.) supposes that if a perpendicular be let fall from the centre of the glass to the wall, 'twill meet it just 20 inches from the corner, and that the middle of the glass will then be 14¼ inches from the wall.

Supposing the perpendicular distance as above to be 14¼ inches, and the diameter of the tube four inches, and the arch, the telescope moves, to be six degrees, then the beam must be cut away so as to be 18,37 inches from the wall, in order to make the tube just free from it; if therefore the beam is cut away nineteen inches, there will be full room.

Mem. June 12. At the beam, or 3½ feet from the object-glass, the rafters must be 26 inches from the south-east front of the wall, and from the other 18,4.

At the middle floor the opening must be 30,4 inches from the south-east wall, and 20,6 inches from the south-west.

June 11th, 1727, 22h. 57′½ by the clock, the sun was in the plane of the wall to which the iron work is to be fastened; the clock went 0′ 56″ too slow for apparent time. By this observation the plane of the wall at top is 28° 39′ from the south, easterly.

June 12th, 22h. 59′ by the clock, the sun in the plane of the wall. Clock too slow for apparent time 0′ 45″: by this observation the plane of the wall is 28° 16′ from the south, easterly.

"[b] This instrument, constructed by that excellent artist Mr. Graham, with his peculiar elegance and accuracy, was fixed up at Wanstead in the year 1727, for the use of that great astronomer Dr. Bradley; who, from his first year's observations with it, discovered the apparent motion of the fixed stars, which he called the aberration of light, and settled the laws of it; and, from the same observations continued for a course of twenty years, discovered the nutation of the earth's axis: two discoveries so profound, and at the same time so useful and necessary to the improvement of astronomy, that they will ever do him honour, while accurate observations and astronomical speculations are held in estimation.

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[b] The following description of the instrument is taken from Dr. Maskelyne's preface to the first volume of his Greenwich Observations, p. ix. nothing of this kind having been found among Bradley's papers excepting the preceding memoranda.

" The tube of the telescope, which constitutes the radius of the instrument, is about 12½ feet in length, made of iron plates tinned over, and is suspended at the top by two horizontal cylinders, placed directly opposite each other, on contrary sides of the object-glass; the common axis of these cylinders lies in the plane of the prime vertical, and they rest in upright angles, like the pivots of the axis of a transit instrument. The arch of the instrument, which is of brass, and comprehends twelve degrees and a half, or six degrees and a quarter on each side of the point answering to the zenith, is fixed firmly to the tube, exactly against the cross wires placed in the common focus of the object-glass and eyeglass. It is divided by fine points to every five minutes along the narrow vertical edge, but the divisions are numbered upon the upper or concave side of the arch, and shewed nearly the distances of stars from the north pole at Wanstead, with the limb facing the west.

" Parallel to the arch of the divisions, and at the same height, but behind the telescope, there is another arch, called, from its position, the back-arch; fixed firmly to the wall in the plane of the meridian, and serving to adjust and direct the fore-arch, and to hold the micrometer, which is screwed fast to it at the time of making observations: the fine steel screw of the micrometer is made to push the telescope at that time in the plane of the meridian, the telescope being drawn in a contrary direction to bear against the screw, by means of a weight fastened to a cord which passes over a pulley.

" In the original construction of this instrument, there was a notch placed exactly at the centre of the instrument, which was vertical when the telescope was so, into which the plumbline, fastened by a screw above the centre, was introduced, and filled it exactly: and from the near agreement of Dr. Bradley's observations together, it seems that this method of suspension of the plumbline was pretty accurate. Nevertheless, as it was still possible that there might be some small error in it, whether from the wire descending deeper into the notch at one time than another, or from its resting on one side of the notch in observations made at some distance from the zenith, I thought it better to have the suspension of the plumbline altered, after the manner of that which was adapted by Mr. Bird to the six feet sector, constructed by him for settling the limits of Maryland and Pennsylvania in North America. This was accordingly done by Mr. Bird; and, the notch being suppressed, a fine point made in a piece of gold was substituted in its stead, and accurately placed at the centre of the instrument in the year 1768.

" As this sector was originally designed to take only the differences of the zenith distances of stars in various seasons of the year, without any view of obtaining their true zenith distances, Mr. Graham was not so exact in laying off the total arc, as he would, doubtless, otherwise have been. For I find, by comparing together the differences of polar distances of stars found by the sector, with those found by the south quadrant, that the sector gives angles greater than the quadrant by 2″,6 in a degree. Therefore all the zenith distances deduced from the sector are to be diminished at the rate of 2″,6 in a degree. But the divisions, although not of the just quantity, seem to be laid off pretty nearly equal to one another; for the point of the sector answering to the zenith, as found by thirteen stars of different zenith distances, comes out the same within the compass of 2″; the greatest number for the zenith point being 38° 24′ 3″,9; and the least result 38° 24′ 2″,0: the mean of all the thirteen results being 38° 24′ 3″,0. Hence, the latitude of the Observatory being exactly 51° 28′ 40″,0, the zenith sector shews polar distances 7′ 17″ too small, with its limb facing the west in the quadrant room; but, with its limb facing the east in the transit room, it shews the complement of the polar distances of the stars to 76° 55′ 23″.

" It is farther to be observed, with respect to this instrument, that one revolution of the micrometer comprehends thirty-four parts; and that, by measuring with the micrometer the intervals of all the points from 37° 10′ to 39° 40′, the mean interval of the points came out 8 rev. 31,0567 parts, which is equal to five minutes of the arch: therefore one part of the micrometer is =0,989914 of the arch; from which the correction of this part of the arch for the error of the total arc at the rate of 2″,6 upon a degree, or 0″,000715, being subtracted, there remains 0″,9892 for the correct value of one part of the micrometer. Hence one revolution of the micrometer, or 34 parts, is equal to 33″,6328. The greatest interval of any two points, as measured by the micrometer in manner above mentioned, was 8 rev. 32,7 parts, and the least interval was 8 rev. 29,8 parts; the first of which differs 1,7 parts, and the latter 1,25 parts from the mean 8 rev. 31,056 parts. Also, by measuring the interval of two given points with different parts of the micrometer screw, the same measure came out without any greater difference than 0,3 parts from the mean result; whence, as one cannot answer for any particular measure, nearer than this, the threads of the micrometer screw may be concluded to be perfectly equal."

OBSERVATIONS[a]

ON

THE FIXED STARS

MADE AT WANSTED IN ESSEX,

BY J. BRADLEY.

Lat. 51° 34′½.

1727, *August* 19.

MR. MOLYNEUX and Mr. Graham came to Wansted, and we fixed up the brass work and tube, &c. of the parallactic instrument, the iron work having been finished the week before.

The radius of the instrument, or focal length of the object-glass, is 12 feet 6,6 inches, and the focal length of the eyeglass is 2,2 inches, so that the telescope magnifies sixty-eight times in diameter.

The wire in the common focus of the glasses is the smallest, that is commonly drawn, being $\frac{1}{440}$th part of an inch in diameter, and subtends an angle of 3″,113 at the distance of the object-glass.

The wire of the plumbline is much larger, being about $\frac{1}{190}$th part of an inch in diameter, and subtends an angle of 7″,21 at the same distance.

This wire will bear about 18 oz. troy.

The weight of the plummet (which I make use of) is 12 oz. 4 dwts. troy; of the other, 9 oz. 19 dwts.

The tin tube (before 'twas painted) and the brass hoops at the ends of it, weighed - - - 8lb. 15oz. averd.

The rest of the work fixed to the tube 6lb. 4oz.

15lb. 3oz. averd.

The object-glass, some screws, and the little tin tube, or aperture, are not included in this weight; we may therefore suppose that all the weight that is borne usually by the axis, is about 16lb. averd.

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[a] These Wansted observations are printed from a 4to book, marked W. 15, in which they are all entered by Bradley's own hand.

There are 81,8 threads in two inches of the screw, that the index, &c. is fastened to; so that one revolution of the screw subtends an angle of 33″,487 or $33''\frac{1}{2}$; and the plate upon this screw being divided in 34 parts, each division is somewhat less than a second, and each of these being divided into two; those smallest divisions are about half seconds.

When we had fixed up the instrument and put on the wire and the heaviest ball, Mr. Molyneux and myself tried with what certainty we could set the instrument, by bringing the same spot several times to the plumbline; and from the trials which we then made with the apparatus for enlightening the wire, &c. we concluded that when the plummet was dead or still, the instrument might be rectified to $\frac{1}{4}$ of a second.

One second upon the arch is the $\frac{1}{1370}$th part of an inch; and therefore $\frac{1}{4}$ of a second is but $\frac{1}{5480}$th part of an inch. The focus of the glass with which we viewed the wire and spots is about $\frac{9}{10}$ths, or somewhat less than an inch; if its focus was just an inch, then one second on the arch of the instruments viewed through this glass would subtend an angle of 150 seconds, or $2'\frac{1}{2}$. An eye therefore that can distinguish an angle of about 40″, might, by the help of the glass we used, distinguish about a quarter of a second on the arch, as we did. I have found by other trials, that when objects appear distinct and bright, my eye can distinguish to less than half a minute, especially when 'tis to judge whether a small spot is bisected by a thread that nearly covers it. For when I tried the object-glass belonging to this instrument, I found (by looking at a land object through it, that appeared distinct) that I could bring the thread of my micrometer to the same object always, without a difference in the several trials of more than one second, so that I was able to distinguish through this glass what subtended an angle of about half a minute in my eye, supposing the glass to magnify 60 times, which it did with the eyeglass that I then used. Upon these considerations, therefore, 'tis not so much to be wondered that the eye should see through such a glass as we look at the plumbline with, so small a part of an inch as $\frac{1}{5480}$, as that the plumbline should always return again to the same situation within that quantity; for 'tis evident that it will, by the trials one may make either by moving it to and fro, and then let it settle again; or by taking the plummet away, and then bringing it again; for in either case there is seldom a quarter of a second difference.

Aug. 19. Caput Dracon. Having fixed every thing, and satisfied ourselves as to the exactness of setting the instrument, we prepared to observe Caput Draconis, and adjusted 38° 25′ to the plumbline; index being at 18 rev. 31″,0.

At 7h. 5′ 11″ by my study clock, the star entered, and about a minute after I bisected it; then index stood at 21 rev. 1″,0; after that I tried to bisect it again, but thought it not very exact; however, Mr. Molyneux found index at 21 rev. 2″,0.

At 7h. 7′ 4″ the star passed the cross, when I bisected it again; index 21 1,6.

At 7h. 8′ 58″ the star went out. After this we put on the plummet again, and bringing the same spot to it, we found index the same as before; viz. 18 rev. 31″,0; deducting therefore 18 rev. 31″ from 21 rev. 1″,6, the remainder 2 rev. 4″,6 gives the distance of the spot from 38° 25′; the star being so much south of the spot. If we suppose one revolution of the screw to be 33″½, then this star is 1′ 11″½ south of 38° 25′ on the instrument; that is, 38° 26′ 11″½ from the pole, by the instrument. By Mr. Flamsteed's Catalogue, this star should be 38° 27′ 50″ from the pole, so that the divisions on the instrument shew the distance from the pole about 1′ 40″ too little, supposing the distance of this star from the pole to be true in the Catalogue.

By the time of this star's passing through the aperture of the eye-glass, the angle taken in by the telescope is about 35′ of a degree.

Aug. 20, β Drac. I brought 37° 30′ to the plumbline, index being 12 rev. 17″,3; afterwards I observed β Draconis well bisected at the cross, index being 7 rev. 21″,8; so that this star was 4 rev. 29″,5 north of 37° 30′.

After this I bisected γ Drac. at cross, index being 13 rev. 15″,8; having adjusted 38° 25′ to the plumbline, both before and after the transit, I found index at 11 rev. 12″,5; so that by this night's observation the star was 2 rev. 3″,3 south of the spot.

This star appeared with too great lustre this evening, and seemed to flutter and dance from one side of the thread to the other, as it passed along, so that 'twas difficult to judge when 'twas truly bisected.

Aug. 21. β Drac. bisected at cross, index 9 rev. 5″,9; before and after, index was 14 rev. 2″,8, when 37° 30′ was brought to the plumbline; so that by this night's observation, star is 4 rev. 30″,9 north of mark.

Aug. 21. γ Drac. passed; index 17 rev. 15″,0; before and after, index 15 rev. 11″,5. Hence star 2 rev. 3″,5 south of mark. Star fluttered much.

Aug. 22. Manè, Capella passed, bisected at cross; index 21 rev. 26″,4; before and after, index was 17 rev. 2″,8, when 44° 15′ was brought to the plumbline. Hence Capella is 4 rev. 23″,6 south of 44° 15′. The star fluttered much, and appeared with too much lustre, though I had put on a 2 inch aperture. I must lessen therefore the aperture more for bright stars.

1727, Aug. 22. After I had made this observation, I adjusted 44° 10′ to the plumbline, index being at 8 rev. 5″,5; as also 44° 20′, and then index stood at 26 rev. 2″; the arm that carries the screw and index being all the while fixed as before the observation. These ten minutes therefore on the arch, answer to 17 rev. 30″,5 of the screw. Hence one revolution is = 33″,53.

These marks compared with 44° 15′, give between 44° 10′ and 44° 15′, 8 rev. 31″,3, for five minutes; and between 44° 15′ and 44° 20′, 8 rev. 33″,2; the difference between them being but two seconds.

By Mr. Flamsteed's Catalogue, the distance of Capella from the pole should be at this time 44° 18′ 20″; by this day's observation 'tis 44° 17′ 37″, by the divisions on my instrument, that is only 0′ 43″ too little, differing almost a minute from γ Draconis.

Aug. 22. β Drac. 2 inch apert. This evening, as I was letting down the plummet in order to adjust the instrument for observing β Dracon., the wire broke five or six feet from the bottom: I suppose it was coiled thereabouts, otherwise I know not how to account for it. I immediately prepared to fix on another wire, and had finished it time enough to observe β Drac, which I bisected at the cross; index being 16 rev. 3″,7; before and after transit, index was 20 rev. 33″,5. By this night's observation, therefore, the star was 4 rev. 29″,8 north of 37° 30′.

γ Drac. 2 inch aperture. γ Drac. passed bisected at cross; index 22 rev. 28″,5; before and after, index 20 rev. 26″,5. Hence the star is 2 rev. 2″,0 south of 38° 25′.

By comparing this night's observations of these two stars with those made yesterday and the day before, it appears that different plumblines occasion no sensible alteration in adjusting or rectifying the instrument; for the difference between this and the last night's observations plainly arises from the uncertainty of bisecting the stars; the difference in the two stars being contrary to what it would have been, if it had proceeded from the wire.

	Rev.	″		Rev.	″		Rev.	″		°	′
ϰ Cygni passed; index	23	8,7	B.	22	11,0	star	0	31,7	south of	37	5
θ ——	24	13,7	B. & A.	20	28,0	—	3	19,7	south of	40	20
1π ——	23	4,2	B. & A.	21	18,8	—	1	19,4	south of	40	0
ζ Ceph.	24	21,7	B. & A.	22	19,0	—	2	2,7	south of	33	5
β Cass.	2	23,7	A.	3	22,7	—	0	33,0	north of	32	20

1727.

	Rev. ″			Rev. ″		Rev. ″		° ′
φ Pers.	11 30,8	- - -	B. & A.	11 13,0	star	0 17,8	south of	40 40
θ Pers.	12 32,6	- - -	B. & A.	12 14,2	—	0 18,4	south of	41 55
τ Pers.	16 18,5	- - -	B. & A.	14 20,5	—	1 32,0	south of	38 20
γ Pers.	14 30,6	- - -	B. & A.	17 6,2	—	2 9,6	north of	37 35
α Pers.	19 21,2	- - -	B. & A.	16 21,2	—	3 0,0	south of	41 5
δ Pers.	17 20,0	- - -	B. & A.	16 33,5	—	0 20,5	south of	43 5

These stars were all observed with the 2 inch aperture, and appeared with too much lustre, and fluttered much, especially the brightest of them; so that I believe 'twill be best to try a much smaller aperture.

I illuminated the threads this night by setting a small lantern between the wall and object-glass, upon the iron work.

Aug. 23. N. B. In writing down the observations in this book, I propose always after the name or character of the star, to set down first the number that the index stood at when the star was bisected at the cross; and then the number that the index pointed at, when the spot belonging to the star was adjusted or brought to the plumbline with the letters B or A, or both, to signify when the spot was brought to the plumbline, whether before or after the transit, or both before and after. And if there is a difference in the rectifying the spot before and after the transit, greater than ½ of a second, I shall set down both the rectifications; but if less than that, I shall set down the mean with the letters A and B, signifying that the index was the same when the spot was adjusted, both before and after the transit of the star.

		Rev. ″		Rev. ″		Rev. ″		° ′
Aug. 23.	β Drac. index	12 7,5	B. & A.	17 4,5	star	4 31,0	north of	37 30
	γ Drac. index	19 18,2	B. & A.	17 14,8	—	2 3,4	south of	38 25

Both these stars moved very steady this night, so that I take these observations to be good and exact.

Friday, Aug. 25. Manè, Capella 24 7,1 B. & A. 19 17,8 star 4 23,3 south of 44 15

Capella moved steady, this observation being made with an aperture of 1,4 inch. the long diameter of it lying east and west.

β Drac.	4 8,3	B. & A.	9 6,1	star	4 31,8 north
γ Drac.	12 4,6#	B. & A.	10 1,0	—	2 3,6 south.

Both these observations were made with an aperture of 1,4 inch. the long diameter lying east and west; the stars moved pretty steady, and seemed of such a diameter as to admit of judging when they were truly bisected, with more certainty than when the aperture is larger, as far as I can guess at present.

This day I fixed up a lamp for illuminating the divisions on the arch, and tried several glasses of different lengths, but none did so well as that which I have hitherto used, whose focal length is about $\frac{9}{10}$ths of an inch. I tried to bring a spot to the plumbline ten several times with each glass; and in that which I at present use, the extremes of the ten trials did not differ above $\frac{1}{4}$ of a second from one another, which is an agreement scarcely to be credited, had not experience shewn it so to be.

		Rev.	″		Rev.	″		Rev.	″		Rev.	″	
Aug. 25.	θ Cygni	17	29,9	flutt. B.	14	11,2	A.	14	11,6	star	3	18,5	south
	π —	17	25,3	B. & A.	16	7,8		-	-	—	1	17,5	south
	ζ Ceph.	19	31,4	B. & A.	17	27,5		-	-	—	2	3,9	south
	5th Lacert. Hev.	21	7,8	B. & A.	22	31,7		-	-	—	1	23,9	north of 43° 40′
Aug. 26. Saturday.													
	Manè, Capella	26	17,0	B. & A.	21	27,5		-	-	—	4	23,5	south

The star fluttered much; I used the 1,4 inch aperture, the long diameter north and south, open face north.

This day I finished the apparatus for illuminating the divisions and plumbline, and brought the line of collimation through the little glass to be exactly perpendicular to the divided arch, when the wire appears in the middle of the glass aperture. I soldered on a cap to the lamp to hinder the flame from disturbing the plummet, which it did very much, when the lamp was open; especially when it burned fierce, by making the plummet dance up and down. When I first perceived this motion of the plummet, I thought that it had been occasioned by a strong wind which then blowed on the chimneys, but as the flame of the lamp increased, I easily guessed at the true cause, and have endeavoured to provide against it for the future by means of the aforementioned cap.

I likewise painted the tube this day, and fixed on the spring that bears the telescope against the fixed brass arch; all the observations having hitherto been made without that spring.

When I had fixed on the spring, and set it free from the brass arch, I turned the screw (that raises or falls the east end of the axis of the telescope above) till the rollers below did but just bear lightly against the fixed brass arch; and having before turned the screw below (that moves the snout to or from the fixed arch) so as to be near its middle situation, I afterwards moved the top of the tube by the screw that carries the whole east and west, till the plumbline was at a proper distance from the divided arch, and then screwed all fast above.

1727.		Rev.	″			Rev.	″		Rev.	″	
Aug. 27.	γ Drac.	7	11,5 : :	-	A.	5	7,5	star	2	4,0	south : :
30.	β Drac.	3	30,6	half-way from cross	B.	8	29,0				
					A.	8	28,3				

Hence star is less than 4rev. 32″,0 north of 37°,30′.

γ Drac.	12	9,8	-	B. & A.	10	6,0	2	3,8	south	
d Drac.	5	24,5	-	B.	10	15,5	4	25,0	north of	33 10
138th Drac.	3	31,4	-	B.	6	3,6	2	6,2	north of	37 0
ϰ Cygni	6	30,7	- -	B. & A.	5	32,2	0	32,5	north of	37 5
θ ——	10	27,3	-	B. & A.	7	10,3	3	17,0	south of	40 20
2 o——	10	29,6	-	B. & A.	6	11,7	4	17,9	south of	44 0

N. B. This star is called a 5th magn. in Mr. Flamsteed's Catalogue, but 'tis brighter than that which precedes it, which he calls a 4th magn.

												°	′
1 f Cygni	6	13,2	-	B. & A.	6	9,0	star	0	4,2	south of	43	30
1 π Cygni	8	14,3	-	B. & A.	6	31,2		1	17,1	south of	40	0

Mem. All the observations hitherto made this night were with the 2 inch aperture, open face north. After this I put on the 1,4 inch aperture, and then observed as follows:

										°	′
ζ Cephei	9	1,2	-	B. & A.	6	33,5	2	1,7	south		
3 Lacert.	8	10,6	-	B. & A.	6	26,9	1	17,7	south of	39	5
7th Lacert.	5	28,0	-	A.	6	3,8	0	9,8	north of	41	5
3 Andr.	3	5,2	-	B. & A.	5	18,3	2	13,1	north of	41	25
τ Cassiop.	6	10,7	-	B. & A.	6	8,7	0	2,0	south of	32	50
β Cass.	6	20,0	-	B. & A.	7	19,3	0	33,3	north of	32	20
λ Cass.	12	3,7	-	B.	7	28,2	4	9,5	south of	36	55
α Cass.	14	21,8	-	B.	12	29,0	1	26,8	south of	34	55
π Cass.	16	4,4	-	A.	13	9,0	2	29,4	south of	44	25
θ —	10	4,8	B. 15 25,2	A.	15	24,6	5	20,1	north of	36	20
φ Pers.	8	18,8	-	B. & A.	8	5,6	0	13,2	south of	40	40
4th Pers.	6	19,5	-	B. & A.	8	0,5	1	15,0	north of	36	50

Then I broke the lantern with which I illuminated the cross threads, so that I could observe no longer.

N. B. This lantern was drawn up so as to illuminate the threads when the face of the aperture was north.

Aug. 31. Manè, Capella 13 10,1 - B. & A. 8 23,7 star 4 20,4 south of 44 15.

At the time of this observation the weather was a little foggy, and the star fluttered exceedingly, so that I could not judge when I had well bisected it at the cross; but as it passed on, I perceived from its general motion that the star was really more south than the thread, so that 4 20,4 is less than the truth. I likewise this day forgot to fix the moveable arm with both screws; upon both these accounts, therefore, this observation is not much to be depended on.

Rev. ″ Rev. ″ Rev. ″
γ Drac. 11 19,0 - - A. 9 16,3 star 2 2,7 south.

This night the star moved steady; but I perceived after it had passed the cross, that the thread was not quite north enough, so that the star is really more south than 2 2,7.

Sept. 1, Friday. Dr. Halley came here, but clouds prevented us from seeing either β or γ Drac.; after they were passed, the sky began to clear up, so that Dr. Halley could see.
° ′
138th Drac. index 10 3,0 B. 12 9,4 star 2 6,4 north of 37 0

He likewise bisected κ Cygni when 'twas $\frac{2}{3}$ from the cross; index 22 1,6; before and after 21 6,7. Hence star 0 28,9 + south of
° ′
Dr. Halley; θ Cygni 18 22,9 B. & A. 15 5,1 star 3 17,8 south of 40 20
I observed 2 o Cygni 21 22,7 B. & A. 17 4,7 — 4 18,0 south of 44

Sept. 2. Manè. Dr. Halley observed Capella; index 24 15,2; before 19 32$\frac{1}{4}$ after 19 32$\frac{3}{4}$. Hence star 4 16,7 south of 44 15. The star fluttered much, and he said that as it went out it appeared to be more southerly in reality than the thread; he therefore fancied from this observation that the direction of the thread was not right; but as all other stars at going out appear on the other side of the wire, this appearance must be owing to his not being able to bisect the star at the cross, because of its fluttering.

This day I painted the tube of the telescope over again.

β Drac. 16 23,9 [7][b] B. & A. 21 20,1 star 4 30,2 north

I take this to be a good observation, the star moving steady, and the thread being almost sufficient to cover it.

N. B. For the future, in setting down the observations, I shall mark those which I esteem good with this mark #, the better I think the observation the more strokes I shall make: those that are dubious I shall mark thus : :

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[b] The figures which are annexed between brackets to the several observations, denote the number of cross lines which Bradley had made against them, to mark the relative degrees of goodness which he considered as belonging to each: so [7] is substituted in this place for a similar mark to that which is inserted four lines below it in the text.

1727.		Rev. ″			Rev. ″			Rev. ″	
	γ Draconis	23 28,4	[8]	B. & A.	21 24,7	steady,	star	2 3,7	south
Sept. 3.	β ——	16 6,2	[8]	B. & A.	21 3,0	steady	—	4 30,8	north
	γ ——	22 27,9	[8]	B. & A.	20 24,6	steady	—	2 3,3	
Manè. 4.	Capella	23 31,5	[7]	B. & A.	19 8,5	steady	—	4 23,0	south

This morning I put on an inch aperture, which made the star appear round and well defined, so that I could bisect it with great exactness, the star moving during this observation pretty steady; for which reason I judge this to be a very good observation. The open face of the aperture was north.

2 inch ap.	β Drac.	15 33,7	[7]	B. & A.	20 29,5	steady	4 29,8	north
1,4inch ap.	γ —	22 13,9	[8]	B. & A.	20 10,2	steady	2 3,7	

After I had made this observation, I finished the new apparatus for illuminating the object-glass, &c. and altered the situation of the light; the lantern being now drawn up between the iron work that supports the tube, &c. so that the face of the aperture, when turned towards the light, is directly parallel to the wall that the iron work is fastened to, the direction of the longest diameter of the aperture being about 28° north of the east point of the horizon; but I don't apprehend that this can cause any alteration in illuminating the threads, or in the apparent places of the stars; however, I propose for the future to place the faces of the apertures the same way both when I use a light, and when there needs none; except I vary it on purpose to try whether I can find any alteration from the different position of the apertures.

										°	′
	2 ο Cygni	16 27,8	[4]	-	B. & A.	12 8,1	star	4 19,7	south	44	0
1,4 ap.	3 ω —	12 2,5	[5]	-	B. & A.	13 3,6	—	1 1,1	north	41	40
	3 Androm.	10 9,4	[5]	-	B. & A.	12 21,3	—	2 11,9	north	41	25
	τ Cassiop.	17 22,6	[5]	-	B. & A.	17 23,4	—	0 0,8	north	32	50
	β ——	16 31,2	[5]	-	-	18 1,6	—	1 4,4	north	32	20
	λ ——	20 15,4	[5]	-	B. & A.	16 8,3	—	4 7,1	south	36	55
	α ——	17 19,8	[4]	B. 15 28,8	A.	15 31,5	—	1 22,3	south	34	55

N. B. As I went to lie down, I touched the telescope and made it fly back, which I suppose occasioned this difference in adjusting the spot before and after the transit. Then clouds hindered me from observing any more before I went to bed.

Manè. 5.	Capella	13 1,0	[8]	-	B. & A.	8 11,3	4 23,7	south

This morning I put on the inch aperture, its face being set the contrary way to what it was in the last observation of this star; and from the agreement between the two observations, it appears that the different situation of the aperture occasioned no sensible alteration in the apparent place of the star.

Sept. 5. I saw for the first time η, or the tip of the great Bear's Tail, very distinctly; index at transit being 5 rev. 23″,3; after transit, index was 1 18,0, when 39° 15′ was adjusted to the plumbline; so that this star is by this observation 4 5,3 south of 39° 15′. The thread wholly covered the star, so that I could not see whether 'twas on the middle of it or not, but I suppose such an observation cannot differ much from the truth.

1727.		Rev. ″	Rev. ″	Rev. ″
Manè. Sept. 7.	Capella	17 11,6 [5]	B. & A. 12 21,4 flutt.	4 24,2 south

This morning the star fluttered; but I take this observation to be pretty good. I used the inch aperture with its face towards the wall.

Mem. I must use the lamp in the next observation to illuminate the cross threads, for they did not appear distinct enough in this.

2 inch ap.	β Drac.	7 18,3 [8]	B. & A. 12 14,3 hazy	4 30,0 north
1,4 ap.	γ —	14 31,6 [8]	B. & A. 12 28,7	2 2,9 south [8]
Manè. 8.	Capella	17 4,4 [5]	B. 12 14,8 A. 12 15,2	4 23,3 south

I illuminated the threads this morning for the first time, and used the inch aperture. Capella appeared pretty distinct, but fluttered much; the air was a little hazy, but I believe the observation is pretty exact.

				° ′
σ Cass.	10 28,1	B. 12 30,7 A. 12 31,1	2 2,8 north	35 45
β —	13 29,7	B. & A. 15 2,4 [6]	1 6,7 north	32 20
λ —	20 1,0 ::	B. & A. 15 28,3	4 6,7 south	36 55
α —	18 8,2 flutt. [4]	B. 16 20,9 A. 16 21,3	1 21,0 south	
2 υ —	11 23,7 [5]	B. & A. 9 28,3	1 29,4 south	32 15
θ —	3 14,1 [6]	9 05,6	5 25,5 north	36 20
34th —	12 0,3 [6]	10 14,3	1 20,0 south	33 10
φ Pers.	11 20,6 [5]	11 8,5	0 12,1 south	40 40

Then clouds prevented me from observing any more before I went to bed; but when Capella passed it was clear again.

Manè. Sept. 9.	Capella	17 5,5 [7]	B. 12 15,3 A. 12 15,7 [4]	4 24,0 south	
18th Camel. Hev.		12 6,0 ::	B. & A. 13 9,8	1 3,8 north	33 0
	γ Drac.	13 11,0 [6]	11 9,0	2 2,0 south	
1,4 ap.	d —	6 20,2 flutt.	11 14,4	4 28,2 north	33 10

1727.		Rev. ″		Rev. ″	Rev. ″	° ′
	c Drac.	14 13,0 flutt.	-	11 12,7	3 0,3 south	34 40
	136—	13 30,6	-	11 12,7	2 17,9 south	34 40
Inch ap.	ϰ Cygni	11 10,5 [6]	B. & A.	10 15,6	0 28,9 south	
	ι —	7 10,1 [6]	-	10 3,2	2 27,1 north	38 50
	θ —	13 31,1 [6]	-	10 14,8	3 16,3 south	40 20
	ψ —	11 13,2 [4]	-	12 9,8	0 30,6 north	38 15
	2 o —	16 32,5 [4]	-	12 16,2	4 16,3 south	
	1 f —	12 16,4	-	12 15,2	0 1,2 south	
	1 π —	14 3,6	-	12 24,6	1 13,0 south	40 0
	μ Ceph.	10 24,8	-	13 26,0	3 1,2 north	34 40
	ζ —	passed, by my study clock, at 9 53 51; index 16 15,9				
			B. & A.	14 19,0	Hence star 1 30,9 south	33 5
10h. 5′ 14″	3 Lacert.	15 19,0	B. & A.	14 3,6	— 1 15,4 south	39 5
10h. 18′ 33″	9 ——	13 18,5	-	14 20,1	— 1 1,6 north	39 50
10h. 44′ 17″	3 Andr.	12 27,7	-	15 9,8	— 2 16,1 north	41 25
Hazy.	8 —	17 23,0	-	15 7,8	— 2 15,2 south	42 25
	τ Cassiop.	15 16,0	-	15 17,5	— 0 1,5 north, hazy.	
	β ——	12 20,6	-	13 28,6	— 1 8,0 north	
1,4 ap.	λ ——	19 6,0 : :	- B.	14 33,3	— 4 6,7 south	36 55
	α ——	19 1,1	B. & A.	17 13,5	— 1 21,6 south	
	θ ——	11 5,8	-	16 31,2	— 5 25,4 north	36 20
	ξ Andr.	13 9,0	-	16 30,8	— 3 21,8 north	44 25
	φ Pers.	18 11,9 : :	-	18 0,1	— 0 11,8 south	40 40
Sept. 10.	Capella	25 8,5 : :	B. & A.	20 18,4	— 4 24,1	Manè.

Clouds prevented my bisecting the star at the cross, but I did it soon after. The star appeared faint because of clouds, and my using the inch aperture.

Sept. 11. I brought 38° 25′, &c. to the plumbline, and index stood as follows:

° ′	Index.			° ′	Index.	
38 25	0 17,3	8 30,2	Then I set the moveable arm	38 40	30 6,7	8 32,3
38 30	9 13,5	8 32,0	anew, and brought the same	38 35	21 8,4	8 32,2
38 35	18 11,5	8 32,5	spots again, but the contrary	38 30	12 10,2	8 30,5
38 40	27 10,0	26 26,7	way to the plumbline.	38 25	3 13,7	26 27,0

If 15′ is = 26 rev. 26″,7 then one revolution will be = 33″,60053.

If 15′ is = 26 rev. 27″,0 then one revolution will be = 33″,58945.

The difference in the two trials being but about $\frac{1}{100}$th part of a second in one revolution.

1727.		Rev.	″				Rev.	″		Rev.	″				
Sept. 11.	θ Cygni	11	0,8		-	A.	7	19,6	star	3	15.2	south			
1,4 inch ap.	2 ο —	10	29,7	: :	-	B. & A.	6	12,7	—	4	17,0	south			
Hazy.	3 ω —	5	29,3	: :	-		6	33,0	—	1	3,7	north			
2 inch ap.	1 f —	7	0,1	[6]	-		6	32,3	—	0	1,8	south			
	1 π —	9	0,5	[6]	-		7	20,6	—	1	13,9	south			
	ζ Ceph.	10	6,0	[6]	-		8	8,5	—	1	31,5	south			
	3 Lacert.	11	7,1	[4]	-		9	26,7	—	1	14,4	south			
Hazy.	9th ——	6	3,3	: :	-		9	31,7	—	3	28,4	north			
	β Cass.	8	18,5	: : ½ out	-		9	26,4	—	1	7,9	north			
	λ —	11	31,9	: :	-	B.	7	25,3	—	4	6,6	south			
	α —	14	19,5	flutt. too bright		B.&A.	12	32,8	—	1	20,7	south			
Hazy.	2 υ —	12	20,5	[6]	-		10	24,9	—	1	28,6				
	θ —	3	11,9	: :	-		9	3,9	—	5	26,0				
	ξ Androm.	7	4,6	[7]	-		10	25,5	—	3	20,9				
	φ Pers.	11	4,0	[4]	-		10	26,1	—	0	11,9			°	′
	4th —	9	23,7	[4]	-		11	10,1	—	1	20,4	north	of	36	50
	h —	13	22,8	[4] ½ out			11	2,2	—	2	20,6	south	—	40	10
	65th And.	5	16,0	[6]	-		11	2,4	—	5	20,4	north	—	41	0
2 inch ap.	θ Pers.	11	8,0	: :	-	B.	10	28,4	—	0	13,6	south			
	τ —	12	31,7	[4]		B. & A.	11	2,9	—	1	28,8	south			
Manè. 12.	Capella	14	16,0	going out			9	27,5	—	4	22,5+				
Manè. 14.	———	18	2,4	: : flutt.	-		13	14,3	—	4	22,1	inch ap.			
	18 Camel.	11	31,3		-	B. & A.	12	29,5	—	0	32,2	north	of	33	0
2 inch ap.	27 ——	18	5,3		-		13	25,9	—	4	13,4	south	—	33	5
	δ Aurig.	14	24,0	[5]	-	B.	14	5,6	—	0	18,4	south	—	35	45
	35th Camel.	17	0,2	[7]	-	A.	14	25,9	—	2	8,3	south	—	38	25
	46th Aurig.	14	24,9	[8]	-	A.	14	2,1	—	0	22,8	south	—	40	35

N. B. The 35th Camel. is the star that Mr. Molyneux has observed, which he calls Telescopica in Auriga.

14. η Urs. Maj. 14 13,7 [8] - B. & A. 10 7,7 star 4 6,0

This observation I take to be very good, as far as I may depend on truly bisecting the star when 'tis not bright enough to be seen on both sides of the thread, which it was not now, being wholly hid by the thread. The star came to the cross sooner than it ought to have been on the meridian about 13″. I did not see the star before 'twas very near the cross, by reason of some thin clouds; but after I had once perceived it, 'twas very easy to keep sight of it. I had brought the thread rather too near the star's track, which perhaps might hinder me from spying it sooner. I believe 'tis best for the future to set the thread about one revolution of the screw from the star's path.

1727.	Rev. ″	Rev. ″	Rev. ″	Rev. ″		° ′
γ Draconis	7 30,9 [6]	B. 5 27,9 : :	A. 5 28,5 [4]	2 2,5		
c ——	10 26,9 [6]	-	B. & A. 7 26,9	3 0,0		
136 ——	10 9,4 [6]	-	- 7 26,9	2 16,5		
ϰ Cygni	8 9,5 [8]	-	- 7 14,9	0 28,6		
ι —	14 1,7 [6]	-	- 7 31,1	6 4,6	south	38 45
ψ —	7 17,6 [6]	B. 8 14,5	A. 8 14,8	0 31,0	north	38 15
2 ο —	11 18,1 [6]	B. 7 2,5		4 15,6		
3 ω —	6 12,8 [8]	B. 7 17,3	A. 7 17,7	1 4,7		
1 f —	7 26,4 [6]	-	B. & A. 7 25,3	0 1,1	south	

9h. 3′ 17″ by study clock, 1 π Cygni entered; at 5′ 6″ 'twas at the cross; index 13 6,5 [6]; at 6′ 56″ star went out, B. & A. 11 28,1 1 12,4

9h. 31′ 53″ ζ Ceph. entered, 34′ 2″ at cross; index 14 4,6 [6]; at 36′ 11″ star went out, B. & A. 12 8,9 1 29,7 south

9h. 45′ 27″ 3 Lac. passed, 14 7,4 [8] - B. & A. 12 27,7 1 13,7

9h. 58′ 46″ 9 — 11 22,6 [8] - - . 12 26,2 1 3,6

Then clouds hindered me from observing any more. The wind blowed very hard at west all this night, which affected the plummet so that 'twas seldom dead; but by making a great many trials to bring the spot to the line, and taking the mean of all, I reckon that I can't differ materially from the truth.

Sept. 15. I saw for the first time β Ursæ Maj.; index 0 16,6 [5] B. 12 17,3 A. 12 18,6 [4]. Hence star 12 2,0 north of 32° 15′

This star came to the cross 17″ sooner than it ought to have been on the meridian, supposing its R. As. true.

	Rev. ″		Rev. ″	Rev. ″	° ′
γ Urs. Maj.	11 17,8 [4]	-	B. & A. 10 3,1	star 1 14,7	south of 34 45

This star appeared rather plainer than β; it was at the cross 15″ sooner than it ought to have been on the meridian.

ε Urs. Maj. 13 27,2 [6] B. 10 33,5 A. 11 0,5 star 2 27,0 south of 32 30

This star seemed to me brighter and distincter than either β or γ, though this is called a 3d magn. and those the 2d in Cat. Flam. It came 20″ sooner than it ought to have been on the meridian, supposing its R. Asc. true.

Clouds hindered me from knowing whether I could have seen ζ Urs. Maj. or not, as they did likewise of observing η Urs. Maj. at the cross; but about ½ a minute after it passed, I bisected it as well as I could; index 16 14,7 : : B. & A. 12 6,7. Hence star 4 rev. 8″,0 + south.

This day, as well as last night, the wind blowed hard at west, but the air seemed very clear when free from clouds.

By the time of the passing of 1π Cygni and ζ Cephei last night, the glass takes in about 35′ of a degree: suppose it to take in just 35′, then a star will be more southerly at the cross than in the other parts of the aperture, as in the following table, which is made to every 2 degrees of distance from the pole that the instrument reaches.

The distance from the north pole.	32°	34°	36°	38°	40°	42°	44°
Star coming in or going out.	4″,3	4″,0	3″,7	3″,4	3″,2	3″,0	2″,8
¾ of the dist. from the cross to the outside.	2,4	2,2	2,1	1,9	1,8	1,7	1,6
½ way from the cross.	1,1	1,0	0,9	0,8	0,8	0,7	0,7
¼ from the cross.	0,3	0,2	0,2	0,2	0,2	0,2	0,2

These would be the differences supposing the wire to lie exactly east and west.

1727. Sept. 15.		Rev. ″	Rev. ″	Rev. ″	Rev. ″	
No aper.	β Drac.	8 4,8 [6]	at exit, 8 1,6	B. & A. 13 1,6	4 30,8	north
2 inch ap.	γ —	entering, 15 18 : :	cross, 15 22,9 [6] going out 15 21,0			
				B. & A. 13 20,7	2 2,2	south
	d —	9 15,0 [8]	-	B. & A. 14 9,7	4 28,7	north
	c —	16 31,1 [8]	exit 27″,2	B. & A. 13 30,9	3 0,2	
	136—	16 13,4 [6]	-	B. & A. 13 30,9	2 16,5	
	ϰ Cygni	14 27,0 [4]	flutt.	B. & A. 13 33,2	0 27,8	
1,4 inch	ι —	19 20,2 [3]	flutt. much : :	13 15,4	6 4,8	error
ap.	θ —	16 20,6 [3]	flutt. B. 13 5,9	A. 13 6,3	3 14,5	
	ψ —	12 23,9 [3]	flutt.	B. & A. 13 20,9	0 31,0	
inch ap.	2 o —	17 32,9 [5]	flutt. B. 13 18,6	A. 13 18,2	4 14,5	
	3 ω —	13 5,3 [8]	-	B. & A. 14 9,8	1 4,5	
	1 f —	12 2,3 [6]	-	B. & A. 12 3,2	0 0,9	north
	g —	22 11,5 [8]	-	18 17,0	3 28,5	
	1π —	19 16,0 [8]	-	18 4,6	1 11,4	
	ζ Cephei	came in at 9h. 27′ 58″; at cross at 30′ 6″; went out at 33′ 14″				
	index	19 0,9 [6]	flutt.	B. & A. 17 5,0	1 29,9	south ° ′
	7 Androm.	12 16,7	-	16 13,2	3 30,5	north 42 5
	τ Cass.	15 29,4 [6]	-	16 1,6	0 6,2	
	σ —	14 23,6	-	16 28,6	2 5,0	north 35 45
	β —	15 22,7 [6]	cloudy	B. & A. 16 32,2	1 9,5	
1,4 ap.	λ —	21 7,3 [8]	-	17 4,3	4 3,0	south 36 55
	α —	19 3,2 [4]	flutt. -	17 16,7	1 20,5	

1727.		Rev. ″			Rev. ″	Rev. ″	
	2 υ Cass.	8 0,0 [9]	-		6 6,0	1 28,0	
	θ —	1 15,8 [6]	-		7 8,0	5 26,2	north 36° 20′
	φ Pers.	4 1,1 [4] : :	-		3 25,1	0 10,0	

Then it grew so cloudy that I could observe no more.

Sept. 16.	β Cass.	2 21,1 [8]	-	B. & A.	3 31,4	1 10,3	
Inch ap.	λ —	7 33,8 [6]	-	B.	3 29,4	4 4,4	
	α —	9 11,0 [8]	-	B.	7 27,0	1 18,0	
	π —	10 28,7 [6]	-	A.	8 0,5	2 28,2	south 44 25
	2 υ —	4 25,9 [8]	-	B. & A.	2 32,9	1 27,0	
	θ —	29 21,4 [8]	-		5 16,2	5 28,8	north
	ξ Andr.	29 33,8 [8]	-		3 20,8	3 21,0	
	φ Pers.	4 10,1 [8]	-		4 0,6	0 9,5	
	4th—	5 33,0 ' -	-		7 21,7	1 22,7	north 36 50
	h —	9 29,4 [4]	-		7 9,2	2 20,2	south 40 10
	65 Andr.	1 24,2 [6]	-		7 10,2	5 20,0	north 41 0
	θ Pers.	9 7,8 [8]	-		8 30,0	0 11,8	
	τ —	11 0,7 [6]	-		9 7,9	1 26,8	
	γ —	7 1,4 [5] flutt.	-		9 16,6	2 15,2	north 37 35

Then clouds.

19. γ Drac. at entrance 14 19,4; at cross 14 22,2; [4] at exit 14 21,1;
2 inch ap. B & A. 12 18,9 2 3,3

The star was quite hid by the wire as it moved along, by reason of thin clouds.

d Drac. going out 7 23,5; - B. 12 20,0 4 30,5
c — at entrance 10 3,6; cross 10 7,9; exit 10 5,5 B. & A. 7 7,2 3 0,7
136— entered 9 19,5; cross 9 24,0; [6] exit 21″,2 A.&B. 7 7,2. Hence
2 16,8

κ Cygni	7 1,1 [6]	-	B. & A.	6 6,8	0 28,3
ι —	2 22,7 [5]	-	B.	5 16,3	2 27,6
θ —	10 16,3 [6]	-	B. & A.	7 1,6	3 14,7
τ Cass.	11 15,8 [8]	-	B. & A.	11 21,4	0 5,6
σ —	8 5,1 [4]	-	B.	10 11,6	2 6,5
β —	8 10,9 [6]	-	B. & A.	9 20,7	1 9,8
λ —	14 23,3 [4]	-		10 19,6	4 3,7
α —	13 6,9 [6]	-		11 22,0	1 18,9
2 υ —	7 7,1 [8]	-		5 13,7	1 27,4
θ —	2 29,1 [6]	-		8 23,1	5 28,0
ξ Andr.	3 29,8 [3]	-		7 20,0	3 24,2
φ Pers.	8 6,3 [6]	-		7 30,3	0 10,0
4th—	5 14,8 [8]	-		7 4,1	1 23,3

I used the two inch aperture in all these observations, the air being a little hazy. The stars generally moved pretty steady this night. After the last observation, it grew so cloudy that I could observe no more stars.

Sept. 20. I rotated the whole telescope, &c. by turning the screw at the top about $\frac{1}{8}$th of a revolution, in order to make the southermost of the rollers bear a little more against the fixed brass arch; the apparent path of the stars having hitherto seemed to be such as to require a little alteration that way, and that wheel not bearing so much on the brass arch about the middle as the other, though it does at both ends.

1727.		Rev. ″			Rev. ″	Rev. ″		° ′
Sept. 22.	γ Urs. Maj.	15 15,2	: : faint	B. & A.	13 33,2	1 16,0	south	34 45
23.	β Drac.	7 4,9	[6]	B. & A.	12 0,1	4 29,2		
2 inch ap.	γ —	13 14,2	[8] steady	B. & A.	11 10,7	2 3,5		
	d —	5 24,3	[8] -		10 18,3	4 28,0		
	c —	15 15,9	[8] -		12 15,9	3 0,0		
	136—	14 33,4	[6]	B. & A.	12 15,9	2 17,5		
	ϰ Cygni	13 27,0	[4] flutt. -		12 33,2	0 27,8		
Inch ap.	θ —	14 30,0	[6] -		11 16,0	3 14,0		
	ψ —	12 5,2	[6] -		13 3,2	0·32,0		
	2 o —	17 19,0	[6] -		13 4,9	4 14,1		
	3 ω —	11 9,6	[6] -		12 15,1	1 5,5		
	1 f —	12 9,8	[6] -		12 10,0	0 0,2	north	
	g —	27 33,2	[6] -		24 4,4	3 28,8		
	1 π —	15 26,4	[6] -		14 16,2	1 10,2		
	μ Cephei	11 29,3	[7] -		14 33,8	3 4,5	north	34 40
	ζ ——	18 8,0	[3] flutt. -		16 13,5	1 28,5		
	Capella	20 28,0	[6] flutt. much		16 5,7	4 22,3		
Inch ap.	18th Camel.	16 14,7	[3] : : -		17 13,3	0 32,6	north	33 0
				forsan	26	forsan 0 18,9		
Error.	δ Aurig.	27 3,2	[3] flutt. -	B.	29 19,2	2 16,0	north	35 45
	35th Camel.	22 14,4	: : too dark		20 7,1	2 7,3	south	38 25
	46 Aurig.	20 28,2	[6] -	B. & A.	20 4,9	0 23,3	south	40 35
2 inch ap.	13 Lyncis	19 10,6	[5] twilight	B.	20 33,4	1 22,8	north	32 35
24.	γ Urs. M.	1 20,7	[6] -	B. & A.	0 3,0	1 17,7	south	34 45
No aper.	ε ——	12 5,6	[6] very plain		9 7,8	2 31,8	south	32 30
	ζ ——	12 22,1	[6] pretty bright		9 7,0	3 15,1	south	33 35
	η ——	13 32,2	[6] bright, a calm day		9 20,8	4 11,4	south	39 15
2 inch ap.	β Drac.	7 5,2	[8]	B. & A.	11 33,9	4 28,7		
	γ —	13 25,0	[4] -		11 21,4	2 3,6		
25.	η Urs. M.	22 5,0	[4] faint	A.	17 29,0	4 10,0		

		Rev. ″			Rev. ″	Rev. ″	
1727.	γ Drac.	14 28,1 [6]	- -	A.	12 24,5 : :	2 3,6	
Sept. 27.	γ —	12 3,4 [9]	-	B. & A.	9 33,6 [8]	2 3,8	

I judge this to be as exact as I can possibly observe, the star moving very steady and appearing the least that can be imagined on both sides the thread.

2 inch ap.	c Drac.	10 31,9 [6]	-	B. & A.	7 31,9 [6]	3 0,0	south
	136 —	10 13,7 [6]	-	B. & A.	7 31,9	2 15,8	
	ϰ Cygni	7 29,2 [6]	flutt.	B. & A.	7 1,6	0 27,6	
	θ —	11 09,2 [8]	- -		7 29,6	3 13,6	
	ψ —	7 12,8 [6]	- -		8 11,8	0 33,0	
	2 ο —	12 23,9 [5]	flutt. -		8 10,9	4 13,0	
2 inch ap.	3 ω —	6 24,6 [5]	flutt. -		7 31,9	1 7,3	
1,4 inch ap.	1 f —	7 18,7 [3]	flutt. very much		7 19,7	0 1,0	north
	g —	23 33,7 [6]	- -		20 4,8	3 28,9	
	1 π —	23 17,0 [6]	- -		22 7,0	1 10,0	
	ζ Ceph.	26 9,2 [7]	- -		24 17,0	1 26,2	
	τ Cass.	23 18,0 [3]	flutt. -		23 27,2	9,2	
	β —	7 23,3 [6]	- -	A.	9 1,0 [3]	1 11,7	
	Capella	16 8,5 [5]	flutt.	B. & A.	11 20,2	4 22,3	
	18 Camel.	13 12,4 [3]	too faint		14 11,4	0 33,0	
	δ Aur.	23 12,3 [6]	- -	B.	22 29,3	0 17,0	
	35 Camel.	26 6,1 : :	very dub.	B.	23 30,7	2 9,4	
	46 Aur.	26 19,1 [8]	-	B. & A.	25 29,6	0 23,5	
	13 Lync.	25 17,9 [8]	- -		27 8,0	1 24,1	north 32° 35′
No ap. 29.	γ Drac.	14 11,0 ½ in.	14 11,3 ½ out.	A.	12 8,2	2 3,0	+0,8
1,4 aper.	ϰ Cygni	13 23,2 [8]	B. 12 28,9	A.	12 29,4	0 28,0	
1,4 aper.	β Cass.	9 14,8 [8]	-	B. & A.	10 28,2	1 13,4	
Octob. 8.	γ Drac.	15 13,9 [8]	-	B. & A.	13 9,4	2 4,5	
1,4 aper.	ϰ Cygni	17 5,8 [6]	- -	A.	16 12,0	0 27,8	
	θ —	19 5,0 [5]	-	B. & A.	15 24,8	3 14,2	
	ψ —	13 29,2 [5]	- -		14 27,7	0 32,5	
	2 ο —	20 0,8 [5]	- -		15 20,2	4 14,6	
	3 ω —	14 29,7 [6]	- -		16 1,7	1 6,0	
	1 π —	17 7,2 [6]	- -		15 33,7	1 7,5	
	ζ Ceph.	18 8,4 [8]	- -		16 17,9	1 29,5	
	τ Cass.	16 14,0 [4]	- -		16 25,8	0 11,8	
	β —	10 12,3 [8]	- -		11 28,0	1 15,7	
1,4 ap.	α —	13 2,6 [9]	- -		11 23,8	1 12,8	
Oct. 10.	ϰ Cygni	13 12,3 [8]	B. 12 17,8	A.	12 18,4	0 28,0	
	θ —	16 25,7 [8]	B. & A. twilight		13 11,2	3 14,5	

1727.		Rev. //			Rev. //	Rev. //
2 inch ap.	ψ Cygni	12 32,6 [7]	illum.	-	13 30,3	0 31,7
	2 ο —	18 32,2 [8]	-	-	14 16,8	4 15,4
	3 ω —	13 6,4 [7]	-	-	14 12,7	1 6,3
	1 π —	15 8,9 [8]	-	-	14 1,0	1 7,9
	ζ Ceph.	14 0,7 [8]	-	-	12 10,7	1 24,0
Oct. 12.	α Cass.	10 21,3 [6]	-	- A.	9 9,8	1 11,5
2 inch ap.	2 υ —	10 28,5 [9]	steady	B. & A.	9 9,1	1 19,4
13.	ζ Urs. m.	6 5,9 [8]	bright	B. 2 17,9 A.	2 19,9	3 20,0

Manè. In this observation there was the greatest difference between the plummet before and after the observation that I have yet met with; but I believe it must be owing to some cobweb or spiders, &c. for just after the observation, I found the index, when the spot was rectified, at 2 18,3; I then lifted up the water and plummet, and setting it down again, I found index at 2 19,9; and removing the water, &c. again, index was 2 20,1; so that I conclude that the true rectification for this observation should be 2 19,9; and that the star is 3 20,0 from the spot.

The star appeared very bright and plain to be seen, otherwise I should have missed it, because I looked for it two minutes before it came to the cross; its right asc. in Mr. Flamsteed's Catalogue being set 30′ of a degree wrong, but 'tis right in Dr. Halley's edition.

η. Urs. M. 8 32,6 [5] - B. & A. 4 16,6 4 16,0

After I had made these observations, I brushed down the dust, &c. all about the telescope, and dropped some paper down about the wire.

γ Drac. 7 4,4 [6] ½ out +, B. & A. 4 33,5 2 5,9

Clouds hindered me from bisecting the star at the cross, but I did it when 'twas a little more than half-way out; so that I suppose the star at the cross 1″ more south than the index.

		Rev. //			Rev. //	Rev. //
	κ Cygni	8 15,7 [8]		B. & A.	7 21,8	0 27,9
2 inch ap.	θ —	11 19,7 [6]	flutt.	-	8 5,8	3 13,9
	ψ —	8 18,1 [6]	flutt.	-	9 17,4	0 33,3
	2 ο —	13 27,0 [5]	flutt. much		9 12,5	4 14,5
	3 ω —	8 6,5 [8]	steady		9 13,8	1 7,3
	1 π —	10 8,8 [6]	steady		9 2,2	1 6,6
2 inch ap.	ζ Ceph.	13 17,0 [5]	flutt. much		11 28,0	1 23,0
Inch ap.	τ Cass.	12 20,8 [8]		-	13 0,8	0 14,0
	β —	10 22,0 [5]	flutt. much		12 5,7	1 17,7
	α —	14 6,2 [3]	flutt. very much		12 29,2	1 11,0

1727.		Rev. ″			Rev. ″	Rev. ″
	θ Cass.	6 23,5 [8]	-		12 26,8	6 3,3
	ξ Andr.	9 3,0 [8]	-		12 31,2	3 28,2
	φ Pers.	13 1,5 [8]	-		12 33,5	0 2,0 south
	θ —	13 0,3 [8]	-		12 29,3	0 5,0
	τ —	15 8,2 [8]	-		13 22,3	1 19,9
	γ —	11 11,2 [4]	flutt. much		13 33,3	2 22,1
	α —	16 18,6 [4]	flutt. much		13 27,2	2 25,4
	δ —	14 0,2 [4]	flutt.		13 21,2	0 13,0
	9th Aurig.	16 0,5 [6]	-		12 33,0	3 1,5
	Capella	17 17,2 [6]	flutt.		12 32,2	4 19,0
	18 Camelop.	14 4,2 [4]	too dull		15 6,6	1 02,4
	35th ——	27 5,0 [8]	-		24 32,7	2 6,3
Oct. 16.	γ Urs. Maj.	16 29,9 [6]	-	B. & A.	15 5,6	1 24,3
Dropped	ε ——	19 12,1 [9]	very bright		16 8,9	3 3,2
paper.	ζ ——	22 19,8 [8]	bright		18 32,3	3 21,5
	η ——	16 28,8 [8]	very bright		12 11,1	4 17,7
17.	ε ——	16 21,4 [8]	very bright		13 16,6	3 4,8
	ζ ——	18 27,8 [8]	-		15 5,4	3 22,4
	η ——	19 33,5 [8]	-		15 14,5	4 19,0
	β Drac.	11 2,9	⅓ from the cross		15 29,9	4 26,5
20.	ε Urs. Maj.	15 7,9	½ out -	A.	12 3,0	3 6,0
	ζ ——	15 26,4 [4]	clouds	B. & A.	12 4,1	3 22,3
	η ——	16 8,5 ::	clouds	B. 11 21,6 A.	11 22,7	4 19,8

In this observation there was a difference of about a second in the rectifying the instrument before and after the observation, which I imagine was occasioned by a small hair, or somewhat that stuck to the wire at the brass edge, which I saw, but did not wipe it off till I took up the plummet, in order to lie down on the couch; so that I judge the rectification after the observation was the truest.

γ Drac. entering 7 24,7; at cross 7 27,7 [10]; going out 7 25,1;
B. & A. 5 20,5. Hence star 2 7,2.

This I take to be a very good and exact observation.

If we make use of the two numbers at coming in and going out, they will make the star about ½ a second more south than the middle observation, or 2 7,7 south of the mark.

	Rev. ″			Rev. ″	Rev. ″
κ Cygni	4 26,9 [8]		B. & A.	3 31,7	0 29,2
θ —	6 9,3 [8]	-		2 29,4	3 13,9
ψ —	3 12,9 [8]	3 10,1 ent.		4 11,3	0 32,4

1727.		Rev. "		Rev. "	Rev. "	
1,4 ap.	2 ο Cygni	8. 4,7	flutt. much	3 22,2	4 16,5	
	ζ Ceph.	14 20,4	flutt. much	12 33,0	1 21,4	
Inch ap.	β Cass.	10 23,5	[6] flutt.	12 8,8	1 19,3	
		14			1	
	α —	19 9,1	[4] flutt. error	13 0,5	6 8,6	
	θ —	8 8,4	[7] -	14 12,0	6 3,6	
	ξ And.	10 0,7	-	13 32,3	3 31,6	
	φ Pers.	14 11,0	½ in, & 14 11,3 ½ out	14 11,1	0 0,8	south
	θ —	13 3,8	[6] flutt. -	12 33,4	0 4,4	
	τ —	14 1,4	[6] -	12 15,8	1 19,6	
	γ —	9 5,6	[3] flutt. much	11 29,6	2 24,0	
	α —	14 17,8	[6] flutt.	11 28,1	2 23,7	
	δ —	11 31,8	flutt. much	11 20,1	0 11,7	
	Capella	16 12,5	[6] flutt. much	11 27,9	4 18,6	
	18th Camel.	10 19,9	too dull B. & A.	11 24,3	1 4,4	
2 inch ap.	35 ——	10 13,7	[8] -	8 7,3	2 6,4	
	46 Aurig.	9 17,1	[3] flutt. much	8 29,6	0 21,5	
Oct. 21.	γ Drac.	12 3,0	[3] very dull, hazy	9 28,6	2 8,4	
	κ Cygni	10 26,4	[6] faint -	9 30,9	0 29,5	
	θ —	12 32,2	[6] hazy -	9 17,2	3 15,0	
	ψ —	9 5,8	[8] twilight	10 4,3	0 32,5	
	2 ο —	14 6,5	[6] illuminated	9 26,0	4 14,5	
22.	β Urs. Maj.	29 11,3	[6] faint -	11 1,8	11 24,5	
	γ ———	13 3,5	very faint	11 11,0	1 26,5	
	ε ———	14 8,3	⅓ out -	11 3,5	3 5,3	
23.	1 π Cygni	12 24,0	[8] steady B. & A.	11 18,2	1 5,8	inch ap.
Inch ap.	ζ Ceph.	12 19,6	[8] steady	10 32,5	1 21,1	
	7th Andr.	7 16,8	[3] faint -	11 23,5	4 6,7	
	τ Cass.	11 9,7	[6] -	11 26,1	0 16,4	
	σ —	9 1,9	-	11 17,4	2 15,5	
	β —	10 23,5	flutt. exceedingly	12 9,8	1 20,3	
			foggy weather.			
	α —	15 21,6	flutt. much	14 13,6	1 8,0	
26.	ζ Urs. Maj.	13 19,5	[6] -	9 28,2	3 25,3	
	η ———	14 6,1	[3] cloudy	9 19,4	4 20,7	
2 inch ap.	1 f Cygni	8 8,4	[3] cloudy	8 9,8	0 1,4	north
	g —	23 13,3	[8] -	19 19,8	3 27,5	
	1 π —	20 17,7	[7] -	19 11,5	1 6,2	
	μ Cephei	9 29,4	[3] cloudy	13 4,9	3 9,5	
	ζ ——	14 24,9	[8] very windy night	13 3,0	1 21,9	
2 inch ap.	β Cass.	8 10,2	[8] steady	9 31,2	1 21,0	
	α —	12 15,7	[8] ¾ in, st., clouds	11 10,5	1 5,5	

1727. Oct. 27.		Rev. ″		Rev. ″	Rev. ″
Manè.	27th Lyncis	8 6,1 [8]	⅓ from cross, steady	11 16,6	3 10,5 north
1,4 ap.	35th ——	7 24,8 [6]	cloudy	11 8,2	3 17,4 north
	ι Urs. Maj.	7 33,0 [8]	-	10 20,3	2 21,3 north
	f ——	7 6,4 [8]	-	10 30,4	3 24,0 north
	θ ——	7 16,8 [8]	ent. 7 12,7	7 32,4	0 15,6 north
	φ ——	7 32,5 [8]	-	8 25,5	0 27,0 north
No ap.	36th ——	12 12,5 [3]	faint -	9 11,6	3 0,9 south
	β ——	29 19,1 [8]	very good	11 7,6	11 22,5
	ψ ——	12 2,9 [8]	-	11 6,2	0 30,7
	γ ——	12 33,5 [8]	-	11 6,0	1 27,5
	ε ——	14 21,5 [7]	-	11 14,0	3 7,5
	ζ ——	15 6,2 [8]	-	11 14,1	3 26,1
	η ——	15 1,7 [9]	-	10 13,7	4 22,0

I went to Oxford.

Nov. 12.	λ Cass.	16 12,1 [8]	steady B. & A.	12 23,2	3 22,9
1,4 ap.	α —	14 6,3 [8]	steady	13 2,3	1 4,0
	θ —	7 3,1 [8]	flutt. -	13 12,5	6 9,4
	ξ Andr.	10 4,1 [8]	-	14 3,7	3 33,6
	φ Pers.	13 31,1 [8]	-	14 0,3	0 3,2 north
13.	ε Urs. Maj.	17 1,0 [10]	-	13 20,2	3 14,8
	ζ ——	18 1,2 [8]	-	14 3,9	3 31,3

N. B. I this day perceived that this is a double star; that which I observe having a smaller contiguous to it, being about 8″ or 10″ from it, north of it, but rather following. The smallest must, I suppose, be of the 4th or 5th magnitude, otherwise I should not have seen it, it being near ¼ after nine when they passed.

	η Urs. M.	17 20,8 [8]	-	12 28,3	4 26,5
14.	ε ——	15 9,9 [7]	-	11 30,9	3 13,0
	ζ ——	16 4,8 [8]	-	12 6,8	3 32,0
	η ——	17 15,8 [8]	-	12 22,3	4 27,5
	β Drac.	8 18,4 [8]	-	13 3,0	4 18,6
	γ —	15 17,5 [8]	-	13 4,0	2 13,5
Inch ap.	ζ Ceph.	16 20,1 [8]	steady	15 2,3	1 17,8
	3 Lacert.	17 7,0 [8]	-	16 0,8	1 6,2
	τ Cass.	9 14,7 [8]	-	10 1,0	0 20,3
	β —	9 8,0 [8]	-	10 31,8	1 23,8
	λ —	15 5,3 [8]	-	11 17,7	3 21,6
	α —	12 15,8 [8]	-	11 12,3	1 3,5
	2υ —	6 13,1 [8]	B. & A.	5 2,2	1 10,9

1727.		Rev. ″		Rev. ″	Rev. ″
	θ Cass.	28 13,6 [7]	extreme	4 23,1	6 9,6
	ξ Andr.	5 25,4 [8]	-	9 26,9	4 1,5
	τ Pers.	12 7,6 [8]	-	A. 10 29,8	1 11,8
	γ Pers.	8 33,7 [8]	-	11 29,7	2 30,0
	α —	14 3,2 [8]	-	11 17,7	2 19,5
	δ —	12 19,1 [8]	-	12 11,7	0 7,4
	Capella	15 23,0 [6]	flutt.	11 6,7	4 16,3
Nov. 15.	ε Urs. M.	12 15,3 [8]	-	9 2,3	3 13,0
	ζ ——	14 18,0 [8]	-	10 19,5	3 32,5
	η ——	14 27,5 [8]	-	9 33,8	4 27,7
16.	9th Aurig.	12 16,1 [8]	-	9 19,1	2 31,0
	Capella	13 21,7 [9]	-	9 3,8	4 17,9
Manè, 17.	β Urs. Maj.	0 8,4 [8]	-	11 25,9	11 17,5
	ψ ———	12 15,8 [8]	-	11 13,3	1 2,5
	γ ———	15 5,3 [10]	-	13 4,6	2 0,7
	ε ———	15 8,1 [9]	-	11 27,4	3 14,7
	ζ ———	16 9,0 [3]	faint	12 10,3	3 32,7
	η ———	14 27,4 [8]	-	9 33,1	4 28,3
	β Drac.	6 18,2 [3]	faint	11 2,0	4 17,8
	γ —	13 3,8 [8]	-	10 23,3	2 14,5
22.	τ Cass.	12 29,4 [8]	-	13 15,9	0 20,5
Inch ap.	β —	10 21,7 [9]	-	12 13,7	1 26,0
	λ —	16 1,7 [8]	-	12 14,4	3 21,3
	α —	14 13,0 [8]	-	13 10,5	1 2,5
	ξ Andr.	8 27,3 [6]	-	12 27,2	4 0,0
	φ Pers.	12 29,7 [8]	-	13 0,0	0 4,3
	θ —	12 28,4 [8]	-	12 30,1	0 1,7
	τ —	14 4,3 [3]	flutt. much	12 28,3	1 10,0
	γ —	9 7,3 [3]	flutt. much	12 6,3	2 33,0
	α —	14 26,8 [3]	flutt. much	12 8,8	2 18,0
	δ —	10 7,5 : :	flutt. exceedingly	10 3,5	0 4,0
	9th Aurig.	11 11,3 [6]	-	8 16,3	2 29,0
	Capella	15 14,0 [6]	flutt. -	11 0,5	4 13,5
	18 Camel.	10 5,7 [3]	too faint	11 15,0	1 9,3
2 inch ap.	35 ——	12 32,1 [6]	-	10 30,3	2 1,8
	46 Aurig.	11 10,6 [6]	-	10 26,2	0 18,4
	13 Lyncis	8 31,8	going out	10 29,6	1 28,0
	ι Urs. M.	6 1,0 [7]	-	8 20,3	2 19,3
	f ——	5 11,0 [9]	-	8 33,2	3 22,2
Inch ap.	θ ——	9 26,5 [8]	-	10 4,5	0 12,0

1727.		Rev. //		Rev. //	Rev. //
	β Urs. M.	29 29,8	[3] flutt. much	11 12,0	11 16,2
	ψ ———	12 26,2	[4] flutt. much	11 21,5	1 4,7
Nov. 23.	γ Drac.	9 31,2	: : very faint, hazy	7 12,7	2 18,5
Dec. 5.	β —	2 26,1	[6] very plain	A. 7 3,3	4 11,2
	γ —	9 28,3	[10] B. 7 7,4	A. 7 8,2	2 20,5

This day the air was very clear, and the stars appeared very distinct and bright, and moved steady; γ Drac. was so bright as plainly to be seen several times (at once) on both sides of the wire. The wind blowed pretty briskly from the north-east, and we had constant moist and rainy weather for a week or ten days before this day; so that I imagine that the difference of the adjusting the mark before and after was owing to the alteration in the chimneys; for about 15′ before the observation, index stood at 7 6,7; and a quarter of an hour after the observation, at near 9 0,0; so that in 30′ it varied near 1′ 1″½.

N. B. I had dropped down paper both before and after the observation, so that this could not be owing to cobwebs, as is evident likewise from the gradual alteration, it continually increasing all the time I observed it.

		Rev. //		Rev. //	Rev. //
Inch ap.	ζ Ceph.	11 7,7	[6] -	A. 9 20,7	1 21,0
	τ Cass.	10 14,8	[8] -	B. & A. 11 2,8	0 22,0
	β —	10 27,2	[6] flutt. -	12 18,8	1 25,6
	λ —	16 19,2	[6] -	B. 12 32,8	3 20,4
	α —	18 17,0	-	B. 17 14,8	1 2,2
	π —	20 31,3	[6] -	A. 18 16,9	2 14,4
	θ —	9 6,5	[5] flutt. -	15 18,5	6 12,0
	ξ Andr.	10 31,2	[8] -	15 1,4	4 4,2
	φ Pers.	14 23,6	[5] flutt. -	14 30,9	0 7,3
	Capella	18 29,8	[7] steady	14 16,9	4 12,9
6 Manè,	β Urs. M.	29 21,0	[3] flutt. much	11 2,3	11 15,3
	ψ ——	10 20,7	[5] flutt. much	9 15,9	1 4,8
	γ ——	12 15,7	[6] flutt. -	10 11,2	2 4,5
	ε ——	16 11,5	::: flutt. exceedingly	12 21,5	3 24,0

I scarce ever saw any star flutter so much as this did. I looked out, but could not perceive any thing more than ordinary in the air; but it soon after grew very foggy.

	ζ Urs. M.	16 22,5	[8] steady, twilight	12 16,5	4 6,0
Inch ap.	η ——	16 12,1	: : flutt. very much	11 12,3	4 33,8
6.	β Drac.	6 0,5	[4] faint -	10 11,0	4 10,5

1727.		Rev. ″		Rev. ″	Rev. ″
	γ Drac.	12 21,0 [8]	B. 10 0,4 [3]	A. 10 1,0 [3]	2 20,3
	7th Andr.	6 6,3 [8]	-	10 13,0	4 6,7
	τ Cass.	10 5,1 [8]	-	10 26,3	0 21,2
	β —	9 23,5 [8]	-	11 16,3	1 26,8
Dec. 9.	Capella	15 14,4 [8]	steady	11 0,6	4 13,8

December 12. I found that the wall at top was fallen so much back from the instrument, that 'twas necessary to bring the top work of the telescope, &c. farther from the wall, which I did by turning the screw that moves it east and west just one revolution, which was more than sufficient to make the plummet hang at a proper distance from the graduated edge.

12.	ζ Ceph.	13 20,0 [10]	-	11 33,0	1 21,0
	3 Lacert.	13 33,5 [10]	-	12 25,8	1 7,7
	τ Cass.	13 14,0 [8]	-	14 1,8	0 21,8
Inch ap.	β —	9 23,9 [10]	steady	11 16,2	1 26,3
	λ —	14 16,4 [8]	-	10 30,1	3 20,3
	α —	10 6,7 [9]	-	9 6,0	1 0,7
	π —	11 3,6 [8]	-	A. 8 22,6	2 15,0
	2 υ —	8 15,5 [8]	-	7 8,8	1 6,7
	θ —	2 14,6 [8]	-	8 27,8	6 13,2
	ξ Androm.	2 29,9 [8]	-	6 30,9	4 1,0
	φ Pers.	6 20,0 [7]	-	6 27,0	0 7,0
	θ —	8 14,5 [8]	-	8 18,0	0 3,5
	τ —	10 31,6 [8]	-	9 23,6	1 8,0
	γ —	8 0,8 [8]	-	11 1,1	3 0,3
	α —	12 33,3 [10]	steady	10 18,3	2 15,0
	δ —	10 22,3 [8]	-	10 19,0	0 3,3
	Capella	14 29,2 [8]	steady, pretty	10 15,4	4 13,8
2 inch ap.	35th Camel.	12 32,2 [9]	-	10 33,0	1 33,2
	46 Aurig.	11 24,4 [7]	-	11 8,2	0 16,2
13 Manè,	β Urs. Maj.	0 19,2 [3]	-	12 2,2	11 17,0
	ψ ——	10 27,1 [8]	-	9 18,7	1 8,4
	γ ——	11 23,9 [8]	-	9 19,9	2 4,0
	ε ——	12 32,3 [8]	-	9 11,8	3 20,5
	ζ ——	15 14,8 [8]	-	11 9,8	4 5,0
	η ——	17 30,8 [8]	-	12 27,8	5 3,0
	β Draconis	8 3,7 [7]	-	12 10,9	4 7,2
	β Cass.	9 29,1 [8]	-	11 21,8	1 26,7
	λ —	14 29,0 [8]	-	11 8,8	3 20,2
	α —	12 1,5 [6]	flutt. -	11 1,3	1 0,2

As I went to set the telescope to observe π Cass, I some how or other touched the plummet and broke the wire: it broke this time just at the notch at the top. I did not fix on another till after ten of the clock, just soon enough to observe Capella, as follows:

		Rev. ″	Rev. ″	Rev. ″
1727.	Capella	15 31,1 [7] flutt. -	11 19,6	4 11,5
2 inch ap.	18th Camel.	9 8,0 : : : -	10 21,0	1 13,0
	35th ——	12 5,8 [8] faint -	10 7,8	1 32,0
	46th Aurig.	10 13,6 [6] flutt. -	9 32,4	0 15,2
Dec. 14.	β Urs. Maj.	0 22,7 [4] flutt. -	12 4,5	11 15,8
Manè.	ψ ——	11 13,6 [7] -	10 6,9	1 6,7
	γ ——	12 20,9 [7] flutt. -	10 15,9	2 5,0
	ε ——	13 30,1 [3] flutt. -	10 7,8	3 22,3
	ζ ——	15 5,2 [6] flutt. -	10 32,7	4 6,5
	η ——	14 29,9 [6] flutt. -	9 27,4	5 2,5
	β Cass.	7 19,6 ½ out -	9 12,8	1 26,2
	λ —	12 33,9 [8] -	9 13,5	3 20,4
	α —	11 5,0 [6] -	10 4,2	1 0,2
	π —	11 32,4 [5] flutt. -	9 16,9	2 15,5
15.	γ Drac.	12 31,1 [6] faint -	10 7,3	2 23,8

Cold snowy weather. By these observations, 'tis evident that there is no sensible difference in rectifying the instrument with the present wire and the former that was broke; for the observations made on the 13th and 14th days differ some one way and some another, whereas did that arise from the plumbline, the difference ought to have been all the same way.

21.	τ Cass.	12 1,9 [5] -	12 23,3	0 21,4
Inch ap.	β —	11 5,4 [8] steady	12 31,6	1 26,2
	λ —	16 26,2 [7] -	13 4,8	3 21,4
	α —	13 3,4 [4] flutt. -	B. 12 2,4	1 1,0
	π —	14 20,3 [8] -	A. 12 5,6	2 14,7
	θ —	5 15,6 [6] flutt. -	11 27,9	6 12,3
	ξ Androm.	7 27,6 [6] -	11 29,6	4 2,0
	φ Pers.	11 22,0 [8] -	B. 11 30,0	0 8,0
	τ —	14 11,0 [8] -	A. 13 3,6	1 7,4
	γ —	10 6,0 [4] flutt. much	13 7,5	3 1,5
	α —	15 3,0 [4] flutt. much	12 24,0	2 13,0
	Capella	19 24,4 [8] steady	15 12,0	4 12,4
	18th Camel.	14 8,9 [8] -	15 25,2	1 16,3
2 inch ap.	35th ——	17 21,5 [8] -	15 24,4	1 31,1
27.	γ Drac.	18 16,3 [8] faint -	15 21,3	2 29,0

1727.		Rev. ″		Rev. ″	Rev. ″
Dec. 28.	β Drac.	11 26,6 [8]	-	15 29,3	4 2,7
	γ —	17 30,5 [8]	bright	15 2,3	2 28,2
29.	β Urs. Maj.	0 28,0 [8]	steady	12 7,8	11 13,8
Manè.	ψ ——	11 10,6 [8]	-	10 2,1	1 8,5
	χ ——	12 21,3 [8]	-	9 27,8	2 27,5
	γ ——	11 7,4 [9]	-	9 0,4	2 7,0
Stars moved steady.	3 Can. Venat.	6 7,5 [9]	windy weather	8 1,5	1 28,0
	ε Urs. Maj.	11 9,8 [9]	-	7 17,8	3 26,0
	21st Can. Ven.	9 13,7 [8]	-	6 24,9	2 22,8
	ζ Urs. M.	15 26,4 [9]	-	11 15,0	4 11,4
	η ——	16 26,7 [9]	-	11 19,5	5 7,2
29.	β Drac.	7 32,5 [7]	-	12 0,8	4 2,3
	γ —	15 3,2 [8]	-	12 8,5	2 28,7
No ap.	β Cassiop.	10 16,3 [9]	steady, twilight	12 7,3	1 25,0
	λ ——	15 15,8 [7]	B. 11 27,6	A. 11 28,4	3 21,8
	α ——	13 6,2 [8]	-	B. 12 5,2	1 1,0
	π ——	14 5,2 [8]	-	A. 11 23,5	2 15,7
	2 υ ——	11 13,3 [8]	-	10 6,3	1 7,0
	θ ——	3 17,5 [9]	-	9 30,5	6 13,0
	ξ Andr.	4 32,1 [8]	-	8 32,9	4 0,8
	φ Pers.	10 32,8 [9]	-	11 6,1	0 7,3
	θ —	10 25,6 [8]	-	10 31,2	0 5,6
	τ —	12 7,0 [8]	-	11 0,9	1 6,1
	γ —	7 15,9 [10]	-	10 18,7	3 2,8
	α —	12 18,9 [10]	-	10 6,6	2 12,3
	δ —	9 21,1 [9]	-	9 20,1	0 1,0

The wind blowed pretty strong from the north-west, and the stars moved very steady all this night, so that I judge these observations good, they having been made with all the care I could take.

1728.					
Jan. 3.	β Cass.	8 4,0 [8]	-	9 28,0	1 24,0
	λ —	13 21,3 [9]	-	9 33,0	3 22,3
	α —	11 3,4 [7]	flutt. -	B. 10 1,4	1 2,0
	π —	13 31,5 [8]	-	A. 11 15,8	2 15,7
	2 υ —	12 1,7 [8]	-	10 28,0	1 7,7
	θ —	4 11,4 [8]	-	10 23,9	6 12,5
	ξ Andr.	6 17,1 [8]	-	10 20,1	4 3,0
	φ Pers.	10 0,0 [8]	-	10 8,0	0 8,0
	θ —	9 26,4 [8]	-	9 31,7	0 5,3
	τ —	10 28,0 [8]	flutt. -	9 20,4	1 7,6

		Rev. //		Rev. //	Rev. //
1728.	γ Pers.	6 24,5 [8]	flutt. -	9 28,5	3 4,0
Jan. 6.	ι Urs. M.	6 8,5 [9]	-	8 30,5	2 22,0
or 7.	f ——	5 15,3 [9]	-	9 5,8	3 24,5
Manè.	θ ——	8 33,4 [8]	-	9 12,7	0 13,3
	β ——	0 0,4 [9]	-	11 14,4	11 14,0
	ψ ——	11 26,1 [8]	-	10 17,5	1 8,6
	χ ——	12 27,5 [8]	-	9 33,4	2 28,1
	γ ——	13 1,2 [9]	-	10 28,5	2 6,7
	3 Can. Ven.	8 15,6 [8]	-	10 9,6	1 28,0
	ε Urs. Maj.	14 11,6 [10]	-	10 19,6	3 26,0

Then it grew cloudy. The wind blowed briskly from the west, and the stars moved all very steady, so that I take this night's observations to be very exact.

12.	Capella	11 32,2 [8]	steady	7 22,9	4 9,3
	18th Camel.	5 18,5 [3]	cloudy	7 4,5	1 20,0
	δ Aurig.	6 27,5 [8]	-	6 25,5	0 2,0
	35th Camel.	8 2,9 [6]	-	6 9,4	1 27,5
	46 Aurig.	6 25,0 [4]	flutt. -	6 12,8	0 12,2
	13 Lyncis	5 4,6 [6]	-	7 9,6	2 5,0
16.	β Draconis	2 27,9 [11]	-	6 23,7	3 29,8
	γ ———	9 5,7 [10]	-	6 4,2	3 1,5
	β Cass.	5 1,8 [8]	faint -	6 23,2	1 21,4
	α —	11 16,0 [8]	-	10 12,0	1 4,0
	θ —	3 24,2 [7]	hazy -	10 0,4	6 10,2
	ξ Andr.	4 17,8 [8]	-	8 19,3	4 01,5
	φ Pers.	9 28,9 [8]	-	10 1,2	0 6,3
	θ —	10 0,3 [8]	-	10 5,3	0 5,0
	τ —	10 29,5 [8]	-	9 23,3	1 6,2
	γ —	6 13,8 [8]	-	9 17,3	3 3,5
	α —	11 1,0 [8]	-	8 23,0	2 12,0
	δ —	8 9,5 [8]	-	8 9,0	0 0,5
	9th Aurig.	11 16,3 [8]	-	8 30,3	2 20,0
	Capella	12 7,0 [8]	steady	7 32,7	4 8,3
	18 Camel.	6 14,1 [3]	: too faint	8 1,1	1 21,0
	δ Aurig.	6 6,5 [8]	-	6 5,0	0 1,5
	35 Camel.	7 21,8 [8]	-	5 29,3	1 26,5
	46 Aurig.	5 29,5 [8]	-	5 18,2	0 11,3
	13 Lyncis	3 23,3 [6]	flutt. -	5 29,3	2 6,0
	ι Urs. Maj.	2 29,4 [7]	-	5 18,1	2 22,7

		Rev. //		Rev. //	Rev. //
1728.	f Urs. Maj.	2 9,6 [8]	-	6 2,3	3 26,7
	θ ———	5 0,5 [6]	flutt. -	5 16,2	0 15,7
	β ———	0 33,2 [6]	flutt. -	12 14,2	11 15,0
	ψ ———	13 2,5 [6]	flutt. -	11 29,5	1 7,0
	χ ———	13 25,8 [6]	flutt. -	10 33,3	2 26,5
	γ ———	13 27,0 [7]	flutt. then clouds	11 19,0	2 8,0
Jan. 20.	β Cass.	8 24,8 [6]	-	10 11,8	1 21,0
	α —	11 16,4 [8]	-	10 12,3	1 4,1
	ξ Andr.	7 9,2 ::	too faint	11 12,5	4 3,3
	φ Pers.	9 29,1 [8]	-	10 2,3	0 7,2
	θ —	9 26,4 [8]	-	9 33,7	0 7,3
	τ —	10 23,9 [3]	flutt. much	9 18,7	1 5,2
	γ —	5 33,3 [6]	flutt. -	9 4,0	3 4,7
23.	β Drac.	4 31,0 [6]	hazy -	8 26,0	3 29,0
	γ —	11 16,8 ::	very hazy	8 14,3	3 2,5
	46 Aurig.	8 16,7 [3] :	-	A. 8 10,5	0 6,2
	ι Urs. Maj.	3 3,1 [6]	flutt. -	5 30,2	2 27,1
	f ———	2 6,5 [6]	-	6 2,2	3 29,7
	ζ ———	10 22,5 [3]	going out	6 13,3	4 11,5
	η ———	11 14,3 [6]	flutt. much	6 6,3	5 8,0

The stars fluttered very much all this night, and when I first began to observe, they appeared in a manner different from what I had ever took notice of before; for as they passed through the glass they seemed very confused and faint, as if it had been hazy, or the object-glass had been foul; only at intervals they would just flash out with their usual lustre, but for the general they were so dull and faint that I could not observe stars of the 6th or 5th magnitude with any certainty. At the same time the sky seemed very clear to the naked eye, but I observed that the stars did not twinkle as usual in a clear air, but rather as they look in hazy weather. The air was pretty cool, and in the morning there was a pretty hard black frost.

24.	γ Drac.	11 5,7 [8]	-	A. 8 2,8	3 2,9
	9th Aurig.	10 27,0 [6]	-	8 8,0	2 19,0
	Capella	11 23,7 [8]	-	7 16,5	4 7,2
	18th Camel.	5 27,3 [6]	-	7 15,3	1 22,0

Memorand. This observation was made with the inch aperture, but this star appears too faint; so that 'twill be better for the future to use a larger aperture.

		Rev. ″			Rev. ″	Rev. ″
1728.	δ Aurig.	7 27,5	[6]	flutt. -	7 27,5	0 0,0
Inch ap.	35th Camel.	9 19,6	[6]	faint -	7 29,6	1 24,0
1,4 ap.	46th Aurig.	7 29,4	[8]	-	7 19,7	0 9,7
	13th Lyncis	5 31,5	[8]	-	8 4,3	2 6,8
	ε Urs. Maj.	11 32,3	[8]	-	8 7,3	3 25,0
	21 Can. Ven.	10 20,7	[8]	-	7 31,2	2 23,5
	ζ Urs. Maj.	11 29,0	[8]	-	7 17,8	4 11,2
	η ——	12 17,1	[9]	-	7 8,8	5 8,3
Jan. 25.	β Drac.	2 13,4	[8]	-	6 8,4	3 29,0
	γ —	8 0,8	[9]	-	4 32,8	3 2,0
	ε Urs. Maj.	8 21,0	[9]	-	4 30,5	3 24,5
	ζ ——	8 9,6	[8]	-	3 31,8	4 11,8
	η ——	11 24,8	[8]	-	6 14,8	5 10,0
26.	9th Auriga	9 32,4	[8]	-	7 12,9	2 19,5
	Capella	10 23,9	[7]	-	6 16,9	4 7,0
	η Urs. Maj.	10 33,7	[3]	flutt. much	5 24,7	5 9,0
27.	Capella	10 29,4	[8]	-	6 21,8	4 7,6
	18th Camel.	4 32,2	[8]	-	6 18,9	1 20,7
	δ Aurig.	9 5,5	[6]	-	9 6,0	0 0,5 north
Frosty	35th Camel.	11 11,5	[8]	-	9 20,8	1 24,7
weather.	46th Aurig.	9 15,5	[8]	-	9 5,5	0 10,0
	13th Lyncis	6 18,4	[8]	-	8 25,9	2 7,5
	ι Urs. Maj.	6 1,2	[8]	-	8 26,2	2 25,0
	f ——	5 15,4	[8]	-	9 9,2	3 27,8
	θ ——	8 29,7	[6]	flutt. -	9 13,5	0 17,8
	β ——	0 10,4	[6]	flutt. -	11 27,8	11 17,4
	ψ ——	12 6,8	[6]	flutt. -	11 0,3	1 6,5
	χ ——	13 21,7	[6]	flutt. -	10 29,0	2 26,7
	γ ——	13 16,5	[6]	flutt. much	11 11,5	2 5,0

The stars fluttered much all this night.

Feb. 2.	β Cass.	11 07,8	[8]	-	A. 12 27,3	1 19,5
Frosty weather, stars moved pretty steady.	α —	14 4,8	[8]	-	12 32,3	1 6,5
	θ Pers.	7 3,2	[6]	-	7 7,7	0 4,5
	τ —	7 25,2	[9]	-	6 18,2	1 7,0
	γ —	3 18,8	[8]	-	6 21,8	3 3,0
	α —	8 19,8	[8]	-	6 8,5	2 11,3
	δ —	5 7,5	[8]	-	5 8,0	0 0,5
	9th Aurig.	6 31,7	[8]	-	4 12,7	2 19,0
	Capella	9 27,8	[8]	-	5 21,4	4 6,4
2 inch ap.	18th Camel.	3 15,0	[9]	-	5 3,0	1 22,0

		Rev. ″		Rev. ″	Rev. ″
1728.	δ Aurig.	6 17,8 [6]	flutt. -	6 19,5	0 1,7
	35th Cam.	8 4,0 [8]	-	6 12,7	1 25,3
Feb. 3.	β Cass.	4 32,1 [8]	-	6 15,7	1 17,6
	α —	9 24,6 [8]	-	8 16,1	1 8,5

I saw θ Cass, but it appeared too faint to be observed.

	θ Pers.	4 23,4 [8]	faint -	4 27,1	0 3,7
	τ —	9 28,3 [6]	-	8 20,3	1 8,0
	γ —	5 28,0 [8]	-	8 30,3	3 2,3
	α —	11 11,7 [10]	-	9 0,0	2 11,7
	δ —	8 12,7 [8]	-	8 12,7	0 0,0
	9th Aurig.	11 19,6 [8]	-	9 1,1	2 18,5
	Capella	13 13,0 [8]	-	9 6,3	4 6,7
	18th Camel.	7 22,2 [8]	-	9 10,2	1 22,0
	δ Aurig.	9 23,2 [8]	-	9 24,2	0 1,0
	35 Cam.	11 26,1 [8]	-	10 1,4	1 24,7
	46 Aurig.	10 23,5 [8]	-	10 14,7	0 8,8
4.	θ Pers.	10 24,7 : :	too faint	10 27,7	0 3,0
	τ —	11 20,0 [3]	faint -	10 14,2	1 5,8
	γ —	7 7,7 [8]	-	10 10,7	3 3,0

These stars fluttered and appeared in the same manner as on January 23.

	α —	12 15,7 [4]	flutt. -	10 4,5	2 11,2
	δ —	9 11,5 [6]	-	9 12,1	0 0,6
	9th Aurig.	10 15,6 [8]	-	7 32,2	2 17,4
	Capella	11 25,6 [3]	flutt. much : :	7 19,6	4 6,0
	18 Camel.	5 20,0 [6]	-	7 9,3	1 23,3
	δ Aurig.	7 8,7 : :	flutt. much	7 10,7	0 2,0
	35 Cam.	8 18,0 [8]	-	6 29,0	1 23,0
	46 Aurig.	7 16,6 [3]	flutt. -	7 8,6	0 8,0
5.	β Cass.	4 7,0 [6]	-	6 24,0	1 17,0
	α —	10 17,0 [8]	-	9 9,5	1 7,5
	θ Pers.	8 26,8 [3]	too faint	8 30,0	0 3,2
	τ —	10 7,5 [6]	-	8 33,1	1 8,4
	γ —	5 11,8 [8]	-	8 13,5	3 1,7
	α —	11 13,1 [6]	too bright	9 0,6	2 12,5
	δ —	8 9,5 [8]	-	8 8,8	0 0,7 south
	9th Aurig.	11 32,8 [8]	-	9 13,8	2 19,0
	Capella	13 32,0 [8]	-	9 25,0	4 7,0
	18 Cam.	7 7,7 [8]	-	8 30,7	1 23,0
	δ Aurig.	8 17,5 [6]	too bright	8 18,7	0 1,2 north

1728.		Rev. ″		Rev. ″	Rev. ″
	35 Camel.	9 0,0 [8]	-	7 8,2	1 25,8
	46 Aurig.	7 2,8 : :	flutt. much	6 27,6	0 9,2
Feb. 10.	β Drac.	1 26,5 [8]	-	A. 5 18,0	3 25,5
	γ —	7 25,3 [8]	-	4 18,3	3 7,0
14.	δ Pers.	4 30,1 [8]	-	4 28,9	0 1,2
	Capella	9 30,1 [8]	-	5 23,6	4 6,5
16.	β Drac.	1 27,5 [8]	-	5 16,5	3 23,0
	ι Herculis	4 17,4 [7]	-	6 16,2	1 32,8
	γ Drac.	9 6,0 [9]	-	5 31,5	3 8,5
	γ Pers.	2 32,2 [4]	-	5 33,2	3 1,0
	α —	12 31,0 [8]	-	10 17,0	2 14,0
	δ —	10 16,5 [8]	-	10 15,5	0 1,0
17.	β Urs. M.	0 9,0 [8]	-	11 32,0	11 23,0
	ψ ——	18 12,7 [6]	flutt. -	17 8,4	1 4,3
	χ ——	19 11,0 [4]	flutt. -	16 21,5	2 23,5
	γ ——	18 21,6 [8]	B. 16 18,0	A. 16 19,0	2 2,6
	3 Can. Ven.	12 19,8 [9]	-	15 18,3	2 32,5
	ε Urs. M.	19 10,3 [6]	flutt. -	15 21,5	3 22,8
	21 Can. Ven.	18 9,7 [8]	-	15 21,1	2 22,6
	ζ Urs. M.	19 17,5 [7]	flutt. -	15 7,8	4 9,7
	η ——	19 17,8 [8]	-	14 9,5	5 8,3
18.	9th Aurig.	16 15,1 [8]	-	13 31,6	2 17,5
	Capella	16 29,2 [8]	flutt. -	12 23,6	4 5,6
	18th Camel.	11 5,0 [8]	-	12 28,0	1 23,0
	δ Aurig.	14 7,5 [8]	-	14 11,0	0 3,5
	35th Camel.	14 23,4 [8]	-	13 0,2	1 23,2
	46 Aurig.	13 10,7 [6]	-	13 3,2	0 7,5
	13 Lyncis	11 30,0 [8]	-	14 7,0	2 11,0
21.	α Pers.	13 9,6 [6]	-	A. 10 30,4	2 13,2
	δ —	11 8,1 [6]	faint B.11 7,8	A. 11 6,7	0 1,4
	9th Aurig.	13 20,0 [8]	-	11 2,0	2 18,0
	Capella	15 0,7 [9]	-	10 29,0	4 5,7

I scarce ever saw a star move so steady as this did now; this is the first night that it has been observed without illuminating the threads.

18th Camel.	8 22,0 [8]	-	10 11,3	1 23,3
δ Aurig.	10 13,2 [9]	-	10 15,5	0 2,3
35 Camel.	12 23,5 [8]	-	11 0,3	1 23,2
46th Aurig.	11 3,9 [8]	-	10 30,4	0 7,5
β Urs. Maj.	0 0,8 [6]	flutt. -	11 24,0	11 23,2
ψ ——	11 25,2 [7]	flutt. -	10 23,7	1 1,5

1728.		Rev. //		Rev. //	Rev. //
March 1.	35 Camel.	12 31,6 [6]	too faint	11 7,2	1 24,4
	46 Aurig.	11 12,8 [4]	-	11 5,6	0 7,2
2.	Capella	15 32,9 [9]	steady	11 25,7	4 7,2
Inch ap.	δ Aurig.	11 20,4 [8]	-	11 22,8	0 2,4
	35th Camel.	12 25,7 [8]	faint -	11 1,2	1 24,5
	46th Aurig.	11 24,5 [8]	flutt. -	11 18,5	0 6,0
	13th Lyncis	9 9,2 [8]	flutt. -	11 19,8	2 10,6
	ι Urs. Maj.	8 11,3 [8]	flutt. -	11 9,3	2 32,0
	f ——	7 1,8 [8]	-	11 3,5	4 1,7
	θ ——	11 16,0 [6]	flutt. much	12 7,5	0 25,5
5.	θ ——	11 14,3 [6]	flutt. -	12 6,8	0 26,5
	β ——	0 22,7 [9]	-	12 14,7	11 26,0
	ψ ——	11 3,5 [8]	flutt. -	10 4,3	0 33,2
6.	Capella	13 6,9 [9]	-	8 33,4	4 7,5
7.	β Drac.	4 1,8 [6]	-	7 25,8	3 24,0
	ι Herculis	7 11,7 [3]	: : -	9 9,9	1 32,2
	γ Drac.	12 21,2 [8]	-	9 12,7	3 8,5
	Capella	12 27,8 [8]	-	8 19,8	4 8,0
8.	——	12 24,2 [10]	-	8 16,3	4 7,9
12.	α Cass.	8 30,5 [3]	½ out, faint	7 13,0	1 17,5
	Capella	10 13,6 [8]	-	6 6,3	4 7,3
	β Urs. M.	0 24,0 [6]	flutt. much	12 15,0	11 25,0
	ψ ——	12 3,8 [3]	flutt. much	11 8,7	0 29,1
13.	f ——	5 22,2 [8]	-	9 26,2	4 4,0
	θ ——	9 25,8 [8]	-	10 18,3	0 26,5
	β ——	0 29,0 [6]	flutt. -	12 23,3	11 28,3
	ψ ——	14 24,7 [6]	flutt. -	13 26,9	0 31,8
15.	f ——	9 17,3 [8]	st. -	13 19,9	4 2,6
17.	β ——	0 0,6 [8]	st. -	11 29,7	11 29,1
	ψ ——	11 15,7 [8]	st. -	10 19,1	0 30,6
	χ ——	13 21,3 [8]	-	11 5,6	2 15,7
	γ ——	12 32,0 [8]	-	11 3,3	1 28,7
18.	β Drac.	6 24,8 [8]	-	10 15,5	3 24,7
Manè.	ι Herculis	7 31,4 [3]	½ out -	9 30,4	1 32,3
	γ Drac.	3 30,9 [7]	flutt. -	0 23,0	3 7,9
	Capella	12 31,8 [9]	st. -	8 24,8	4 7,0
19.	——	13 0,9 [8]	-	8 26,1	4 8,8
20.	β Drac.	4 10,7 [8]	-	8 2,1	3 25,4
Manè.	ι Herculis	7 33,3 [6]	flutt. -	9 32,0	1 32,7
	γ Drac.	16 21,8 [8]	-	B. 13 14,3 [6]	3 7,5

As I was letting down the plummet to rectify the spot to it, after the passage as usual, the wire catched in something and broke about a yard from the bottom.

March 20. I put on another wire this evening.

Memorand. 'Tis sufficient to pinch the wire with the head of the small screw at top (after 'tis in the notch and drawn up with the plummet on) without turning the wire round the screw.

1728.		Rev. ″		Rev. ″	Rev. ″
. 21.	β Cass.	11 16,8 [6]	wind very strong	A. 12 24,8 : :	1 8,0
	α —	16 7,8 [6]	B. 14 24 0 : :	A. 14 25,0 : :	1 17,3

The wind blowed so strong that I could not adjust the spot with any certainty, by reason of the motion of the plummet.

	Capella	19 11,8 [8]		B. & A. 15 3,4	4 8,4
	ε Urs. M.	18 6,9 [8]	st. -	A. 14 28,4	3 12,5
	21 Can. Ven.	14 25,6 [8]	-	12 11,1	2 14,5
	ζ Urs. M.	16 16,3 [9]	st. -	12 15,8	4 0,5
	η ——	16 21,5 [8]	st. -	11 21,0	5 0,4
22.	β Drac.	8 33,8 [8]	st. -	12 25,8	3 26,0
Manè.	ι Herc.	10 0,7 [8]	-	A. 11 33,5	1 32,8
	γ Drac.	14 30,2 [8]	st. -	11 22,5	3 7,7
	Capella	15 32,4 [8]	st. -	11 23,7	4 8,7
	γ Urs. Maj.	17 20,5 [8]	-	15 29,3	1 25,2
23.	β Cass.	12 32,7 [6]	faint -	14 4,9	1 6,2
	α —	16 6,3 [8]	-	14 22,9	1 17,4
	α Pers.	13 29,3 [8]	-	11 10,8	2 18,5
24.	β Drac.	6 29,4 [8]	flutt. -	10 21,9	3 26,5
Manè.	ι Herc.	8 17,4 [7]	flutt. -	10 16,5	1 33,1
	γ Drac.	13 4,4 [7]	flutt. -	9 32,2	3 6,2
	β Urs. M.	0 15,1 [6]	flutt. -	12 13,1	11 32,0
	ψ ——	12 14,9 [7]	flutt. -	11 19,6	0 29,3
	γ ——	13 6,6 [8]	-	11 15,3	1 25,3
April 2.	ι ——	9 9,9 [8]	-	12 12,4	3 2,5
	f ——	7 25,9 [8]	-	11 32,4	4 6,5
	θ ——	11 1,6 [8]	-	11 32,8	0 31,2
	χ ——	13 18,3 [8]	-	11 7,3	2 11,0
	γ ——	12 9,8 [8]	flutt. -	10 21,2	1 22,6
6.	ε ——	14 13,0 [8]	-	11 5,0	3 8,0
	21 Can. Ven.	12 2,7 [8]	-	9 27,9	2 8,8
	ζ Urs. Maj.	13 30,4 [10]	-	9 33,7	3 30,7
	β Drac.	7 21,9 [8]	-	11 17,2	3 29,3

1728.		Rev. ″		Rev. ″	Rev. ″
	ι Herculis	9 16,8	[8] -	11 18,6	2 1,8
	ξ Draconis	8 13,0	[8] -	B. 11 22,0	3 9,0
	γ Drac.	11 5,3	[8] flutt. -	A. 8 0,3	3 5,0
	d —	3 31,6	[8] -	7 26,1	3 28,5
	c —	11 14,1	[10] -	7 13,3	4 0,8
April 7.	β Cassiop.	7 6,7	[8] faint -	8 10,0	1 3,3
	α ——	11 21,5	[8] faint -	10 0,9	1 20,6
	α Persei	11 4,2	[8] faint -	8 17,7	2 20,5
	ε Urs. Maj.	11 1,7	[6] flutt. much	7 26,0	3 9,7
	21 Can. Ven.	11 2,4	[6] flutt. -	8 27,4	2 9,0
	ζ Urs. Maj.	14 6,0	[6] flutt. much	10 5,3	4 0,7
	η ——	15 11,7	[6] flutt. much : :	10 17,2	4 28,5
	β Drac.	7 4,8	[3] flutt. very much	10 32,5	3 27,7
	ι Herculis	9 22,9	flutt. much	11 26,9	2 4,0
	ξ Drac.	7 32,6	flutt. much	B. 11 6,3	3 7,7
	γ —	9 30,0	flutt. exceedingly	A. 6 25,0	3 5,0
10.	η Urs. Maj.	16 11,2	[10] -	11 15,7	4 29,5
15.	ζ ——	16 3,5	[9] st. -	12 9,0	3 28,5
	η ——	17 4,0	[9] st. -	12 10,0	4 28,0
16.	Capella	16 29,8	[9] -	12 18,1	4 11,7
	β Urs. Maj.	0 28,2	[6] flutt. -	12 30,3	12 2,1
	ψ ——	12 21,1	[6] flutt. -	11 30,6	0 24,5
	χ ——	13 20,4	[8] -	11 10,4	2 10,0
	γ ——	12 18,6	[8] flutt. -	10 31,0	1 21,6
	ε ——	14 21,3	[7] flutt. -	11 15,0	3 6,3
	21 Can. Ven.	13 19,3	[6] -	11 11,8	2 7,5
	ζ Urs. Maj.	15 13,8	[8] -	11 18,8	3 29,0
	η ——	15 27,8	-	10 33,0	4 28,8
17.	ι ——	7 11,1	[4] faint -	10 14,1	3 3,0
	θ ——	7 19,0	[8] -	8 15,4	0 30,4
	β ——	0 9,7	[6] flutt. -	12 9,7	12 0,0
	ψ ——	12 25,3	[6] flutt. -	12 2,0	0 23,0
	χ ——	13 27,8 :	[4] flutt. m.	11 18,8	2 9,0
	γ ——	13 7,0 :	[3] flutt. v. m.	11 19,0	1 22,0
	ε ——	15 8,2 :	flutt. exc.	11 32,4	3 9,8
	ζ ——	15 14,5 :	flutt. m.	11 20,0	3 28,5
	η ——	15 2,2 : :	flutt. exceed.	10 7,4	4 28,8

The stars fluttered very much all this night; the wind was south-east, and the sky looked very clear.

18.	β Cass.	9 3,0	[6] faint -	10 4,4	1 1,4

		Rev. ″			Rev. ″	Rev. ″
1728.	α Cass.	11 9,8	[6] faint -		9 20,2	1 23,6
	β Urs. M.	29 21,1	[6] flutt. -		11 22,5	12 1,4
	ψ ——	12 11,7	[6] -		11 20,7	0 25,0
	ε ——	15 7,5	[6] flutt. -		12 1,7	3 5,8
	ζ ——	16 5,3	[8] flutt. -		12 9,6	3 29,7
	η ——	17 17,8	[7] flutt. -		12 24,1	4 27,7
April 23.	Capella	19 7,9	[8] -		14 29,8	4 12,1
24.	ε Urs. Maj.	18 04,6	[3] flutt. m.		14 32,8	3 05,8
25.	α Pers.	16 11,3	[8] -		13 22,3	2 23,0
26.	β Urs. Maj.	0 12,7	[7] -		12 16,4	12 3,7
	ψ ——	11 14,1	[8] st. -		10 25,4	0 22,7
	γ ——	12 24,6	[8] -		11 6,1	1 18,5
28.	Capella	16 32,8	[8] -		12 19,8	4 13,0
May 5.	ε Urs. Maj.	13 13,0	[6] flutt. -		10 9,8	3 3,2
	21 Can.Ven.	11 9,2	[8] -		9 6,0	2 3,2
	ζ Urs. Maj.	12 0,5	[6] flutt. -		8 10,5	3 24,0
	η ——	13 31,5	[9] -		9 7,8	4 23,7
	β Drac.	4 4,4	[6] flutt. -		8 6,1	4 1,7
	ι Herc.	5 24,5	[3] flutt. m.		7 33,8	2 9,3
	ξ Drac.	4 31,0	flutt. ext.		8 10,2	3 13,2
	γ —	7 29,1	flutt. ext.		4 30,6	2 32,5
6.	β —	0 11,6	[8] -		4 13,9	4 2,3
	ι Herc.	4 29,8	[8] -		7 4,8	2 9,0
	ξ Drac.	5 4,6	[8] -	B.	8 20,1	3 15,5
	γ —	7 13,9	[8] -	A.	4 16,4	2 31,5
7.	Capella	8 21,6	[8] -		4 6,8	4 14,8
	β Urs. Maj.	29 4,9	[8] -		11 7,9	12 3,0
	ψ ——	12 31,4	[8] -		12 7,9	0 23,5
	χ ——	13 18,5	[8] -		11 12,5	2 6,0
	γ ——	13 8,7	[7] -		11 22,5	1 20,2
	β Drac.	7 25,5	[3] flutt. v. m.		11 23,0	3 31,5
	ι Her.	8 24,8	[3] flutt. v. m.		11 2,3	2 11,5
	ξ Drac.	8 07,0	flutt. ext.		11 17,5	3 10,5
	γ —	10 0,1	flutt. ext.		7 3,8	2 30,3
8.	β Cass.	7 1,8	[6] hazy -		7 32,8	0 31,0
	α —	11 24,0	[8] -		9 33,0	1 25,0
	α Pers.	13 12,0	[7] -		10 20,2	2 25,8
	Capella	12 22,5	[6] flutt. m.		8 9,0	4 13,5
10.	——	12 19,5	[6] -		8 3,5	4 16,0
	β Drac.	4 28,0	[6] -		8 28,5	4 0,5

		Rev. ″			Rev. ″	Rev. ″
1728.	ι Herc.	6 9,3	[3]	flutt. m.	8 20,0	2 10,7
	ξ Drac.	4 23,5	[3]	flutt. m.	8 6,5	3 17,0
	γ —	7 16,0	[3]	flutt. m.	4 19,0	2 31,0
May 11.	ε Urs. Maj.	7 20,2	[6]	-	4 17,2	3 3,0
	21 Can. Ven.	7 17,5	[7]	-	5 15,5	2 2,0
	ζ Urs. M.	10 1,7	[3]	flutt. -	6 11,0	3 24,7
	η ——	10 19,5	[3]	flutt. much	5 32,5	4 21,0
Then I went to Oxford.						
June 4.	β Drac.	4 25,9	[6]	-	9 3,6	4 11,7
	ι Herc.	8 31,4	[6]	-	11 15,4	2 18,0
	ξ Drac.	7 24,0	[6]	-	11 16,0	3 26,0
	γ —	10 18,2	[6]	-	7 30,4	2 21,8
5.	Capella	11 32,8	[8]	-	7 14,8	4 18,0
	γ Urs. Maj.	9 19,5	[6]	-	8 2,5	1 17,0
	ε ———	11 11,9	[10]	-	8 12,9	2 33,0
	ζ ———	11 31,2	[9]	-	8 12,0	3 19,2
	η ———	13 31,8	[9]	-	9 14,0	4 17,8
	ι Herculis	7 27,5	[6]	-	10 11,0	2 17,5
	ξ Drac.	5 17,1	[6]	-	9 10,8	3 27,7
	γ —	8 10,6	[6]	-	5 23,0	2 21,6
7.	β Cassiop.	7 16,9	[6]	-	8 19,4	1 2,5
	α ——	11 6,7	[7]	-	9 17,9	1 22,8
	Capella	15 25,1	[8]	-	11 5,9	4 19,2
8.	β Cass.	9 8,0	[6]	-	10 11,0	1 3,0
	β Urs. Maj.	0 6,0	[7]	-	12 9,5	12 3,5
	γ ———	12 11,0	[6]	faint, hazy	10 29,5	1 15,5
	ε ———	13 29,1	[10]	-	10 30,1	2 33,0
	ζ ———	13 14,7	[9]	-	9 30,7	3 18,0
	η ———	18 1,8	[11]	-	13 18,3	4 17,5
9.	ε ———	17 27,2	[10]	-	14 28,7	2 32,5
12.	Capella	19 5,5	[8]	-	14 19,5	4 20,0
15.	γ Urs. Maj.	16 15,9	[8]	-	14 32,2	1 17,7
	ε ———	17 24,0	[10]	-	14 25,2	2 32,8
	ζ ———	17 22,7	[10]	-	14 4,7	3 18,0
	η ———	19 28,8	[9]	-	15 11,3	4 17,5
	ι Herculis	7 31,7	[8]	-	10 21,2	2 23,5
	ξ Draconis	7 15,7	[6]	-	11 7,7	3 26,0
	γ ——	10 7,4	[8]	-	7 20,9	2 20,5
17.	β Cassiop.	6 26,3	[8]	-	7 30,5	1 4,2
	α ——	10 21,2	[10]	-	8 33,2	1 22,0

		Rev. ″		Rev. ″	Rev. ″
1728.	α Persei	13 7,2 [8]	-	10 13,2	2 28,0
	Capella	13 24,5 [8]	-	9 4,5	4 20,0
June 21.	η Urs. Maj.	14 00,0 [8]	-	9 17,0	4 17,0
	β Drac.	5 1,0 [6]	flutt. -	9 18,0	4 17,0
	ι Herc.	6 4,7 [8]	-	8 28,2	2 23,5
	ξ Drac.	5 7,5 [9]	flutt. -	9 2,5	3 29,0
	γ —	8 16,0 [7]	flutt. -	5 33,5	2 16,5
22.	α Pers.	9 3,0 [8]	-	6 8,5	2 28,5
	Capella	9 31,2 [8]	-	5 11,0	4 20,2
	γ Urs. Maj.	9 3,0 [6]	faint -	7 20,7	1 16,3
	ε ———	10 32,7 [8]	-	8 1,7	2 31,0
	ζ ———	11 3,5 [10]	I could see the small star.	7 20,3	3 17,2
	η ———	11 26,0 [9]	-	7 9,6	4 16,4
	β Drac.	2 30,2 [10]	-	7 15,0	4 18,8
	ι Herc.	5 15,0 [6]	flutt. -	8 5,5	2 24,5
	ξ Drac.	4 31,7 [7]	-	8 33,2	4 1,5
	γ —	24 23,0 [6]	flutt. -	22 06,4	2 16,6
23.	β Cass.	9 24,0 [6]	hazy -	10 27,3	1 3,3
	α —	12 32,0 [6]	-	11 9,3	1 22,7
	α Persei	12 5,3 [6]	faint, hazy	9 9,6	2 29,7
	Capella	11 9,6 [8]	flutt. -	6 22,8	4 20,8
24.	β Cass.	6 11,8 [6]	hazy -	7 15,8	1 4,0
	α —	11 5,1 [8]	-	9 17,1	1 22,0

When I went to observe α Persei, I found the wire broke just at the notch at top, but cannot tell whether I had done any thing more than usual to occasion this. I immediately fixed on another, and then observed

	Capella	13 25,2 [6]	very hazy	9 6,2	4 19,0
	β Urs. M.	29 5,0 [3]	very faint	11 6,5	12 1,5
	γ ———	13 5,8 [6]	faint -	11 22,8	1 17,0
	ε ———	15 21,0 [8]	-	12 22,5	2 32,5
	ζ ———	17 7,0 [8]	hazy -	13 23,0	3 18,0
	η ———	17 24,4 [10]	-	13 8,9	4 15,5
25.	Capella	17 9,5 [7]	v. hazy	12 23,0	4 20,5
	ε Urs. M.	20 1,2 [6]	Ld. C. Cavendish obs.	17 2,2	2 33,0
	η ——	22 29,0 [6]	ditto -	18 13,0	4 16,0
	β Drac.	13 32,9 [6]	ditto -	18 16,9	4 18,0
	γ —	21 8,5 [6]	ditto -	18 27,5	2 15,0
28.	β Cass.	12 32,7 [9]	-	14 4,2	1 5,5
	α —	16 32,0 [8]	-	15 12,0	1 20,0

		Rev. ″	Rev. ″	Rev. ″
1728.	γ Pers.	12 24,5 [6] faint -	15 9,5	2 20,0
	α —	18 3,5 : : cloudy	15 10,0	2 27,5
	ζ Urs. M.	24 10,0 [8] -	20 25,5	3 18,5
	η ——	24 22,5 [9] -	20 7,0	4 15,5
June 29.	η ——	28 26,6 [6] -	24 10,6	4 16,0
30.	ι Herculis	24 5,0 [8] flutt. -	26 32,0	2 27,0
	ξ Drac.	11 18,2 [8] -	15 17,2	3 33,0
	γ —	13 28,7 [8] -	11 13,7	2 15,0
July 2.	ζ Urs. Maj.	14 9,5 [10] -	10 25,0 :	3 18,5
	η ——	15 19,0 [9] -	11 3,2	4 15,8
	β Drac.	6 24,5 [6] -	11 9,5	4 19,0
	ι Herc.	9 1,0 [6] flutt. m.	11 31,0	2 30,0
	ξ Drac.	8 9,5 [3] flutt. m.	12 8,5	3 33,0
	γ —	10 29,7 [3] flutt. m.	8 14,7	2 15,0
3.	γ Urs. Maj.	12 4,5 [3] v. faint	10 20,5	1 18,0
	ζ ——	17 15,5 [6] faint	13 30,5	3 19,0
	β Drac.	12 16,4 [8] st. -	17 3,7	4 21,3
	ι Hercul.	16 8,0 [8] -	19 2,5	2 28,5
	ξ Drac.	15 8,0 [7] -	19 8,5	4 0,5
	γ —	18 4,4 [9] -	15 23,9	2 14,5
4.	Capella	19 26,5 [9] -	15 7,5	4 19,0
	β Drac.	10 1,5 [6] flutt. -	14 21,5	4 20,0
	ι Herc.	11 25,3 [4] flutt. m.	14 20,8	2 29,5
	ξ Drac.	13 24,0 : : flutt. ext.	9 24,0	4 0,0
	γ —	10 17,7 : : flutt. ext.	8 3,2	2 14,5
5.	β Cass.	6 18,8 [7] flutt. -	7 24,8	1 6,0
	α —	12 12,6 [6] hazy -	10 27,2	1 19,4
	α Persei	11 11,3 [9] -	8 17,7	2 27,6
	Capella	12 10,0 [6] flutt. -	7 24,5	4 19,5
	ε Urs. M.	11 9,0 [6] -	8 9,5	2 33,5
	β Drac.	3 26,5 [8] -	8 13,0	4 20,5
	ξ —	5 20,0 [6] -	9 21,0	4 1,0
	γ —	7 8,0 [6] flutt. m.	4 30,3	2 11,7
6.	Capella	9 27,7 [9] -	5 7,7	4 20,0
	ε Urs. Maj.	10 27,0 [9] -	7 26,5	3 0,5
12.	Capella	21 19,0 [8] -	16 33,5	4 19,5
13.	——	21 12,3 [8] -	16 26,3	4 20,0
	β Drac.	12 6,3 [9] st. -	16 28,3	4 22,0
	ι Herculis	13 30,8 [8] -	16 26,4	2 29,6
	ξ Drac.	12 9,4 [8] -	16 11,1	4 1,7

		Rev. ″		Rev. ″	Rev. ″
1728.	γ Drac.	14 19,2	[8] st. -	12 9,4	2 9,8
July 17.	η Urs. Maj.	17 20,9	[8] -	13 4,4	4 16,5
18.	β Drac.	8 13,2	[9] st. -	13 2,7	4 23,5
	ι Herc.	10 17,0	[6] -	13 14,0	2 31,0
	ξ Drac.	8 25,5	[6] -	12 30,5	4 5,0
	γ —	10 25,0	[8] flutt. -	8 15,5	2 9,5
19.	α Pers.	11 3,0	[6] cloudy	8 9,5	2 27,5
	Capella	11 22,3	[9] -	7 1,6	4 20,7
20.	γ Pers.	5 30,5	[6] faint, hazy	8 15,5	2 19,0
	α —	10 22,0	[8] -	7 31,0	2 25,0
	δ —	8 31,7	[6] hazy, v. faint	8 18,2	0 13,5
	Capella	13 14,7	[10] -	8 29,2	4 19,5
22.	α Pers.	10 32,6	[9] -	8 5,8	2 26,8
26.	β Drac.	6 23,5	[6] ½ out	11 15,0	4 25,0
	ι Herc.	8 3,2	[8] -	11 2,5	2 33,3
	ξ Drac.	6 26,0	[8] -	10 30,8	4 4,8
	γ —	7 30,4	[8] -	5 21,4	2 9,0
Manè. 27.	β Cass.	5 22,2	[9] st. -	7 0,7	1 12,5
	α —	9 25,0	[8] flutt. -	8 11,0	1 14,0
	γ Pers.	8 13,0	[6] cloudy	11 0,7	2 21,7
	α —	13 28,4	[10] st. -	11 3,4	2 25,0
	δ —	10 25,0	[4] hazy -	10 14,0	0 11,0
	Capella	15 1,8	[10] -	10 16,3	4 19,5
28.	β Drac.	13 33,5	[8] st. -	18 24,0	4 24,5
	ι Herc.	15 8,5	[8] flutt. -	18 8,0	2 33,5
	ξ Drac.	14 18,4	[8] -	18 24,4	4 6,0
	γ —	16 0,0	[10] -	13 27,0	2 7,0
Manè. 29.	β Cass.	8 25,5	[9] -	10 4,5	1 13,0
	α —	11 18,2	[8] -	10 5,4	1 12,8
	τ Pers.	11 32,0	[6] v. faint	10 10,3	1 21,7
	α —	14 27,5	[9] -	12 2,5	2 25,0
	δ —	12 25,2	[6] v. faint	12 14,7	0 10,5
	Capella	16 33,2	[9] -	12 13,4	4 19,8
31.	β Cass.	10 33,5	going out	12 15,5	1 14,5
	α —	14 26,0	¾ out	13 14,0	1 13,0
Aug. 1.	β Dracon.	8 11,0	[10] st. twilight	13 2,7	4 25,7
2 inch ap.	ι Herc.	9 17,2	[9] -	12 17,7	3 0,5
	ξ Drac.	8 16,4	[8] -	12 24,9	4 8,5
	γ —	11 17,0	: : dub. ½ out	9 11,0	2 7,0
2.	ε Urs. M.	12 0,0	[6] cloudy	8 29,0	3 5,0

1728.		Rev. //		Rev. //	Rev. //
	ζ Urs. Maj.	12 31,5	[6] faint	9 8,5	3 23,0
	η ——	12 22,8	[8] -	8 4,8	4 18,0
	β Drac.	2 29,5	[8] -	7 20,5	4 25,0
	ι Herc.	4 33,5	[8] -	8 0,5	3 1,0
	ξ Drac.	3 10,5	[6] -	7 17,7	4 7,2
	γ —	6 26,5	[9] st. -	4 19,5	2 7,0
Aug. 3.	τ Pers.	6 26,0	[6] faint -	5 8,0	1 18,0
Manè.	γ —	3 15,0	[6] faint, flutt.	6 1,5	2 21,5
	α —	10 6,0	[6] flutt. m. foggy	7 17,0	2 23,0
	δ —	7 1,5	[6] v. faint	6 23,5	0 13,0
	Capella	10 16,5	[10] st. -	5 32,0	4 18,5
	η Urs. M.	13 19,5	[3] : : hazy	9 1,5	4 18,0
5.	β ——	29 25,0	[3] v. faint	11 17,5	11 26,5
	ε ——	14 10,0	[8] -	11 4,0	3 6,0
	ζ ——	14 23,0	[7] -	11 0,0	3 23,0
	η ——	15 9,5	[7] -	10 24,5	4 19,0
	β Drac.	6 11,5	[9] -	11 4,5	4 27,0
	ι Herc.	8 4,0	[8] -	11 6,0	3 2,0
	ξ Drac.	6 31,5	[8] -	11 5,0	4 07,5
	γ —	8 0,8	[9] -	5 28,8	2 6,0
7.	τ Pers.	7 4,5	[7] flutt. -	5 18,0	1 20,5
	γ —	2 11,5	[7] flutt. -	4 33,5	2 22,0
Error.	α —	8 11,3	[8] flutt. v. m.	5 26,8	2 18,5
Forsan.		8 16,3			
	δ —	5 11,0	: : v. faint	5 2,0	0 9,0
	Capella	9 10,5	[6] flutt. m.	4 27,5	4 17,0
8.	τ Pers.	7 22,2	[8] flutt. exc.	6 1,7	1 20,5
	α —	8 27,5	[4] flutt. exc.	6 4,5	2 23,0
	δ —	6 3,0	: : flutt. exc.	5 30,0	0 7,0
	Capella	10 21,5	[4] flutt. ext.	6 4,0	4 17,5

The stars fluttered very much all this morning, so that I could not bisect them with any certainty. I observed most of these several times, and took the mean of the several trials.

10.	β Drac.	0 25,8	[8] -	5 17,8	4 26,0
	ι Herculis	2 18,5	[8] -	5 21,5	3 3,0
	ξ Drac.	3 27,5	[6] -	7 33,0	4 5,5
	γ —	4 23,7	[9] flutt. m.	2 15,7	2 8,0
12.	ζ Urs. Maj.	6 28,0	[8] -	3 3,0	3 25,0
	η ———	8 17,0	[8] -	3 31,0	4 20,0

		Rev. ″		Rev. ″	Rev. ″
1728.	β Drac.	2 14,4	[8] -	7 7,4	4 27,0
	ι Hercul.	4 0,5	[8] -	7 4,5	3 4,0
	ξ Drac.	4 7,2	[8] -	8 14,7	4 7,0
	γ —	5 5,5	[8] flutt. -	2 33,5	2 6,0
Aug. 13.	τ Pers.	5 14,5	[8] -	3 29,8	1 18,7
	γ —	2 5,0	[8] -	4 28,5	2 23,5
	α —	6 20,0	[8] flutt. -	3 33,5	2 20,5
	δ —	3 24,7	[7] -	3 16,7	0 8,0
	Capella	7 24,0	[8] -	3 8,7	4 15,3
14.	γ Drac.	8 11,5	[9] st. -	6 6,5	2 5,0
15.	β —	6 13,0	[8] st. -	11 7,0	4 28,0
	ι Herc.	8 21,5	[8] : -	11 24,0	3 2,5
	ξ Drac.	7 25,5	[8] st. -	12 1,5	4 10,0
	γ —	10 7,3	[8] st. -	8 3,3	2 4,0
17.	ξ —	7 27,5	[9] -	12 3,5	4 10,0
	γ —	9 19,7	[10] v. st.	7 15,7	2 4,0
18.	β Drac.	2 16,7	[8] -	7 10,2	4 27,5
	ι Hercul.	3 18,0	[6] : -	6 23,0	3 5,0
	ξ Drac.	2 22,0	[8] flutt. -	6 30,5	4 8,5
	γ —	6 1,5	[8] flutt. -	3 31,2	2 4,3
Manè. 19.	τ Pers.	6 19,0	[6] cloudy	5 1,0	1 18,0
	γ —	4 17,5	[6] flutt. m.	7 8,5	2 25,0
	α —	9 21,8	[8] flutt. m.	7 2,8	2 19,0
	δ —	6 13,5	[6] flutt. m.	6 10,0	0 3,5
	Capella	10 32,5	[8] flutt.	6 17,2	4 15,3
21.	β Drac.	11 0,5	[7] $\frac{3}{4}$ out	15 30,7	4 29,5
	ι Herc.	13 28,7	[6] $\frac{1}{2}$ out, cloudy	16 30,4	3 1,5
22.	Capella	22 21,5	[8] hazy	18 3,8	4 17,7
	ξ Draconis	15 6,5	[8] -	19 17,5	4 11,0
	γ ——	15 28,5	[10] st. -	13 24,2	2 4,3
23.	β ——	8 27,7	[10] v. st.	13 21,7	4 28,0
	ι Hercul.	11 4,6	[8] -	14 6,8	3 2,2
	γ Drac.	16 12,5	[11] v. st.	14 8,5	2 4,0
24.	β Urs. Maj.	29 29,0	[9] -	11 14,3	11 19,3
	γ ——	14 4,0	[9] -	12 7,0	1 31,0
	ε ——	15 10,2	[9] -	12 1,5	3 8,7
	ζ ——	16 7,5	[10] -	12 15,2	3 26,3
	η ——	16 28,5	[10] -	12 6,2	4 22,3
25.	β Drac.	12 11,0	[10] -	17 5,0	4 28,0
	ι Herc.	14 14,0	[8] faint -	17 16,0	3 2,0

		Rev. ″			Rev. ″	Rev. ″
1728.	ξ Drac.	12 32,5 [8]		-	17 8,7	4 10,2
	γ —	14 8,2 [9]		- -	12 5,0	2 3,2
Aug. 26.	Capella	15 18,5 [8]	st.	-	11 1,0	4 17,5
27.	———	15 19,0 [9]		-	11 1,5	4 17,5
29.	β Drac.	5 3,0 [6]	cloudy		9 33,0	4 30,0
	γ —	11 26,5 [8]		-	9 23,5	2 3,0
31.	Capella	13 22,5 [11]	v. st.		9 5,0	4 17,5
Sept. 1.	γ Drac.	10 24,7 [8]		-	8 21,7	2 3,0

Sept. 3. I took off the wire of the plummet and fixed it on again about a quarter of an inch shorter, so that the bent part (that rested in the notch at top) might be cut off after 'twas again fastened. This I did in order to try whether it would hang exactly the same after as before; because I suspected that there might be some difference, the observations of last year not exactly agreeing with those of this. But upon my adjusting the same spot to the plummet after I had altered it, as I had done before, I could not perceive any difference; so that I judge this difference cannot arise from the uncertainty of hanging of the wire truly in the same place, but may possibly proceed from some small alteration in the tube itself, &c. if the succeeding observations shall be found to confirm the small disagreement that those already made of γ Drac. seem to shew.

		Rev. ″			Rev. ″	Rev. ″
	ζ Urs. Maj.	20 15,7 [10]		-	16 20,5	3 29,2
	β Drac.	14 19,4 [8]		-	19 4,4	4 29,0
	ι Herc.	15 15,2 [8]		-	18 17,5	3 2,3
	γ Drac.	19 26,5 [8]	flutt.	-	17 23,5	2 3,0
4.	γ —	15 1,3 [8]		-	12 32,3	2 3,0
6.	ε Urs. M.	16 4,6 [9]		-	12 25,6	3 13,0
	β Drac.	6 29,3 [8]		-	11 24,3	4 29,0
	γ —	27 3,5 [8]		-	25 0,5	2 3,0
8.	η Urs. M.	23 26,6 [8]		-	19 0,4	4 26,2
Manè. 9.	τ Pers.	19 30,0 [4]	flutt. ext.		18 17,5	1 12,5
	γ —	16 6,5 [3]	flutt. ext.		19 3,5	2 31,0
	α —	20 17,2 [3]	flutt. ex.		17 33,7	2 17,5
	δ —	18 4,7 [3]	flutt. ex.		17 33,5	0 5,2
	β Drac.	12 17,7 [9]		-	17 12,0	4 28,3
	γ —	19 2,9 [8]		-	17 0,4	2 2,5

Sept. 10. I took off the wire after having very well rectified a spot to it, and immediately fixed on a new one, and then compared the same spot with the

new wire as before with the old one; but I could not perceive that there was the least difference; I am sure, had there been a quarter of a second odds, I could have discerned it.

The reason why I have been thus curious in trying whether different wires agree with each other is, that if there should appear any little difference in the different years' observations, I might be satisfied that it cannot arise from the different wires, but must proceed from some other cause. And though the experiment of September 3d might seem sufficient, yet because that was made with the same wire (which was also pieced near the bob) I chose to repeat the experiment in the manner I did this day; which has not only confirmed the former, but also makes it evident that the tying a knot, &c. near the bob or ball, does no harm.

1728.		Rev. ″		Rev. ″	Rev. ″
Sept. 10.	β Drac.	10 7,8	[9] v. st.	15 2,1	4 28,3
	γ —	17 2,5	[9] -	14 33,5	2 3,0
11.	β Urs. M.	0 1,3	[9] -	11 15,3	11 14,0
	γ ——	14 12,7	[8] -	12 11,7	2 1,0
	ε ——	15 10,0	[3] : hazy	11 30,5	3 13,5
	β Drac.	10 31,8	[10] v. st.	15 26,5	4 28,7
	γ —	17 30,6	[10] -	15 28,1	2 2,5
12.	Capella	19 31,5	[6] flutt. m.	15 16,8	4 14,7
Manè. 13.	——	19 28,5	[6] flutt. m.	15 14,8	4 13,7
	18 Camel.	14 7,5	[3] faint -	15 6,5	0 33,0
	δ Aurig.	14 32,3	[6] -	14 12,8	0 19,5
	35 Camel.	16 27,7	[8] -	14 19,9	2 7,8
	β Urs. M.	29 32,5	[9] -	11 13,0	11 14,5
	γ ——	13 4,0	[8] -	11 1,0	2 3,0
	ε ——	14 11,8	[8] -	10 30,8	3 15,0
	β Cassiop.	9 24,9	[8] -	11 20,6	1 29,7
	α ——	13 4,0	[6] flutt. -	12 5,0	0 33,0
15.	β Drac.	8 1,7	[8] st. -	12 30,7	4 29,0
	γ —	14 14,6	[8] -	12 11,3	2 3,3
16.	Capella	16 17,5	: : flutt. v. m.	12 6,0	4 11,5
	β Drac.	6 27,9	[8] -	11 21,2	4 27,3
	γ —	14 3,2	[7] faint, hazy	12 0,5	2 2,7
17.	β Urs. M.	0 20,3	[8] -	11 33,8	11 13,5
	α Cassiop.	16 7,0	[6] flutt. m.	15 8,0	0 33,0
Manè. 18.	9 Aurig.	17 27,0	[3] faint -	14 25,5	3 1,5
	Capella	18 22,2	[6] flutt. -	14 7,7	4 14,5
	18 Camel.	12 12,0	[6] faint -	13 13,0	1 1,0

1728.		Rev. //		Rev. //	Rev. //
	δ Aurig.	13 15,5	flutt. ext.	12 32,0	0 17,5
	35 Camel.	14 27,0	: : flutt. ext. & faint	12 19,0	2 8,0
	β Cass.	12 4,0	[7] -	14 2,0	1 32,0
	α —	14 25,2	[8] cloud	13 29,2	0 30,0
Sept. 19.	α —	14 25,5	[7] -	13 30,0	0 29,5
20.	β Urs. M.	0 21,0	[8] -	11 33,3	11 12,3
	γ ——	13 22,5	[8] -	11 18,0	2 4,5
	η ——	15 23,7	[8] -	10 28,2	4 29,5
	β Cass.	11 3,0	[8] st. -	13 1,2	1 32,2
	α —	14 10,0	[8] -	13 14,2	0 29,8
21.	γ Urs. M.	15 4,7	[8] -	12 33,7	2 5,0
	ζ ——	17 12,5	[8] -	13 11,5	4 1,0
	η ——	18 12,6	[9] -	13 16,6	4 30,0
22.	Capella	18 17,0	[6] -	14 1,0	4 16,0
	18 Camel.	12 28,5	[3] faint -	13 31,5	1 3,0
26.	Capella	18 9,0	[10] -	13 30,0	4 13,0
	18 Camel.	12 24,7	[6] -	13 25,7	1 1,0
	δ Aurig.	13 22,0	[8] -	12 3,2	0 18,8
	35 Camel.	15 2,0	[7] -	12 28,0	2 8,0
	46 Aurig.	13 17,0	[4] flutt. -	12 26,0	0 25,0
27.	γ Drac.	15 4,5	[8] -	13 1,0	2 3,5
28.	ζ Urs. M.	16 0,0	[7] -	11 31,5	4 2,5
	η ——	16 10,2	[10] -	11 12,5	4 31,7
	β Drac.	6 25,7	[9] -	11 17,7	4 26,0
	γ —	13 3,0	[10] -	10 33,0	2 4,0
30.	Capella	14 30,0	[8] -	10 17,3	4 12,7
	18th Camel.	9 2,5	[3] faint -	10 2,5	1 0,0
Oct. 1.	Capella	15 14,5	[8] -	11 1,0	4 13,5
	18th Camel.	9 5,5	[6] -	10 9,0	1 3,5
	δ Aurig.	10 28,0	[6] -	10 10,2	0 17,8
	35th Camel.	12 5,8	[8] -	9 33,3	2 6,5
9.	ε Urs. M.	14 14,5	[10] v. g.	10 24,7	3 23,8
	ζ ——	13 32,5	[9] -	9 25,0	4 07,5
	η ——	14 27,8	[3] cloudy	9 25,3	5 2,5
11.	γ ——	15 24,7	[10] -	13 12,7	2 12,0
	ε ——	16 31,7	[11] v. g.	13 7,2	3 24,5
	ζ ——	16 29,3	[11] -	12 21,3	4 8,0

The air was so clear that I could this day see the small star that is contiguous to ζ.

	η Urs. Maj.	17 9,8	[9] -	12 7,3	5 2,5

		Rev. ″		Rev. ″	Rev. ″
1728.	β Drac.	6 24,5 [4]	hazy -	11 15,5	4 25,0
Oct. 12.	β Urs. M.	0 8,8 [8]	-	11 13,3	11 4,5
13.	β ——	0 4,2 [6]	hazy -	11 8,2	11 4,0
15.	ψ Cygni	10 9,0 [7]	-	11 18,7	1 9,7
	2 ο —	15 13,5 [6]	flutt. m.	11 12,5	4 1,0
	3 ω —	9 14,0 [8]	-	11 2,0	1 22,0
	β Cass.	8 0,0 [4]	flutt. v. m.	10 5,0	2 5,0
	α —	9 27,0 [6]	-	9 5,7	0 21,3
16.	γ Dracon.	7 26,5 [8]	-	5 19,2	2 7,3
18.	γ ——	10 27,0 [8]	-	8 20,0	2 7,0
	κ Cygni	13 13,3 [8]	-	12 26,7	0 20,6
	ι —	9 20,3 [8]	-	10 22,8	1 2,5
	θ —	15 9,5 [8]	-	12 5,0	3 4,5
	2 ο —	15 26,6 [8]	-	11 25,6	4 1,0
	3 ω —	9 25,0 [6]	-	11 12,0	1 21,0
	β Cass.	8 10,2 [9]	v. st. -	10 17,7	2 7,5
	α —	10 22,7 [8]	-	10 2,7	0 20,0
	τ Pers.	10 6,5 [8]	st. -	9 5,5	1 1,0
	γ —	6 28,7 [8]	flutt. -	10 1,7	3 7,0
	α —	11 25,0 [8]	flutt. -	9 17,0	2 08,0
	δ —	8 21,0 [8]	flutt. -	8 23,0	0 2,0 north
19.	ψ Cygni	9 22,7 [8]	-	10 32,2	1 9,5
	2 ο —	14 33,2 [10]	-	10 31,7	4 1,5
	3 ω —	9 8,2 [8]	-	10 29,2	1 21,0
	β Cassiop.	9 5,7 [8]	-	11 13,4	2 7,7
22.	γ Urs. Maj.	12 17,3 [4]	cl. -	10 3,3	2 14,0
	β Drac.	6 1,5 ::	dub. -	10 25,0	4 23,5
	γ —	10 14,3 [10]	-	8 7,3	2 7,0
29.	ε Urs. Maj.	11 29,7 [8]	-	7 33,0	3 30,7
	ζ ——	12 15,5 [6]	hazy -	8 0,2	4 15,3
	η ——	13 24,5 [4]	v. hazy	8 16,0	5 8,5
31.	τ Cass.	15 20,8 [8]	st. -	16 26,8	1 6,0
	β —	13 33,2 [9]	v. st., v. windy	16 9,2	2 10,0
	α —	17 3,2 [8]	-	16 19,6	0 17,6
	τ Pers.	16 18,5 [8]	-	15 20,0	0 32,5
	γ —	12 28,5 [8]	very windy	16 3,5	3 9,0
	α —	17 18,0 [8]	-	15 13,0	2 5,0
Nov. 16.	I came from Oxford.				
	9th Aurig.	19 12,0 [8]	st. -	16 22,0	2 24,0
	Capella	20 6,8 [9]	v. st. -	15 30,3	4 10,5

1728.		Rev. ″		Rev. ″	Rev. ″
Nov. 17.	ε Urs. M.	20 24,7	[8] st. -	16 21,7	4 3,0
	ζ ——	20 20,3	[9] v. st. -	16 0,0	4 20,3
	η ——	20 18,8	[10] v. g. -	15 04,1	5 14,7
	β Cass.	7 16,1	[8] st. -	9 29,6	2 13,5
	λ —	12 8,2	[8] st. -	9 9,2	2 33,0
	α —	10 12,0	[8] -	9 31,0	0 15,0
19.	η Urs. M.	15 20,9	[9] cloudy	10 5,4	5 15,5
	β Dracon.	4 30,7	[10] -	9 10,7	4 14,0
	γ ——	11 14,8	[10] -	8 33,8	2 15,0
	τ Pers.	9 3,0	[8] st. -	8 9,0	0 28,0
	γ —	5 3,5	[8] -	8 17,5	3 14,0
	α —	10 19,0	[4] cl. -	8 16,0	2 3,0
	δ —	7 18,6	[4] cl. -	7 26,6	0 8,0
	9th Aurig.	10 1,7	[8] -	7 12,7	2 23,0
	Capella	12 2,5	[8] -	7 26,5	4 10,0
22.	β Drac.	3 3,0	[9] -	7 16,0	4 13,0
A very clear frosty night, with a strong Aurora Borealis; stars movéd pretty steady.	β Cass.	7 21,8	[9] -	10 3,3	2 15,5
	λ —	13 32,7	[9] -	10 33,4	2 33,3
	α —	20 25,7	[9] -	20 11,7	0 14,0
	τ Pers.	11 9,7	[9] -	10 16,2	0 27,5
	γ —	7 32,3	[9] -	11 12,8	3 14,5
	α —	13 9,5	[9] -	11 8,0	2 1,5
	δ —	10 26,0	[8] -	11 0,0	0 8,0
Manè. 23.	β Urs. M.	0 10,5	[9] flutt. -	11 5,8	10 29,3
	ψ ———	12 26,5	[8] -	11 3,0	1 23,5
	χ ———	16 16,0	[6] flutt. m.	13 7,0	3 9,0
	γ ———	18 17,8	[8] flutt. m.	15 27,3	2 24,5
	ε ———	20 2,0	[9] flutt.	15 30,0	4 6,0
	ζ ———	19 3,5	[4] flutt. m.	14 13,0	4 24,5
	η. ———	21 32,8	[8] flutt. v. m.	16 15,8	5 17,0
	β Drac.	12 8,0	[8] -	16 21,0	4 13,0
	γ —	18 7,5	[9] -	15 26,0	2 15,5
	β Cass.	5 32,5	[9] st. -	8 13,2	2 14,7
	α —	11 7,0	[3] cl. -	10 28,0	0 13,0
	τ Pers.	11 24,0	[7] -	10 30,5	0 27,5
	γ —	7 2,3	[6] -	10 16,3	3 14,0
	α —	12 27,0	[8] st. -	10 24,5	2 2,5
	δ —	10 4,0	[3] cl. -	10 12,5	0 8,5
Manè. 24.	β Urs. M.	0 14,5	[8] st. -	11 10,0	10 29,5
	γ ——	14 14,5	[7] -	11 24,5	2 24,0

		Rev. ″		Rev. ″	Rev. ″
1728.	ε Urs. M.	18 18,8 [7]	-	14 14,3	4 4,5
	ζ ———	19 28,2 [8]	-	15 6,2	4 22,0
	η ———	20 19,0 [10]	-	15 2,5	5 16,5
Nov. 28.	β Drac.	10 12,5 [7]	-	14 24,0	4 11,5
	γ —	17 3,7 [9]	-	14 19,2	2 18,5
29.	γ —	11 18,7 [10]	-	9 0,4	2 18,3
	β Cass.	7 11,5 [6]	-	9 26,5	2 15,0
	λ —	12 6,7 [8]	-	9 8,4	2 32,3
	α —	9 31,7 [8] flutt.	-	9 17,2	0 14,5
30.	β —	7 14,3 [10]	-	9 29,8	2 15,5
	λ —	12 13,5 [7]	-	9 14,5	2 33,0
	α —	10 12,5 [6]	-	9 33,0	0 13,5
Dec. 1.	β —	7 33,0 [12] v. st.		10 14,0	2 15,0
	λ —	13 21,0 [9]	-	10 22,0	2 33,0
	α —	11 16,2 [8]	-	11 3,0	0 13,2
	τ Pers.	11 8,0 [8]	-	10 15,5	0 26,5
	γ —	6 19,2 [9]	-	10 2,0	3 16,8
	α —	11 22,7 [7]	-	9 22,0	2 0,7
	δ —	9 2,5 [8]	-	9 11,5	0 9,0
	Capella	12 24,7 [10]	-	8 16,2	4 8,5
4.	β Cass.	9 21,8 [8] st.	-	12 2,1	2 14,3
	λ —	14 25,0 [7]	-	11 26,0	2 33,0
	α —	13 16,0 [8]	-	13 2,5	0 13,5
Manè. 7.	β Urs. M.	0 15,5 [9] flutt.	-	11 10,0	10 28,5
	ψ ———	12 28,9 [8]	-	11 2,9	1 26,0
	γ ———	12 33,7 [9]	-	10 8,7	2 25,0
	ε ———	15 15,0 : : v. hazy		11 7,5	4 7,5
9.	β Cass.	8 19,7 [8]	-	11 0,7	2 15,0
	λ —	14 11,0 [8] st.	-	11 12,8	2 32,2
	α —	11 11,8 [10] st.	-	11 0,0	0 11,8
	γ Pers.	7 15,0 [10] st.	-	10 33,2	3 18,2
	α —	12 25,6 [9] st.	-	10 26,6	1 33,0
	δ —	9 31,8 [10] st.	-	10 8,8	0 11,0
	9th Aurig.	12 26,0 [7]	-	10 7,0	2 19,0
	Capella	14 6,8 [10] st.	-	10 0,5	4 6,3
Manè. 11.	ε Urs. M.	14 30,7 [10] v. st.		10 21,4	4 9,3
	ζ ———	16 4,7 [10]	-	11 11,7	4 27,0
	Alcore	9 26,2 [8]	-	11 11,7	1 19,5
	η Urs. Maj.	17 2,5 [9]	-	11 13,5	5 23,0
	β Cass.	8 12,0 [9] st.	-	10 27,5	2 15,5

		Rev. ″		Rev. ″	Rev. ″
1728.	α Cass.	10 29,0 [9]	-	10 16,2	0 12,8
	τ Pers.	10 28,0 [3]	: v. hazy	10 4,5	0 23,5
	γ —	6 27,5 [6]	hazy -	10 11,0	3 17,5
	α —	11 24,2 [8]	-	9 25,6	1 32,6
	δ —	9 7,8 [9]	-	9 19,0	0 11,2
Dec. 13.	β Cass.	7 6,5 [10]	-	9 21,0	2 14,5
	λ —	13 6,0 [8]	-	10 8,5	2 31,5
	α —	10 19,3 [8]	-	10 7,1	0 12,2
	γ Pers.	6 18,2 [8]	-	10 3,0	3 18,8
	α —	11 16,0 [8]	-	9 17,6	1 32,4
	δ —	8 28,3 [8]	cloudy	9 4,8	0 10,5
	9th Aurig.	11 12,6 [8]	-	8 28,6	2 18,0
	Capella	12 23,0 [4]	-	8 17,0	4 6,0
Manè. 14.	β Urs. M.	0 7,5	going out, clouds	11 5,0	10 29,0
15.	α Cass.	11 31,5 [8]	-	11 19,2	0 12,3
Manè. 16.	β Urs. M.	0 9,7 [4]	cloudy	11 4,7	10 29,0
	γ ——	13 5,0 [8]	cl. -	10 12,0	2 27,0
	ε ——	15 14,6 [9]	-	11 5,1	4 9,5
	η ——	15 18,8 [8]	-	9 30,2	5 22,6
	β Cass.	9 25,5 [8]	-	12 6,5	2 15,0
	α —	11 32,5 [9]	-	11 20,5	0 12,0
	Capella	16 12,5 [9]	st. -	12 7,5	4 5,0
17.	γ Drac.	10 1,5 [8]	-	7 11,3	2 24,2
19.	β —	2 28,7 [9]	-	6 33,2	4 4,5
	γ —	9 9,0 [9]	g. -	6 18,5	2 24,5
20.	γ —	9 8,3 [10]	-	6 17,0	2 25,3
21.	γ —	4 26,2	Dr. Halley obs.	2 1,2	2 25,0
	β Cass.	4 17,3 [8]	flutt. -	6 32,6	2 15,3
	λ —	9 22,5 [8]	flutt. -	6 25,0	2 31,5
	α —	10 0,8 [8]	flutt. m. error	7 20,5	2 14,3
	τ Pers.	7 6,7 [8]	-	6 18,2	0 22,5
	γ —	2 14,0 [8]	hazy -	5 33,0	3 19,0
	α —	9 25,0 [9]	flutt. -	7 28,5	1 30,5
23.	γ —	4 6,5 [7]	flutt. -	7 24,5	3 18,0
	α —	9 28,0 [3]	flutt. ext.	7 29,0	1 33,0
	Capella	11 15,0 [3]	flutt. ext.	7 13,5	4 1,5
24.	———	16 2,0 [8]	-	11 31,0	4 5,0
	δ Aurig.	11 8,5 [8]	-	11 6,5	0 2,0 south
	35 Camel.	13 1,5 [8]	-	11 6,5	1 29,0
Manè. 25.	β Urs. M.	0 15,0 [3]	cl. :: -	11 8,0	10 27,0

1729.		Rev. ″	Rev. ″	Rev. ″
Jan. 3.	τ Pers.	9 21,2 [6] flutt. -	8 33,6	0 21,6
	γ —	5 4,1 [8] -	8 24,9	3 20,8
	α —	10 26,0 [4] flutt. m.	8 30,0	1 30,0
Manè. 4.	γ Urs. M.	12 6,5 [6] flutt. -	9 12,5	2 28,0
14.	τ Pers.	10 15,0 [8] st. -	9 26,5	0 22,5
	γ —	6 21,7 [9] v. st. -	10 7,4	3 19,7
	α —	11 19,0 [9] v. st. -	9 22,6	1 30,4
	δ —	8 20,0 [8] -	9 0,0	0 14,0
Manè. 16.	ε Urs. Maj.	13 1,7 [10] v. st.	8 23,7	4 12,0
	ζ ———	13 6,7 [9] st. -	8 8,7	4 32,0
	η ———	14 18,0 [10] v. st.	8 23,2	5 28,8
19.	Capella	12 5,1 [11] v. st.	8 2,7	4 2,4
Manè. 20.	ε Urs. Maj.	13 1,0 [9] st. -	8 22,0	4 13,0
	ζ ———	13 32,0 [10] st. -	8 33,2	4 32,8
	Alcore	7 18,5 [7] -	8 33,2	1 14,7
	η Urs. Maj.	15 8,0 [10] -	9 12,5	5 29,5
	β Drac.	4 10,7 [7] cloudy	8 4,7	3 28,0
Manè. 21.	ε Urs. Maj.	15 12,0 [9] -	11 0,5	4 11,5
	ζ ——	16 13,5 : : cloudy	11 16,5	4 31,0
	η ——	22 7,5 [10] v. st.	16 12,8	5 28,7
Manè. 22.	ε ——	20 28,2 [8] -	16 16,2	4 12,0
	ζ ——	18 22,8 [8] st. -	13 24,8	4 32,0
	Alcore	12 9,5 [9] -	13 24,8	1 15,3
	η Urs. M.	19 0,5 [10] st. -	13 5,5	5 29,0
	β Cass.	11 6,0 [8] -	13 16,0	2 10,0
Manè. 25.	ε Urs. M.	18 30,8 [8] -	14 17,3	4 13,5
	ζ ——	19 2,0 [9] -	14 3,5	4 32,5
	Alcor.	12 22,8 [7] -	14 3,5	1 14,7
	η Urs. M.	20 2,1 [9] -	14 6,4	5 29,7
	β Drac.	10 0,8 [7] hazy -	13 28,3	3 27,5
	γ —	16 24,7 [4] v. hazy, too faint	13 22,7	3 2,0
	α Cass.	16 28,3 [10] g.	16 10,8	0 17,5
	τ Pers.	16 19,5 [8] -	15 31,0	0 22,5
	γ —	12 12,3 [8] -	15 32,8	3 20,5
	δ —	8 0,5 [8] -	8 14,0	0 13,5
	Capella	13 3,5 [9] st. -	9 1,5	4 2,0
Manè. 27.	ε Urs. M.	13 3,5 [8] -	8 25,0	4 12,5
	ζ ———	14 1,0 [8] flutt. -	9 3,0	4 32,0
	Alcor.	7 22,5 [6] -	9 3,0	1 14,5
	η Urs. M.	14 5,3 [9] -	8 10,3	5 29,0

1729.		Rev. "		Rev. "	Rev. "
Jan. 28.	β Drac.	3 32,5 [8]	-	7 25,0	3 26,5
	γ —	10 13,5 [8]	-	7 10,5	3 3,0
Feb. 1.	τ Pers.	7 25,3 [9]	v. g. -	7 1,5	0 23,8
	α —	7 22,4 [9]	v. g. -	5 26,2	1 30,2
	δ —	5 2,7 [9]	-	5 16,7	0 14,0
	Capella	9 4,7 [9]	-	5 3,7	4 1,0
5.	β Drac.	1 13,2 [9]	-	5 3,7	3 24,5
	γ —	7 26,5 [8]	-	4 21,2	3 5,3
6.	β —	1 29,5 [6]	-	5 20,0	3 24,5
	γ —	8 27,8 [10]	-	5 22,3	3 5,5
	Capella	9 26,0 [9]	st. -	5 24,8	4 1,2
7.	γ Drac.	7 30,8 [8]	flutt. hard frost	4 25,7	3 5,1
	Capella	8 30,0 [9]	v. st. -	4 28,2	4 1,8
12.	———	8 22,7 [9]	st. -	4 21,7	4 1,0
13.	γ Drac.	8 12,5 [4]	hazy & cloudy	5 6,0	3 6,5
14.	β —	1 6,7 [8]	-	4 30,2	3 23,5
	γ —	7 28,0 [7]	flutt. -	4 21,0	3 7,0
	Capella	9 25,5 [9]	st. -	5 23,8	4 1,7
15.	———	9 23,2 [6]	cl. -	5 22,5	4 0,7
21.	β Drac.	1 21,8 [8]	-	5 9,6	3 21,8
	γ —	7 27,9 [8]	-	4 19,9	3 8,0
24.	Capella	12 31,0 [6]	cl. -	8 30,7	4 0,3
25.	β Drac.	5 13,6 [8]	-	9 3,0	3 23,4
	ι Herculis	7 25,5 [6]	-	9 24,0	1 32,5
	γ Drac.	13 10,2 [6]	flutt. -	10 3,7	3 6,5
	α Cassiop.	11 6,3 [8]	-	10 16,0	0 24,3
	γ Pers.	6 14,5 [6]	faint -	9 30,5	3 16,0
	α —	11 9,0 [8]	-	9 9,0	2 0,0
	δ —	8 19,8 [6]	faint -	8 32,0	0 12,2 north
	Capella	14 16,6 [8]	st. -	10 15,3	4 1,3
	δ Aurig.	11 3,6 [8]	-	11 8,3	0 4,7 north
	35 Camel.	11 25,3 [7]	-	10 2,8	1 22,5
26.	β Drac.	7 15,3 [7]	-	11 4,8	3 23,5
	ι Herculis	9 2,8 [8]	-	11 0,6	1 31,8
	γ Drac.	14 16,0 [8]	-	11 9,5	3 6,5
	β Cassiop.	9 14,5 [6]	faint -	11 15,2	2 0,7
	α ———	12 13,7 [8]	-	11 24,7	0 23,0
	γ Pers.	8 9,5 [6]	faint -	11 26,0	2 16,5
	α —	13 30,7 [8]	-	11 30,7	2 0,0
	δ —	11 11,0 [4]	v. faint	11 23,5	0 12,5

1729.		Rev. ″		Rev. ″	Rev. ″
	Capella	14 25,0 [8]	-	10 23,5	4 1,5
	35 Camel.	14 6,5 [8]	-	12 17,5	1 23,0
	46 Aurigæ	12 17,7 [8]	-	12 9,7	0 8,0
Feb. 27.	β Drac.	9 13,4 [8]	-	13 2,6	3 23,2
	γ —	16 11,5 [8]	-	13 5,0	3 6,5
	Capella	16 9,3 [8]	-	12 8,0	4 1,3
28.	———	17 25,2 [8]	st. -	13 23,2	4 2,0
	δ Aurig.	13 21,7 [8]	-	13 26,2	0 4,5
	35 Camel.	15 8,0 [7]	faint -	13 19,2	1 22,8
	46 Aurig.	13 24,7 [7]	-	13 16,4	0 8,3
March 1.	γ Drac.	14 31,5 [8]	-	11 23,5	3 8,0
5.	γ ——	15 10,3 [8]	-	12 3,5	3 6,8
6.	β ——	7 26,9 [8]	-	11 15,4	3 22,5
	ι Herc.	9 1,2 [8]	-	10 33,7	1 32,5
	ξ Drac.	5 33,0 [6]	-	9 5,0	3 6,0
	γ —	8 6,0 [8]	-	4 32,5	3 7,5
9.	Capella	9 8,0 [8]	-	5 6,3	4 1,7
	δ Aurig.	6 12,5 [6]	flutt. -	6 17,0	0 4,5
	β Urs. Maj.	9 1,5 [7]	-	12 7,2	12 5,7
	γ ———	15 18,0 [7]	flutt. -	13 0,7	2 17,3
	ε ———	17 21,5 [6]	flutt. m.	13 16,8	4 4,7
10.	Capella	16 15,0 [8]	-	12 13,0	4 2,0
17.	β Drac.	7 16,0 [6]	-	11 5,3	3 23,3
	ι Her.	9 1,5 [8]	-	11 0,2	1 32,7
	ξ Drac.	7 15,0 [6]	-	10 21,2	3 6,2
	γ —	11 2,5 [8]	-	7 29,8	3 6,7
	β Cass.	5 26,0 [6]	faint -	7 21,5	1 29,5
	α —	8 15,5 [7]	v. windy	7 20,0	0 29,5
	Capella	11 28,0 [6]	flutt. ext. smoke	7 26,5	4 1,5
18.	β Drac.	3 16,5 [6]	flutt. -	7 5,5	3 23,0
	ι Herc.	5 7,5 [6]	flutt. -	7 7,5	2 0,0
	ξ Drac.	4 30,0 [6]	flutt. -	8 0,7	3 4,7
	γ —	6 31,7 [6]	flutt. -	3 23,7	3 8,0
	β Cass.	1 31,8 [6]	faint -	3 28,3	1 30,5
	α —	6 33,0 [4]	-	6 1,5	0 31,5
20.	β Drac.	4 10,0 [3]	: -	8 0,5	3 24,5
	ι Herc.	5 22,5 [3]	: -	7 25,0	2 2,5
	ξ Drac.	5 5,0 [3]	-	8 13,0	3 8,0
	γ —	7 15,3 [8]	-	4 9,3	3 6,0
24.	β —	0 4,0 [6]	-	3 29,5	3 25,5

1729.		Rev. ″		Rev. ″	Rev. ″
March 25.	Capella	10 11,8 [9]	-	6 8,8	4 3,0
April 4.	ε Urs. M.	10 14,0 [6]	-	6 15,5	3 32,5
	ζ ——	11 20,5 [8]	flutt. -	7 0,5	4 20,0
	Alcore	5 8,5 [6]	-	7 0,5	1 26,0
	η Urs. M.	12 4,0 [4]	flutt. m.	6 20,0	5 18,0
5.	ε ——	12 7,7 [8]	st. -	8 8,7	3 33,0
	ζ ——	13 2,6 [8]	-	8 18,1	4 18,5
	Alcore	6 23,0 [6]	flutt. m.	8 18,0	1 29,0
	η Urs. M.	13 22,8 [8]	flutt. -	8 5,8	5 17,0
8.	ξ ——	12 30,0 [6]	flutt. -	8 11,0	4 19,0
	η ——	13 31,0 [4]	: : cloudy	8 14,0	5 17,0
9.	β Cass.	5 32,0 [8]	-	7 23,0	1 25,0
	α —	8 22,0 [4]	cloudy	7 23,0	0 33,0
12.	ζ Urs. M.	15 31,5 [8]	-	11 13,3	4 18,2
	Alcore	9 17,7 [8]	-	11 13,3	1 29,6
	η Urs. M.	16 19,3 [8]	-	11 4,5	5 14,8
13.	ζ ——	14 12,0 [8]	-	9 29,5	4 16,5
	Alcore	7 33,2 [8]	-	9 29,5	1 30,3
	η Urs. M.	15 12,2 [8]	-	9 31,0	5 15,2
16.	γ ——	9 28,0 [7]	flutt. -	7 17,7	2 10,3
	ε ——	12 1,2 [8]	flutt. -	8 5,4	3 29,8
	η ——	13 22,0 [8]	flutt. -	8 8,5	5 13,5
	β Drac.	4 1,0 [8]	flutt. -	7 30,0	3 29,0
	ι Hercl.	5 12,3 [7]	flutt. -	7 16,0	2 3,7
	ξ Drac.	6 27,3 [7]	flutt. -	10 3,0	3 9,7
	γ —	9 4,0 [8]	flutt. -	6 0,5	3 3,5
18.	Capella	10 7,7 [8]	-	6 0,7	4 7,0

April 21, I set out for Oxford. May 10th, I returned to Wansted.

May 11.	β Urs. M.	0 12,0 [9]	st. -	11 29,0	11 17,0
	γ ——	14 12,2 [8]	st. -	12 7,2	2 5,0
	ε ——	16 3,5 [8]	-	12 15,0	3 22,5
	ζ ——	15 28,7 [6]	flutt. -	11 18,8	4 9,9
	Alcore	9 16,7 [6]	-	11 18,8	2 2,1
	η Urs. M.	17 13,0 [8]	flutt. -	12 4,2	5 8,8
13.	γ ——	18 11,3 [9]	-	16 6,5	2 4,8
14.	Capella	20 3,5 [8]	-	15 27,5	4 10,0
17.	ζ Urs. M.	19 28,5 [6]	flutt. -	15 20,7	4 7,8
	Alcore	13 16,2 [6]	flutt. -	15 20,7	2 4,5
26.	Capella	13 31,7 [7]	flutt. -	9 19,3	4 12,4

1729.		Rev. ″		Rev. ″	Rev. ″
May 29.	β Drac.	6 15,5 [8]	-	10 23,2	4 7,7
	γ —	13 17,5 [7]	flutt. m.	10 27,2	2 24,3
30.	β Urs. M.	0 19,3 [6]	v. faint	12 2,3	11 17,0
	γ ——	13 33,2 [8]	st. -	11 28,7	2 4,5
	ε ——	16 28,5 [8]	-	13 7,3	3 21,2
	ζ ——	16 16,0 [8]	-	12 10,0	4 6,0
	Alcore	10 3,0 [5]	faint -	12 10,0	2 7,0
	η —	16 22,2 [8]	-	11 18,7	5 3,5
31.	Capella	10 25,0 [6]	flutt. m.	6 12,0	4 13,0
	β Urs. M.	0 15,0	v. faint -	11 32,0	11 17,0
	η ——	16 11,3 [8]	-	11 7,8	5 3,5
	β Drac.	6 19,6 [8]	-	10 26,3	4 6,7
	γ —	13 1,5 [6]	flutt. m.	10 11,0	2 23,5
June 2.	β —	5 5,8 [6]	flutt.	9 12,4	4 6,6
	γ —	12 14,3 [6]	flutt. m.	9 25,3	2 23,0
3.	β Cassiop.	8 16,5 [8]	-	10 7,0	1 24,5
	α ———	11 1,2 [9]	-	9 33,5	1 1,7
	Capella	13 9,7 [6]	clear, windy	8 31,0	4 12,7
4.	β Cass.	6 27,5 [8]	-	8 17,0	1 23,5
	α —	11 2,7 [8]	-	10 0,0	1 2,7
	Capella	14 33,7 [4]	v. cl. haz.	10 20,5	4 13,2
	β Urs. M.	0 16,7 [8]	-	11 31,7	11 15,0
	γ ——	13 28,5 [8]	-	11 23,0	2 5,5
	ε ——	14 24,5 [8]	v. st.	11 3,2	3 21,2
	ζ ——	15 11,2 [3]	v. haz.	11 4,7	4 6,5
	η ——	15 33,5 [10]	-	10 29,8	5 3,7
5.	β Cass.	9 4,0 [8]	-	10 27,4	1 23,4
	α —	12 19,5 [9]	windy	11 17,0	1 2,5
	β Drac.	7 25,5 [8]	st. -	12 1,0	4 9,5
	ι Herc.	9 3,3 [8]	-	11 21,8	2 18,5
	ξ Drac.	8 2,0 [6]	-	11 27,3	3 25,3
	γ —	10 4,5 [8]	-	7 17,0	2 21,5
6.	β Cass.	7 3,7 [8]	-	8 26,7	1 23,0
	α —	9 16,6 [8]	-	8 15,0	1 1,6
	Capella	13 1,8 [8]	-	8 22,3	4 13,5
	γ Urs. M.	10 30,7 [8]	-	8 25,2	2 5,5
	η ——	16 16,2 [8]	-	11 12,4	5 3,8
	γ Drac.	13 24,3 [8]	flutt. m.	11 2,1	2 22,2
7.	β Cass.	10 6,0 [8]	-	11 29,7	1 23,7
	α —	10 30,7 [8]	-	9 28,2	1 2,5

1729.		Rev. "		Rev. "	Rev. "
	Capella	14 5,0	[3] cloudy	9 25,0	4 14,0
	γ Urs. M.	17 5,0	[6] faint -	15 0,0	2 5,0
	ε ——	20 25,7	[8] -	17 4,0	3 21,7
	ζ ——	19 15,2	[8] -	15 9,7	4 5,5
	η ——	19 28,5	[10] g. -	14 25,2	5 3,3
	β Drac.	7 16,2	[8] -	11 25,4	4 9,2
	ι Herc.	8 30,5	[8] flutt. -	11 17,0	2 20,5
	ξ Drac.	25 8,5	[7] -	28 33,5	3 25,0
	γ —	24 3,7	[8] -	21 16,7	2 21,0
June 8.	β Cass.	12 10,0	[6] faint -	13 33,0	1 23,0
	α —	7 8,2	[7] faint -	6 5,2	1 3,0
9.	γ Drac.	8 25,0	[8] -	6 3,0	2 22,0
10.	β Urs. M.	0 21,0	[3] too faint	12 1,0	11 14,0
	γ ——	14 25,5	[6] faint -	12 20,2	2 5,3
11.	β Cass.	12 25,5	[8] -	14 16,5	1 25,0
	α —	15 16,5	[8] -	14 15,0	1 1,5
18.	ε Urs. M.	8 11,2	[10] -	4 25,4	3 19,8
20.	Capella	11 19,0	[8] flutt. -	7 4,5	4 14,5
	γ Drac.	9 15,8	[8] st. -	6 33,8	2 16,0
23.	α Pers.	6 11,5	[8] -	3 31,3	2 14,2
	Capella	6 9,3	[8] -	1 29,1	4 14,2
	β Urs. M.	0 19,5	[6] v. faint	11 32,8	11 13,3
	γ ——	14 14,5	[6] v. faint	12 8,0	2 6,5
	ε ——	15 31,5	[6] hazy -	12 11,5	3 20,0
	ζ ——	15 19,5	[8] -	11 15,5	4 4,0
	η ——	16 20,7	[9] -	11 19,2	5 1,5
24.	β Cass.	10 6,7	[8] -	11 32,2	1 25,5
	α —	13 26,0	[8] -	12 27,0	0 33,0
	α Pers.	12 16,5	[8] -	10 3,0	2 13,5
	Capella	14 17,8	[9] -	10 3,8	4 14,0
	γ Drac.	12 3,3	[9] flutt. -	9 20,8	2 16,5
25.	ζ Urs. M.	13 11,3	[8] -	9 7,0	4 4,3
	η ——	14 19,5	[8] -	9 17,0	5 2,5
26.	Capella	15 17,0	[8] -	11 2,0	4 15,0
	ζ Urs. M.	16 17,5	Dr. Hoadley observed this	12 11,5	4 6,0
27.	γ Pers.	7 2,0	v. dub. -	10 2,0	3 0,0
	α —	9 20,0	[8] -	7 6,3	2 13,7
28.	ε Urs. M.	10 4,5	[6] cl. -	6 17,0	3 21,5
29.	γ Pers.	7 9,5	v. dub. -	10 11,0	3 1,5
	α —	9 30,0	[8] -	7 15,0	2 15,0

		Rev. ″		Rev. ″	Rev. ″
1729.	q. Capella	12 32,5 [8]	-	7 20,0	5 12,5
	ζ Urs. M.	12 12,5 [8]	-	8 8,2	4 4,3
	η ——	12 31,0 [6]	haz. -	7 30,5	5 0,5
June 30.	γ Pers.	5 29,0 [4]	faint -	8 29,0	3 0,0
	α —	12 7,7 [8]	-	9 28,0	2 13,7
	Capella	14 10,0 [8]	-	9 28,0	4 16,0
	γ Urs. M.	13 21,0 [6]	faint -	11 14,3	2 6,7
	ε ——	15 18,7 [10]	-	11 33,2	3 19,5
	ζ ——	17 8,7 [10]	-	13 4,0	4 4,7
	η ——	17 31,0 [12]	-	12 29,3	5 1,7
	er. β Dracon.	7 30,8 [8]	-	12 10,4	4 13,6
	ι Hercul.	9 2,5 [8]	-	11 32,0	2 29,5
	ξ Drac.	7 25,0 [7]	-	11 22,0	3 31,0
July 1.	α Pers.	12 12,8 [8]	-	9 32,8	2 14,0
	Capella	15 17,0 [8]	cl. -	11 2,0	4 15,0
	β Drac.	29 17,3 [8]	-	3 33,3	4 16,0
	γ —	6 15,5 [6]	flutt. -	4 2,0	2 13,5
4.	ε Urs. M.	7 19,3 [8]	-	4 0,0	3 19,3
	ζ ——	8 9,3 [6]	-	4 5,3	4 4,0
5.	ε ——	17 19,7 [8]	-	13 33,7	3 20,0
	ζ ——	17 7,5 [10]	I could see the small star.	13 3,5	4 4,0
	η ——	18 18,0 [10]	-	13 16,0	5 2,0
10.	β Drac.	8 14,7 [8]	-	13 0,3	4 19,6
	ι Herc.	9 16,0 [6]	-	12 11,0	2 29,0
	ξ Drac.	8 7,5 [8]	-	12 10,7	4 3,2
	γ —	10 0,5 [9]	-	7 23,5	2 11,0
12.	γ Pers.	3 8,0 [6]	haz. -	7 10,5	4 2,5
	α —	9 7,8 [8]	-	6 28,6	2 13,2
	δ —	6 20,0 : :	v. faint	6 19,5	0 0,5
	Capella	10 27,5 [10]	-	6 10,0	4 17,5
14.	———	10 33,3 [8]	-	6 15,8	4 17,5
18.	ε Urs. Maj.	9 32,0 [6]	flutt. haz.	6 10,0	3 22,0
	ζ ———	9 24,5 [6]	haz. -	5 19,0	4 5,5
21.	Capella	14 14,0 [8]	-	9 31,3	4 16,7
	ε Urs. M.	14 5,2 [8]	-	10 16,2	3 23,0
	ζ ——	14 21,0 [6]	cl. -	10 15,0	4 6,0
	η ——	14 33,0 [6]	cl. -	9 30,7	5 2,3
27.	β Drac.	5 25,2 [8]	st. -	10 14,4	4 23,2
	ι Herc.	6 32,5 [8]	-	9 29,8	2 31,3
	ξ Drac.	5 7,7 [8]	-	9 15,0	4 7,3

		Rev. ″			Rev. ″	Rev. ″
1729.	γ Drac.	6 1,5 [8]		-	3 28,7	2 6,8
July 28.	β —	0 16,0 [6]		-	5 5,0	4 23,0
	ι Herc.	2 8,5 [8]		-	5 6,0	2 31,5
	ξ Drac.	3 26,0 [6]		-	7 32,5	4 6,5
	γ —	5 24,5 [7]	flutt.	-	3 17,5	2 7,0
29.	β —	2 14,2 [8]		-	7 4,2	4 24,0
	ι Herc.	4 21,2 [6]		-	7 19,7	2 32,5
	ξ Drac.	4 0,0 [5]		-	8 6,5	4 06,5
	γ —	5 21,7 [8]		-	3 14,2	2 7,5
30.	β —	2 7,8 [8]		-	6 31,8	4 24,0
	ι Herc.	4 29,0 [8]		-	7 27,5	2 32,5
Aug. 5.	β Drac.	2 2,6 [8]	twilight		6 26,3	4 23,7
	ι Herc.	5 5,5 [8]		-	8 5,5	3 0,0
	ξ Drac.	6 13,5 [8]		-	10 21,8	4 8,3
q.	γ —	9 25,7 [8]	st.	-	7 18,2	2 7,5
6.	β —	12 30,5 [8]		-	17 21,0	4 24,5
	ι Herc.	15 4,2 [8]		-	18 4,7	3 0,5
	ξ Drac.	13 27,5 [4]		-	18 3,0	4 9,5
	γ —	16 17,0 [8]		-	14 13,5	2 3,5
7.	γ Persei	20 11,8 [7]		-	23 18,8	3 7,0
	α ——	16 23,7 [7]		-	14 15,7	2 8,0
	Capella	14 27,5 [8]		-	10 11,0	4 16,5
10.	β Drac.	5 10,0 [6]	cl.	-	10 3,0	4 27,0
	ι Herc.	7 19,0 [6]		-	10 18,2	2 33,2
	ξ Drac.	5 25,0 [4]	cl.	-	10 1,0	4 10,0
	γ —	6 17,5 [8]		-	4 14,0	2 3,5
11.	Capella	8 24,0 [8]		-	4 9,2	4 14,8
12.	β Drac.	2 12,5 [6]		-	7 3,5	4 25,0
	ι Hercul.	4 18,5 [4]	clouds		7 20,0	3 1,5
	γ Drac.	13 6,3 [8]	flutt.	-	11 2,3	2 4,0
13.	Capella	15 19,0 [3]	cl.	-	11 4,5	4 14,5
	β Drac.	9 1,0 [8]		-	13 26,8	4 25,8
	ι Herc.	11 0,5 [8]		-	14 1,0	3 0,5
	ξ Drac.	10 01,0 [8]		-	14 11,0	4 10,0
	γ —	11 31,5 [8]		-	9 27,3	2 4,2
Manè. 15.	τ Pers.	11 17,7 [8]	st.	-	10 16,2	1 1,5
	γ —	7 13,2 [8]		-	10 19,7	3 6,5
	α —	12 30,2 [9]		-	10 21,4	2 8,8
	δ —	10 22,7 [8]		-	10 25,4	0 2,7 north
17.	β Drac.	6 31,5 [10]	cl.	-	11 23,5	4 26,0

1729.		Rev. ″		Rev. ″	Rev. ″
	ι Herc.	7 30,8 [9]	-	10 30,8	3 0,0
	ξ Drac.	7 24,0 [6]	-	11 1,8	4 11,8
	γ —	7 24,2 [8]	-	5 20,2	2 4,0
Aug. 19.	τ Pers.	6 31,0 : :	flutt. ext.	5 29,0	1 2,0
	γ —	2 12,5 : :	flutt. ext.	5 14,5	3 2,0

The stars fluttered so much this morning (the weather being foggy) that I could not bisect them with any certainty.

	Capella	9 23,5 : :	flutt. ext.	5 12,0	4 11,5
	ι Herc.	3 14,3 [8]	-	6 15,0	3 0,7
	ξ Drac.	2 5,7 [8]	-	6 16,0	4 10,3
	γ —	3 10,7 [8]	-	1 8,0	2 2,7
Manè. 20.	τ Pers.	3 31,0 [6]	flutt. -	2 31,0	1 0,0
	γ —	2 27,5 [4]	flutt. m.	5 32,0	3 4,5
	α —	10 6,0 [6]	flutt. m.	8 1,0	2 5,0
	Capella	12 12,0 [8]	flutt.	7 30,0	4 16,0
21.	τ Pers.	10 5,5 [8]	-	9 3,5	1 2,0
	γ —	6 25,0 [8]	flutt. -	9 31,0	3 6,0
	α —	11 31,5 [6]	flutt. -	9 23,5	2 8,0
	ε Urs. Maj.	13 6,0 [6]	faint	9 12,0	3 28,0
22.	Capella	15 13,8 [8]	st. -	10 33,3	4 14,5
23.	β Drac.	6 9,0	Dr. Hoadley observed.	11 0,7	4 25,7
	γ —	10 32,7		10 31,2	2 1,0
24.	Capella	15 22,3 [8]	-	11 6,0	4 16,3
25.	β Drac.	4 1,2 [7]	hazy -	8 27,9	4 26,7
	γ —	10 20,5 [8]	st. -	8 18,0	2 2,5
28.	β —	3 8,7 [8]	st. -	8 1,5	4 26,8
	ι Herc.	4 18,0 [8]	st. -	7 18,5	3 0,5
	ξ Drac.	3 10,8 [8]	-	7 23,3	4 12,5
	γ —	4 18,3 [9]	-	2 16,3	2 2,0
29.	β —	2 22,5 [11]	v. g.	7 15,0	4 26,5
	ι Herc.	5 19,5 [8]	-	8 18,2	2 32,7
	ξ Drac.	3 26,0 [6]	-	8 5,0	4 13,0
	γ —	5 1,8 [8]	-	2 33,6	2 2,2

30. This morning as I was moving the telescope to set it for Capella, the wire broke just at the notch at top; I afterwards fixed on the same wire again, it being long enough without piecing. I had used this wire almost twelve months without breaking, for it was put on Sept. 10, 1728.

	d Drac.	5 31,5 [8]	-	11 0,0	5 2,5
	c —	15 6,5 [6]	-	12 14,3	2 26,2

1729.		Rev. ″		Rev. ″	Rev. ″
	ϰ Cygni	13 9,0 [6]	-	12 29,5	0 13,5
	ι —	8 14,5 [8]	-	11 25,0	3 10,5
Aug. 31.	Capella	14 1,0 [8]	-	9 21,5	4 13,5
	ξ Drac.	5 31,0 [6]	-	10 7,2	4 10,2
	γ —	7 3,2 [8]	st. -	5 0,7	2 2,5
	c —	7 29,5 [8]	-	5 3,5	2 26,0
	ϰ Cygni	5 19,2 [8]	-	5 4,7	0 14,5
	ι —	1 18,0 [6]	flutt. -	4 27,5	3 9,5
Sept. 1.	Capella	9 14,3 [8]	-	5 2,8	4 11,5
2.	———	9 18,5 [6]	cl. -	5 5,3	4 13,2
	β Drac.	3 21,7 [8]	-	8 14,0	4 26,3
	γ —	7 5,7 [6]	clouds	5 3,2	2 2,5
3.	Capella	9 7,0 [6]	clouds	4 28,5	4 12,5
8.	β Drac.	29 33,5 [8]	hazy -	4 25,3	4 25,8
	γ —	7 12,5 [8]	st. -	5 10,3	2 2,2
9.	Capella	9 31,0 [8]	st. E. w. strong	5 17,3	4 13,7
	β Drac.	5 13,7 [8]	v. windy	10 6,4	4 26,7
	γ —	12 13,2 [9]	-	10 11,0	2 2,2
10.	η Urs. M.	18 31,3 [8]	-	13 20,3	5 11,0
13.	β Drac.	3 1,7 [8]	-	7 29,0	4 27,3
	γ —	10 18,0 [8]	-	8 16,4	2 1,6
18.	γ —	10 21,5 [8]	-	8 19,7	2 1,8
19.	Capella	13 10,5 [8]	st. -	8 31,5	4 13,0
	18 Camel.	8 11,7 [8]	-	9 18,7	1 7,0
	δ Aurig.	9 33,0 [8]	flutt. -	9 13,0	0 20,0
	35 Camel.	11 30,5 [7]	too faint	9 21,0	2 9,5
20.	δ Aurig.	11 27,8 [8]	st. -	11 10,8	0 17,0
	35 Camel.	13 4,6 [8]	-	10 30,6	2 8,0
22.	η Urs. Maj.	23 19,5 [6]	v. windy	18 04,2	5 15,3
24.	δ Aurig.	16 27,5 [8]	cl. -	16 11,0	0 16,5
	35 Camel.	15 20,0 [4]	cl. -	13 12,7	2 7,3
26.	β Drac.	7 29,7 [9]	-	12 20,4	4 24,7
	γ —	10 12,0 [10]	v. st.	8 9,5	2 2,5
27.	α Cass.	7 6,5 [8]	st. -	7 1,0	0 5,5 south
29.	Capella	10 21,7 [7]	flutt. foggy	6 14,0	4 7,7
	18 Camel.	5 11,2 [6]	-	6 17,2	1 6,0
	δ Aurig.	6 30,0 [6]	flutt. -	6 11,0	0 19,0
	35 Camel.	8 9,0 [7]	-	6 1,0	2 8,0
Octob. 4.	β Drac.	0 17,7 [8]	-	5 8,5	4 24,8
	γ —	6 9,5 [8]	-	4 6,0	2 3,5

1729.		Rev. "				Rev. "	Rev. "	
Oct. 7.	η Urs. Maj.	11 1,5	[8]		-	5 15,3	5 20,2	
	β Drac.	1 12,5	[8]		-	6 2,5	4 24,0	
	γ —	7 9,8	[8]	v. st.	-	5 6,5	2 3,3	
8.	Capella	9 29,5	[8]	st.	-	5 19,0	4 10,5	
	18 Camel.	5 7,5	[8]		-	6 16,0	1 8,5	
	δ Aurig.	7 15,5	[8]		-	7 0,5	0 15,0	
	35 Camel.	8 32,5	[8]		-	6 26,0	2 6,5	
10.	β Drac.	0 29,0	[8]		-	5 18,0	4 23,0	
	β Cassiop.	2 30,2	[8]		-	5 22,2	2 26,0	
13.	ε Urs. Maj.	9 28,0	[8]		-	5 17,0	4 11,0	
	ζ ———	9 30,5	[8]		-	5 3,5	4 27,0	
	η ———	11 25,3	[9]		-	6 4,0	5 21,3	
14.	ε ———	11 4,0	[8]		-	6 26,2	4 11,8	
	η ———	15 4,0	[9]		-	9 15,5	5 22,5	
	β Drac.	4 2,5	[8]		-	8 25,5	4 23,0	
	β Cass.	6 30,7	[7]	flutt.	-	9 23,7	2 27,0	
	α —	9 23,5	[6]	flutt. m.		9 24,5	0 1,0	north
15.	β —	7 2,7	[8]		-	9 31,7	2 29,0	
	α —	10 13,0	[7]		-	10 14,0	0 1,0	north
17.	β —	7 8,0	[9]	st.	-	10 3,8	2 29,8	
	α —	9 33,5	[8]		-	10 0,2	0 00,7	north
18.	β Drac.	5 20,5	[8]		-	10 8,5	4 22,0	
	γ —	12 7,0	[8]		-	10 2,0	2 5,0	
20.	Capella	14 1,0	[8]	st.	-	9 25,0	4 10,0	
24.	ε Urs. Maj.	14 17,2	[8]		-	10 1,2	4 16,0	
	η ———	13 1,5	[6]	cl.	-	7 10,0	5 25,5	
	γ Drac.	9 16,5	[9]		-	7 9,5	2 7,0	

Oct. 27th, I went to Oxford, and returned to Wansted, Nov. 11.

Nov. 21.	γ Drac.	13 10,2	[6]		-	10 29,7	2 14,5
23.	β Cassiop.	7 31,8	[8]	st.	-	11 0,3	3 2,5
	α ———	10 6,5	[4]	cl.	-	10 15,5	0 9,0
25.	Capella	15 6,5	[8]	st.	-	11 0,0	4 6,5
26.	β Drac.	6 30,0	[8]		-	11 5,5	4 9,5
	γ —	13 23,0	[4]	haz. faint		11 7,0	2 16,0
28.	γ —	10 14,5	[10]	very bright, windy		7 31,5	2 17,0
	β Cass.	5 0,5	[8]	v. st.	-	8 3,7	3 3,2
	λ —	11 3,7	[8]		-	8 27,0	2 10,7
	α —	10 2,5	[9]	v. st.	-	10 10,7	0 8,2
	τ Pers.	10 10,2	[7]	st.	-	10 0,2	0 10,0
	γ —	6 7,0	[8]	st.	-	10 4,0	3 31,0

1729.		Rev. ″		Rev. ″	Rev. ″
	α Pers.	12 6,5	[9] st. -	10 18,5	1 22,0
Nov. 30.	γ Drac.	10 22,7	[10] v. bright	8 5,4	2 17,3
Dec. 2.	λ Cass.	10 14,0	[8] st. -	8 3,5 q.	2 10,5
	α —	9 29,7	[8] -	10 5,0	0 9,3
	τ Pers.	8 13,7	[8] -	8 4,5	0 9,2
	γ —	3 33,5	[8] -	7 32,0	3 32,5
	α —	9 27,0	[8] st. -	8 6,0	1 21,0
	Capella	12 0,7	[8] st. -	7 30,5	4 4,2
3.	β Drac.	3 7,8	[8] -	7 16,3	4 8,5
	γ —	9 20,7	[8] -	7 2,7	2 18,0
	β Cass.	4 12,5	[8] -	7 15,5	3 03,0
	λ —	9 18,5	[4] hazy -	7 10,3	2 8,2
	α —	10 22,7	[8] st. -	10 32,2	0 9,5
	ξ Androm.	5 28,0	[6] -	11 3,0	5 9,0
	τ Pers.	11 21,0	[4] haz. -	11 12,3	0 8,7
	γ —	7 3,5	[6] -	11 2,0	3 32,5
	α —	11 9,5	[3] cl. -	9 23,5	1 20,0
5.	λ Cass.	12 25,0	[7] st. -	12 13,5	0 11,5
	α —	12 4,5	[9] v. g. -	12 13,0	0 8,5
6.	β Drac.	8 30,5	[3] haz. faint	13 4,0	4 7,5
	γ —	15 18,2	[8] windy	12 32,7	2 19,5
10.	γ —	12 23,5	[8] -	10 3,0	2 20,5
	β Cass.	7 29,5	[8] st. -	10 33,0	3 3,5
	λ —	13 28,5	[8] -	11 18,0	2 10,5
	α —	11 15,5	[8] st. -	11 26,3	0 10,8
	ξ Androm.	6 8,0	[8] -	11 18,0	5 10,0
	τ Pers.	8 29,0	[8] -	8 20,0	0 9,0
error.	γ —	13 6,0	[8] -	8 8,3	4 31,7
	α —	9 16,0	[8] -	7 31,3	1 18,7
	δ —	7 22,5	[8] -	8 11,8	0 23,3
	Capella	11 0,5	[8] v. st. -	6 31,8	4 2,7
	18 Camel.	6 0,5	[6] -	7 20,0	1 19,5
	δ Aurig.	8 19,5	[8] -	8 14,0	0 5,5
	35 Camel.	9 33,5	[9] -	8 1,3	1 32,2
	46 Aurig.	8 8,7	[9] -	7 23,0	0 19,7
11.	β Cass.	4 3,3	[8] -	7 8,0	3 4,7
	λ —	10 29,3	[8] -	8 19,0	2 10,3
	α —	10 29,5	[7] -	11 5,5	0 10,0
	ξ Andr.	5 29,5	[7] -	11 6,3	5 10,8
15.	γ Drac.	9 27,5	[7] -	7 5,0	2 22,5

I went to Oxford in December.

1730.		Rev. ″		Rev. ″	Rev. ″
Jan. 31.	τ Pers.	8 32,7 [8]	-	8 24,7	0 8,0
	γ —	5 6,7 [8]	-	9 7,2	4 0,5
	α —	10 13,5 [8]	-	8 30,0	1 17,5
	δ —	7 31,0 [4] cl.	-	8 21,8	0 24,8
	Capella	12 19,0 [8]	-	8 19,7	3 33,3
Feb. 21.	———	12 10,0 [7] st.	-	8 12,5	3 31,5
24.	———	12 13,8 [8]	-	8 15,8	3 32,0
Mar. 3.	———	12 11,7 [8] v. st.	-	8 13,5	3 32,2
	δ Aurig.	9 27,0 [8]	-	9 30,5	0 3,5
	35 Camel.	11 26,7 [9]	-	10 2,3	1 24,4
10.	β Drac.	8 7,8 [8]	-	11 29,6	3 21,8
	ι Hercul.	9 31,6 [8]	-	11 29,5	1 32,0
	ξ Drac.	8 17,0 [7]	-	11 25,5	3 8,5
	γ —	10 17,6 [8]	-	7 12,3	3 5,3
16.	β —	3 14,0 [2]	-	7 3,0	3 23,0
	ι Hercul.	4 27,8 [4]	-	6 25,5	1 31,7
	ξ Drac.	4 2,8 [3] cl.	-	7 11,3	3 8,5
	γ —	7 22,5 [6] cl.	-	4 16,5	3 6,0
18.	β —	5 14,5 [8]	-	9 2,7	3 22,2
	ι Hercul.	7 15,8 [8]	-	9 14,8	1 33,0
	ξ Drac.	5 18,0 [8]	-	8 27,0	3 9,0
	γ —	8 13,5 [8]	-	5 8,3	3 5,2
April 3d, I set out for Oxford. Returned May 23d.					
May 25.	Capella	11 7,5 [9]	-	7 1,2	4 6,3
26.	β Urs. M.	1 7,0 [4] faint cloud:		12 1,5	10 28,5
27.	Capella	17 0,5 [8]	-	12 25,8	4 8,7
June 6.	———	17 3,0 [6] flutt.	-	12 27,0	4 10,0
	β Drac.	8 15,0 [8]	-	12 24,5	4 9,5
	ι Hercul.	10 12,0 [8]	-	12 30,0	2 18,0
	ξ Drac.	9 15,0 [7]	-	13 9,5	3 28,5
	γ —	11 4,5 [7] flutt.	-	8 18,8	2 19,7
7.	β Cassiop.	6 5,2 [7]	-	8 16,7	2 11,5
	α ———	8 9,5 [8]	-	7 29,5	0 14,0
	β Drac.	3 24,0 [8] st.	-	7 33,0	4 9,0
	ι Herc.	6 3,0 [8]	-	8 20,2	2 17,2
	ξ Drac.	5 19,5 [8]	-	9 13,5	3 28,0
	γ —	7 21,5 [8]	-	5 2,0	2 19,5
11.	β Cass.	4 7,0 [7]	-	6 19,0	2 12,0
	α —	7 3,8 [8]	-	6 23,8	0 14,0
17.	ε Urs. M.	12 2,0 [5] cl.	-	7 29,0	4 7,0

1730.		Rev. ″		Rev. ″	Rev. ″
June 19.	β Cass.	11 8,5 [8]	-	13 20,0	2 11,5
	α —	10 17,0 [8]	-	10 3,7	0 13,3
	Capella	12 30,0 [8]	-	8 18,0	4 12,0
21.	γ Urs. Maj.	12 24,5 [7]	-	9 31,0	2 27,5
July 14.	Capella	13 0,7 [8]	-	8 21,4	4 13,3
15.	α Pers.	9 25,2 [8]	-	7 24,7	2 0,5
	δ —	7 24,5 [7]	faint -	8 0,0	0 9,5
	Capella	12 7,5 [8]	-	7 29,0	4 12,5
17.	δ Pers.	5 10,0 [8]	-	5 21,0	0 11,0
	Capella	7 5,5 [9]	-	2 25,3	4 14,2
	ζ Urs. Maj.	11 3,0 [5]	hazy -	6 10,0	4 27,0
	η ———	11 6,0 [8]	hazy -	5 18,3	5 21,7
19.	η ———	11 5,3 [11]	-	5 17,6	5 21,7
24.	ε ———	12 5,5 [8]	-	7 28,5	4 11,0
	ζ ———	12 13,5 [6]	haz. -	7 20,0	4 27,5
	η ———	13 27,0 [8]	-	8 6,0	5 21,0
25.	Capella	8 17,2 [8]	-	4 4,2	4 13,0
Aug. 5.	———	8 19,8 [8]	-	4 6,8	4 13,0
	ζ Urs. Maj.	9 3,5 [8]	-	4 8,5	4 29,0
	η ———	10 1,5 [8]	-	4 12,3	5 23,2
6.	Capella	8 25,3 [8]	-	4 12,8	4 12,5
9.	———	8 23,5 [7]	haz. -	4 11,0	4 12,5
12.	———	9 24,5 [6]	too bright	5 13,0	4 11,5
	η Urs. Maj.	12 5,0 [8]	-	6 14,0	5 25,0
13.	Capella	7 18,5 [10]	small aper.	3 5,5	4 13,0
16.	———	12 8,0 [9]	-	7 30,0	4 12,0
17.	γ Drac.	9 1,0 [7]	-	6 33,0	2 2,0
18.	Capella	10 25,5 [7]	flutt. -	6 14,5	4 11,0
	ε Urs. M.	11 23,5 [6]	flutt. m.	7 7,5	4 16,0
	η ——	14 10,0 [7]	flutt. m.	8 18,0	5 26,0
	β Drac.	12 7,5 [6]	-	16 32,0	4 24,5
	ι Hercul.	14 18,5 [8]	-	17 18,5	3 0,0
	ξ Drac.	4 6,0 [6]	-	8 17,0	4 11,0
	γ —	6 0,5 [8]	-	3 32,2	2 2,3
20.	Capella	8 19,2 [8]	-	4 8,7	4 10,5
23.	ε Urs. Maj.	9 31,7 [8]	-	5 15,7	4 16,0
	ζ ———	10 12,5 [6]	v. faint	5 15,0	4 31,5
	β Drac.	4 32,5 [8]	-	9 23,5	4 25,0
	ι Hercul.	6 27,0 [8]	-	9 27,5	3 0,5
	ξ Drac.	5 10,5 [8]	-	9 24,2	4 13,7

1730.		Rev. ″		Rev. ″	Rev. ″
	γ Drac.	8 20,5 [8]	-	6 19,5	2 1,0
Aug. 24.	Capella	6 15 7 [7] flutt.	-	2 4,4	4 11,3

This day I tried how many revolutions the spot that Capella is adjusted to was distant from that to which ι Herculis is; that is, how far 44° 15′ is from 43° 50′; and I found it to be 44 rev. 18″,3.

	β Drac.	3 1,7 [8]	-	7 27,2	4 25,5
	ι Herc.	5 2,0 [6]	-	8 2,5	3 0,5
	ξ Drac.	3 20,5 [4]	-	8 0,0	4 13,5
	γ —	5 6,0 [7]	-	3 4,8	2 1,2
25.	Capella	7 19,0 [6] flutt.	-	3 9,5	4 9,5
	β Drac.	1 29,5 [8]	-	6 20,5	4 25,0
	ξ —	2 0,5 [6]	-	6 12,3	4 11,8
	γ —	3 10,8 [8]	-	1 9,5	2 1,3
26.	Capella	4 22,5 [8]	-	0 12,0	4 10,5

I this day cleaned the eyeglass, and-screwed it as near as I could in the same manner as it was before.

	ε Urs. Maj.	8 3,0 [8]	-	3 18,5	4 18,5
	ζ ———	8 27,0 [8]	-	3 28,0	4 33,0
	η ———	10 8,0 [8]	-	4 14,8	5 27,2
	β Drac.	29 33,7 [8]	-	4 24,7	4 25,0
	ι Hercul.	3 2,0 [6]	-	6 3,2	3 1,2
	γ Drac.	7 18,7 [8]	-	5 17,7	2 1,0
29.	Capella	10 16,0 [8]	-	6 5,5	4 10,5
31.	β Drac.	1 1,7 [8]	-	5 27,2	4 25,5
	ι Herc.	2 19,5 [5]	-	5 21,0	3 1,5
	ξ Drac.	5 0,5 [6]	-	10 12,0	5 11,5
	γ —	6 11,5 [8]	-	4 10,5	2 1,0
Sept. 1.	γ —	5 33,7 [10]	-	3 32,7	2 1,0
3.	Capella	7 30,0 [6] flutt.	-	3 21,3	4 8,7
7.	γ Drac.	6 27,0 [8]	-	4 26,0	2 1,0
8.	Capella	8 26,5 [8] st.	-	4 14,8	4 11,7
	γ Drac.	4 29,0 [8]	-	2 28,0	2 1,0
9.	γ —	3 30,5 [8]	-	1 30,5	2 0,0
16.	γ —	7 31,2 [10]	-	5 30,5	2 0,7
18.	ζ Urs. Maj.	11 21,0 [8]	-	6 17,0	5 4,0
	η ———	11 27,5 [8]	-	5 30,0	5 31,5
	β Drac.	0 11,7 [8]	-	5 3,9	4 26,2
	γ —	6 13,3 [8]	-	4 13,5	1 33,8
20.	β —	2 4,9	-	6 30,3	4 25,4

1730.		Rev. ″			Rev. ″	Rev. ″
	γ Drac.	9 5,7 [9]		-	7 5,3	2 0,7
Sept. 22.	β —	1 9,4 [9]		-	6 0,9	4 25,5
	γ —	7 32,3 [9]		-	5 32,4	1 33,9

I went to Oxford, Sept. 25: returned Nov. 27.

Dec. 9.	γ —	9 4,3 [7]		-	6 19,7	2 18,6
	β Cass.	2 22,8 [8]		-	6 14,8	3 26,0
	α —	5 24,7 [8]		-	6 21,7	0 31,0
11.	β —	1 21,0 [6]		-	5 12,3	3 25,3
	α —	4 28,0 [8]	flutt.	-	5 25,5	0 31,5
12.	β Draconis	1 10,5 [8]		-	5 15,5	4 5,0
	γ ———	8 2,2 [8]		-	5 16,4	2 19,8
	β Cassiop.	1 31,0 [9]	st.	-	5 22,8	3 25,8
	λ ———	7 8,0 [8]		-	5 19,3	1 22,7
	α ———	6 8,7 [8]		-	7 7,2	0 32,5
15.	β Drac.	3 20,0 [6]		-	7 23,7	4 3,7
	γ —	10 11,0 [8]		-	7 24,5	2 20,5
18.	β —	3 8,5 [7]		-	7 10,2	4 1,7
24.	γ —	10 25,2 [8]		-	8 1,7	2 23,5
	β Cass.	3 33,5 [8]		-	7 23,5	3 24,0

I found that the wall was fallen back (to the eastward) so much that the plumbline was not quite free from the arch, I therefore brought the top of the tube from the wall about one revolution of the screw that moves it east and west.

	λ Cass.	9 16,5 [7]		-	7 28,0	1 22,5
err.	α —	7 3,0 [8]		-	3 32,7	3 4,3
	θ —	3 30,5 [6]		-	12 4,2	8 7,7
	τ Pers.	10 29,5 [7]		-	11 2,5	0 7,0 south
	γ —	7 0,5 [8]	flutt.	-	11 14,8	4 14,3
	α —	12 30,5 [8]		-	11 24,5	1 6,0
	Capella	15 18,0 [8]	flutt.	-	11 18,5	3 33,5
27.	———	15 22,5 [8]		-	11 23,2	3 33,3
28.	β Drac.	6 30,5 [6]	haz.	-	10 30,0	3 33,5
	ε Urs. Maj.	18 3,2 [10]		-	12 18,2	5 19,0
	ζ ———	18 5,2 [8]		-	12 3,7	6 1,5
	Alcore	11 26,5 [6]		-	12 3,7	0 11,2
1731.	η Urs. Maj.	18 3,6 [8]		-	11 6,6	6 31,0
Jan. 1.	γ Drac.	12 25,8 [5]	: : dub.		9 32,8	2 27,0
3.	τ Pers.	12 22,5 [8]		-	12 30,7	0 8,2
	γ —	7 33,5 [9]	st.	-	12 15,5	4 16,0

1731.		Rev. ″			Rev. ″	Rev. ″
	α Pers.	12 20,7 [7]		-	11 15,7	1 5,0
	δ —	9 2,0 [8]		-	10 4,0	1 2,0
	Capella	14 17,3 [8]		-	10 18,8	3 32,5
	ε Urs. M.	14 28,0 [7]	flutt.	-	9 9,0	5 19,0
	ζ ——	14 31,5 [6]	flutt.	-	8 29,8	6 1,7
	Alcore	8 19,5		-	8 29,8	0 10,3
	η —	15 30,0 [8]	flutt.	-	8 31,3	6 32,7
Jan. 4.	γ Drac.	10 2,8 [4]	haz.	-	7 9,8	2 27,0
18.	τ Pers.	6 16,0 [8]	st.	-	6 24,2	0 8,2
	γ —	2 19,0 [7]		-	7 0,2	4 15,2
	α —	8 11,5 [7]		-	7 6,0	1 5,5
	9 Aurig.	8 28,5 [8]		-	6 23,0	2 5,5
	Capella	10 29,0 [8]		-	6 31,3	3 31,7
24.	———	12 0,0 [4]		-	8 2,5	3 31,5
Feb. 3.	γ Drac.	11 13,0 [4]		-	8 10,0	3 3,0
	Capella	12 2,5 : :	v. dub. cl.		8 6,5	3 30,0
5.	9 Aurig.	14 5,5 [7]		-	12 1,3	2 4,2
	Capella	15 30,5 [7]		-	11 32,5	3 32,0
	35 Camel.	12 33,3 [8]		-	11 7,6	1 25,7
6.	β Drac.	7 20,2 [7]		-	11 8,5	3 22,3
	γ —	13 17,0 [8]		-	10 14,0	3 3,0
	Capella	14 32,3 [8]		-	11 2,8	3 29,5
7.	γ Drac.	12 26,2 [8]		-	9 22,5	3 3,7
Ap. 27.	Capella	12 30,5 [7]		-	8 28,3	4 2,2

May 3d, I went to Oxford, and returned to Wansted August 3d. The next day, August 4th, I examined the instrument, and found that when I brought 44° 15′ to the plumbline, the index stood at 11 rev. 1″,3; so that from April 27th to August 4th, the wall and the top of the telescope had fallen towards the north 2 rev. 7″ = 1′ 14″. This I suppose must have been occasioned by some sinking in the north end of the house, which was observed within this interval, and that end which was before supported by a timber partition, is now supported by a brick wall, that parts my aunt Pound's and the next new built house.

N. B. The telescope remained fixed in the same manner to the brass arch and wall, &c. at my return from Oxford, as when I observed Capella on April 27th.

		Rev. ″		Rev. ″	Rev. ″
Aug. 4.	ε Urs. Maj.	17 31,3 [8]	-	12 33,8	4 31,5
	ζ ———	17 6,2 [7]	-	11 28,4	5 11,8

		Rev. ″			Rev. ″	Rev. ″
1731.	η Urs. Maj.	15 4,0 [8]		-	8 31,8	6 6,2
	β Drac.	3 29,0 [8]		-	8 17,5	4 22,5
Aug. 7.	η Urs. Maj.	14 20,0 [7]		-	8 13,5	6 6,5
9.	Capella	5 33,7 [9]		-	1 23,7	4 10,0
10.	———	5 31,0 [8]		-	1 20,5	4 10,5
14.	β Drac.	2 6,2 [8]		-	6 30,0	4 23,8
	γ —	8 23,5 [8]		-	6 22,2	2 1,3
17.	Capella	10 26,5 [8]	st.	-	6 16,0windy	4 10,5
	η Urs. M.	12 10,5 [7]	cl.	-	6 2,7windy	6 7,8
	β Drac.	1 27,0 [8]		-	6 17,0	4 24,0
	γ —	8 3,3 [8]		-	6 2,8	2 0,5
18.	β —	3 24,5 [7]		-	8 14,7	4 24,2
	γ —	11 2,7 [8]		-	9 2,2	2 0,5
19.	Capella	12 10,0 [7]		-	8 0,0	4 10,0
20.	———	12 9,5 [8]		-	8 0,0	4 9,5
	ε Urs. M.	12 33,5 [7]	faint	-	7 31,5	5 2,0
	η ——	13 16,5 [6]		-	7 8,0	6 8,5
	β Drac.	2 19,0 [6]		-	7 10,0	4 25,0
	γ —	9 4,5 [8]		-	7 5,5	1 33,0
21.	Capella	11 20,2 [8]		-	7 10,5	4 9,7
	γ Drac.	9 12,5 [8]		-	7 12,8	1 33,7
22.	β —	9 27,5 [8]		-	14 17,5	4 24,0
	γ —	14 28,0 [8]		-	12 28,2	1 33,8
23.	β —	7 2,5 [8]		-	11 27,0	4 24,5
	γ —	13 19,5 [8]		-	11 19,7	1 33,8
Sept. 3.	β —	8 17,7 [7]		-	13 8,7	4 25,0
	γ —	13 6,5 [8]		-	11 7,3	1 33,2
7.	β —	6 5,5 [8]		-	10 30,5	4 25,0
	γ —	12 31,8 [8]		-	11 32,8	1 33,0
9.	Capella	11 27,0 [7]	flutt.	-	7 16,5	4 10,5
10.	———	11 23,0 [4]	flutt. m.		7 14,5	4 8,5
	ε Urs. M.	11 7,5 [6]	faint	-	6 0,5	5 7,0
	ζ ——	11 6,5 [7]	faint	-	5 17,3	5 23,2
	η ——	10 27,5 [6]	faint	-	4 13,0	6 14,5
	β Drac.	1 5,5 [8]		-	5 30,5	4 25,0
	γ —	7 21,8 [8]		-	5 23,3	1 32,5
11.	η Urs. M.	11 0,0 [6]	flutt.	-	4 19,7	6 14,3
12.	Capella	6 7,0 [5]	flutt. m.		1 31,7	4 9,3
	η Urs. M.	7 2,5 : :		-	1 21,0	5 15,5
	β Drac.	2 30,5 [10]	v. g.		7 20,5	4 24,0

1731.		Rev. ″		Rev. ″	Rev. ″
	γ Drac.	9 10,8 [9]	v. g. -	7 11,8	1 33,0
Aug. 13.	Capella	11 26,8 [8]	err. -	7 15,3	4 11,5
	ε Urs. Maj.	13 18,0 [8]	-	8 11,0	5 7,0
	ζ ———	13 2,5 [4]	haz. -	7 13,0	5 23,5
	η ———	14 5,5 [8]	g. -	7 23,8	6 15,7
Sept. 13.	β Drac.	2 25,5 [6]	cl. -	7 15,5	4 24,0
	γ —	9 20,8 [8]	-	7 21,8	1 33,0
15.	ε Urs. Maj.	13 16,5 [6]	-	8 8,0	5 8,5
	ζ ———	13 20,5 [6]	-	7 32,0	5 22,5
	β Drac.	8 20,0 [8]	-	13 11,5	4 25,5
	γ —	12 21,0 [8]	-	10 22,8	1 32,2
18.	ζ Urs. M.	15 17,5 [7]	-	9 27,8	5 23,7
	η ——	14 31,5 [7]	-	8 15,0	6 16,5
19.	β Drac.	3 6,0 [5]	cl. -	7 29,7	4 23,7
	γ —	9 5,5 [8]	no aper. too bt.	7 5,7	1 33,8
20.	ζ Urs. M.	12 26,0 [6]	haz. -	7 1,7	5 24,3
	β Drac.	6 29,5 [9]	-	11 19,0	4 23,5
	γ —	13 1,3 [9]	st. -	11 2,5	1 32,8
22.	Capella	15 19,0 [8]	-	11 9,5	4 9,5
	η Urs. M.	14 25,2 [3]	: : v. haz.	8 7,7	6 17,5
	β Drac.	3 13,0 [8]	-	8 3,0	4 24,0
	γ —	9 32,5 [8]	-	7 33,2	1 33,3
25.	β —	3 1,5 [4]	cl. -	7 24,5	4 23,0
	γ —	9 3,0 [5]	cl. -	7 3,5	1 33,5
Oct. 1.	ε Urs. Maj.	12 4,0 [6]	-	6 23,0	5 15,0
	ζ ———	11 31,0 [7]	-	6 1,5	5 29,5
	η ———	12 12,5 [6]	-	5 26,0	6 20,5
2.	γ Drac.	3 11,2 [8]	-	1 10,5	2 0,7
4.	η Urs. M.	7 2,5 [6]	-	0 15,5	6 21,0
	β Drac.	3 20,7 [8]	-	8 9,0	4 22,3
	γ —	10 3,3 [4]	v. haz.	8 2,3	2 1,0
Dec. 18.	β —	3 28,7 [8]	-	7 30,7	4 2,0
	γ —	9 33,7 [8]	-	7 13,7	2 20,0
26.	τ Pers.	6 25,5 [6]	-	7 14,0	0 22,5
	γ —	2 0,0 [6]	faint cl.	6 29,5	4 29,5
	α —	7 25,6 [8]	-	6 31,6	0 28,0
31.	γ Drac.	10 12,7 [8]	-	7 22,7	2 24,0
Jan. 1.	ε Urs. M.	13 1,8 [7]	flutt. -	6 30,8	6 5,0
Manè.	ζ ———	12 32,0 [8]	-	6 11,8	6 20,2
	Alcore	6 18,8 [7]	-	6 11,8	0 7,0

1732.		Rev. ″			Rev. ″	Rev. ″
	η Urs. M.	12 30,0 [8]		-	5 17,0	7 13,0
Jan. 4.	Capella	9 0,5 [8]	st.	-	5 2,5	3 32,0
8.	β Cass.	0 28,5 [8]		-	5 3,5	4 9,0
	λ —	6 0,3 [6]		-	4 29,3	1 5,0
	α —	5 19,3 [8]		-	7 1,0	1 15,7
	τ Pers.	6 33,5 [7]		-	7 21,8	0 22,3
	γ —	2 2,2 [8]		-	6 31,7	4 29,5
	α —	7 23,0 [8]	st.	-	6 30,2	0 26,8
	Capella	11 3,5 [7]		-	7 5,5	3 32,0
Manè. 9.	ε Urs. Maj.	12 33,0 [7]		-	6 28,6	6 4,4
	ζ ———	13 0,5 [8]		-	6 13,5	6 21,0
	Alcore	6 21,5 [7]		-	6 13,5	0 8,0
	η Urs. M.	13 24,5 [8]	st.	-	6 9,0	7 15,5
	γ Drac.	8 11,5 [8]	st.	-	5 18,0	2 27,5
	τ Pers.	4 6,5 [5]		-	4 28,0	0 21,5
Manè. 11.	ε Urs. Maj.	12 7,5 [7]	st.	-	6 5,2 q.	6 2,3
	ζ ———	12 11,0 [7]		-	5 25,3	6 19,7
	Alcore	5 32,5 [8]		-	5 25,3	0 7,2
	η Urs. M.	13 0,0 [7]		-	5 18,5	7 15,5

Then I went to Oxford, and left the telescope fixed as in this last observation. On my return from Oxford, I rectified the same spot to the plumb-line, (on April 7th,) and found the index at 5 17,5; so that the situation was the same within a single second as three months before.

April 7.	Capella	9 3,5 [8]		-	5 5,7	3 31,8
13.	η Urs.	11 10,7 [6]		-	4 7,2	7 3,5
17.	α Cass.	2 20,0 [6]		-	3 13,5	0 27,5
	η Urs. M.	10 7,0 [7]	flutt.	-	3 5,0	7 2,0
	Capella	6 15,7 [7]		-	2 13,7	4 2,0

April 22, I found the wire broke at the notch at top; I fastened on the same wire again, and added a piece at the bottom to make it of a proper length.

22.	Capella	6 11,5 [6]		-	2 11,0	4 0,5
Sept. 2.	β Drac.	10 9,7 [7]		-	14 33,5	4 23,8
	γ —	14 2,7 [7]		-	12 5,0	1 31,7
3.	β Urs. M.	1 2,0 [3]	faint	-	10 2,5	9 0,5
	γ ———	13 27,0 [3]	v. faint		9 9,5	4 17,5
	ε ———	18 15,6 [6]		-	12 23,6	5 26,0
	ζ ———	16 24,5 [5]	v. faint		10 20,5	6 4,0

1732.		Rev. ″		Rev. ″	Rev. ″
	β Drac.	4 1,5 [8]	-	8 26,0	4 24,5
	γ —	10 19,5 [8]	-	8 22,8	1 30,7
Sept. 4.	γ Urs. M.	13 0,0 : : dub.	-	8 16,5	4 17,5
	ε ———	20 10,6 [6]	-	14 17,6	5 27,0
	β Drac.	2 5,1 [7]	-	6 29,7	4 24,6
	γ —	9 19,3 [6] flutt.	-	7 21,3	1 32,0
5.	β —	3 7,2 [8]	-	7 32,2	4 25,0
	γ —	10 30,3 [6] flutt.	-	8 33,8	1 30,5
6.	β Urs. M.	0 32,5 [6]	-	9 31,8	8 33,3
	ε ———	16 16,5 [7]	-	10 25,5	5 25,0
	ζ ———	13 16,5 : : dub.	-	7 12,0	6 4,5
	β Drac.	2 11,7 [8] st.	-	7 2,5	4 24,8
	γ —	8 32,7 [8]	-	7 1,7	1 31,0

December 22, I rectified the same spot, and found the index stood at 7 13,5; so that the instrument had altered 12″ between September and December.

30.	β Cass.	2 15,0 [6]	-	7 9,5	4 28,5
	λ —	8 27,0 [6]	-	8 8,0	0 19,0
1733.	α —	6 15,3 [8]	-	8 15,3	2 0,0
Jan. 1.	β —	3 18,5 [7]	-	8 12,7	4 28,2
21.	γ Drac.	10 20,5 [7]	-	7 26,5	2 28,0
	β Cass.	2 33,5 [7]	-	7 24,0	4 24,5
	α —	5 16,7 [7]	-	7 14,2	1 31,5
	τ Pers.	6 13,5 [7]	-	7 14,5	1 1,0
forsan.	γ —	2 13,0 [4]	-	7 20,0 err.	5 7,0
22.	β Drac.	3 7,0 [8]	-	6 31,8	3 24,8
	γ —	9 24,0 [7]	-	6 29,3	2 28,7
31.	9 Aurig.	10 1,0 [8]	-	8 3,2	1 31,8
	Capella	11 15,0 [8] st.	-	7 23,8	3 25,2
	18 Camel.	6 21,5 [5] faint	-	8 18,5	1 31,0
	δ Aurig.	6 33,7 [8]	-	7 1,7	0 2,0
	35 Camel : :	9 28,5 [7]	-	7 31,7	1 30,8

August 25, I found the index, when rectified to the same spot as Jan. 31, to stand at 7 23,2; the difference being 8″,5 since January.

25.	β Drac.	4 7,5 [8]	-	8 33,5	4 26,0
	γ —	11 15,0 [8]	-	9 22,0	1 27,0
26.	β —	4 12,5 [8]	-	9 5,0	4 26,5
	γ —	11 32,5 [8]	-	10 4,5	1 28,0
29.	β —	2 28,5 [8]	-	7 20,0	4 25,5

1734.		Rev. ″		Rev. ″	Rev. ″
	γ Drac.	10 15,0 [8]	-	8 21,5	1 27,5
June 11.	Capella	16 11,0 [8]	-	12 8,0	4 3,0
	ε Urs. Maj.	17 12,5 [8]	-	10 31,0	6 15,5
	ζ ———	16 10,2 [8]	I saw the small star.	9 13,7	6 30,5
	η ———	10 27,0 [7]	-	3 5,5	7 21,5
12.	β Cass.	1 25,0 [8]	-	6 13,5	4 22,5
	α —	5 16,5 [7]	-	7 11,5 q.	1 29,0
	α Pers.	10 20,7 [8]	-	9 32,7	0 22,0
	Capella	12 30,0 [7]	-	8 27,0	4 3,0
	ε Urs. M.	14 21,5 [7]	-	8 5,0	6 16,5
	ζ ———	15 21,0 [7]	-	8 24,0	6 31,0
	η ———	9 3,0 [7]	-	1 14,5	7 22,5
13.	β Cass.	2 8,0 [9]	-	6 31,0	4 23,0
	α —	6 8,5 [8]	-	8 4,0 q.	1 29,5
16.	ε Urs. M.	13 30,0 [5]	-	7 13,5	6 16,5
	η ———	19 30,8 [8]	-	12 8,8	7 22,0
18.	ζ ———	17 13,0 [8]	-	10 17,5	6 29,5
	η ———	12 23,5 [8]	-	5 2,5	7 21,0
19.	β Drac.	2 6,0 [8]	-	6 17,0	4 11,0
	γ —	7 15,7 [8]	-	5 8,7	2 7,0
21.	β —	4 3,5 [8]	-	8 16,0	4 12,5
	γ —	11 10,7 [8]	flutt. -	9 4,7	2 6,0
July 3.	Capella	8 4,0 [7]	-	4 0,5	4 3,5
5.	β Drac.	3 12,7 [8]	-	7 27,7	4 15,0
	γ —	9 22,0 [8]	-	7 17,5	2 4,5
9.	ε Urs. M.	16 3,0 [6]	flutt. -	9 20,0	6 17,0
	ζ ———	15 31,0 [7]	flutt. -	9 2,0	6 29,0
	η ———	15 12,7 [8]	-	7 26,2	7 20,5
10.	ε ———	15 31,0 [8]	-	9 14,5	6 16,5
	ζ ———	17 16,7 [8]	-	10 22,2	6 28,5
	η ———	19 0,0 [7]	cl. -	11 13,5	7 20,5
	β Drac.	7 0,2 [8]	-	11 17,2	4 17,0
	γ —	11 28,0 [9]	-	9 26,7	2 1,3
11.	α Pers.	11 3,0 [6]	-	10 16,0	0 21,0
	Capella	11 12,0 [8]	-	7 7,5	4 4,5
	ζ Urs. M.	12 19,5 [7]	-	7 24,5	4 29,0
	η ———	10 30,7 [8]	-	3 10,2	7 20,5
16.	Capella	11 14,0 [8]	-	7 10,0	4 4,0
	η Urs. M.	15 33,0 [6]	-	8 13,0	7 20,0
	β Drac.	4 26,2 [8]	st. -	9 10,7	4 18,5

1734.		Rev. ″		Rev. ″	Rev. ″
	γ Drac.	13 9,2 [9] st.	-	11 9,2	2 0,0
July 17.	ε Urs. M.	18 28,0 [6]	-	12 11,0	6 17,0
	η ——	16 16,5 [9]	-	8 31,5	7 19,0
29.	β Drac.	4 16,5 [6]	-	9 4,7	4 22,2
	γ —	10 1,2 [8]	-	8 4,7	1 30,5
31.	ε Urs. M.	13 21,0 [4] faint	-	7 3,0	6 18,0
	β Drac.	3 26,0 [3] flutt. m.		8 13,5	4 21,5
	γ —	12 8,0 [4] flutt. m.		11 11,0	1 31,0
Aug. 3.	Capella	10 1,2 [9]	-	6 0,2	4 1,0
	ζ Urs. M.	12 10,0 [8]	-	5 12,2	6 31,8
	η ——	13 4,7 [9]	-	5 15,2	7 23,5
	β Drac.	4 2,2 [8]	-	8 24,0	4 21,8
	γ —	9 23,7 [10]	-	7 26,5	1 31,2
6.	Capella	8 18,0 [9] st.	-	4 14,0	4 4,0
	β Drac.	2 19,5 [6]	-	7 8,5	4 23,0
	γ —	8 12,0 : :	-	6 16,5	1 29,5
7.	Capella	8 26,2 [9]	-	4 23,0	4 3,2
	ζ Urs. M.	17 25,0 [8]	-	10 26,7	6 32,3
	η ——	13 10,0 [9]	-	5 21,7	7 22,3
	β Drac.	0 16,7 [9] st.	-	5 5,7	4 23,0
	γ —	7 17,0 [9] st.	-	5 22,5	1 28,5
8.	β —	1 0,0 [9] st.	-	5 24,0	4 24,0
	γ —	8 1,7 [9] st.	-	6 7,0	1 28,7
11.	γ —	8 27,0 [8]	-	6 32,0	1 29,0

Dec. 11th, 1734. After I had adjusted the same spot to the plumbline as left Aug. 11th, the index stood at 7 rev. 5″; so that the telescope or wall, &c. was altered 7″ only.

Dec. 11.	β Drac.	2 32,0 [8]	-	7 3,2	4 5,2
	γ —	9 23,7 [8]	-	7 12,2	2 11,5
	β Cassiop.	0 6,2 [8] st.	-	6 6,2	6 0,0
	λ ———	8 29,2 [8]	-	9 15,0	0 19,8
	α ———	6 27,0 [8]	-	9 31,6	3 4,6
22.	β Drac.	2 31,5 [8]	-	6 31,5	4 0,0
	γ —	9 26,5 [8]	-	7 11,2	2 15,3
	β Cass.	1 10,0 [8]	-	7 9,5	5 33,5
	λ —	7 2,0 [8]	-	7 21,0	0 19,0
	α —	7 21,5 [8]	-	10 25,5	3 4,0
	Capella	12 28,5 [8]	-	9 6,5	3 22,0
23.	β Drac.	5 0,7 [8]	-	9 0,0	3 33,3
	γ —	10 28,0 [8]	-	8 11,7	2 16,3

1734.		Rev. ″		Rev. ″	Rev. ″
Dec. 25.	β Cass.	1 20,0 [8]	-	7 19,0	5 33,0
	λ —	7 17,0 [8]	-	8 3,0	0 20,0
	α —	6 20,5 [8]	-	9 25,5	3 5,0
	Capella	11 15,7 [9] st.	-	7 26,0	3 23,7
27.	β Drac.	4 28,8 [8]	-	8 27,3	3 32,5
	γ —	11 0,3 [8]	-	8 16,3	2 18,0

Dec. 27th, I brought 37° 30′, &c. to the plumbline, and index stood as follows:

° ′	Index.	° ′	Index.	° ′	Index.	° ′	Index.
37,30	0 4,0	37,45	0 2,0	38, 0	0 2,0	38,15	0 0,5
37,45	26 30,0	38, 0	26 28,0	38,15	26 28,7	38,25	17 29,8
Diff.	26 26,0	Diff.	26 26,0	Diff.	26 26,7	38,30	26 26,5

Hence by these observations, one degree on the arch, viz. from 37° 30′ to 38° 30′ is equal to 107 rev. 2,″7; and 55′ from 37° 30′ to 38° 25′ is equal to 98 rev. 6″; by which last number may be determined the exact difference at any time between the declination of β and γ Draconis in the rev. of the screw; and if the exact difference of declination of those two stars be tried by some other method, the true angle corresponding to the divisions of the screw will be known. If we suppose that from 37° 30′ to 38° 30′ on the arch of the instrument were an exact degree, then one revolution of the screw would be 33″,6199. If we suppose that from 38° 25′ to 37° 30′ on the arch be exactly 55′ of a degree, then one revolution of the screw would be 33″,613.

1735.		Rev. ″		Rev. ″	Rev. ″
Sept. 9.	η Urs. M.	13 8,5 [8]	-	4 30,5	8 12,0
	γ Drac.	5 7,8 [10] g.	-	3 14,7	1 27,1
10.	η Urs. M.	9 8,7 [8]	-	0 31,0	8 11,7
	β Drac.	2 33,8 [9] st.	-	7 23,0	4 23,2
	γ —	7 30,7 [8] st.	-	6 4,0	1 26,7
13.	β Urs. M.	8 5,7 [7]	-	15 17,0	7 11,3
	γ ———	16 11,5 [8]	-	10 5,5	6 6,0
	ε ———	15 15,0 circiter cl.		7 32,5	7 16,5
	ζ ———	14 13,0 [7]	-	6 22,0	7 25,0
	β Drac.	6 18,5 [8]	-	11 7,3	4 22,8
1736.	γ —	12 15,5 [9]	-	10 24,0	1 25,5
Aug. 31.	β —	5 4,7 [8]	-	9 26,2	4 21,5
	γ —	10 29,8 [8]	-	9 4,0	1 25,8
Sept. 8.	ζ Urs. M.	15 8,0 [4] faint	-	7 2,5	8 5,5

1736.		Rev. ″			Rev. ″	Rev. ″
	η Urs. M.	7 32,0 [8]		-	29 6,0	8 26,0
	β Drac.	0 21,2 [8]		-	5 9,0	4 21,8
	γ —	6 7,2 [8]		-	4 15,7	1 25,5
Sept. 9.	β —	0 17,7 [8]		-	5 5,0	4 21,3
	γ —	6 26,7 [8]		-	5 0,7	1 26,0
10.	ε Urs. M.	7 30,0 [4]		-	0 0,0	7 30,0
12.	ε ———	8 8,5 [4]	faint	-	0 12,7	7 29,8
	ζ ———	8 16,7 [6]	faint	-	0 11,2	8 5,5
	η ———	10 32,2 [6]		-	2 5,2	8 27,0

After I had made the observations on Aug. 31, I left the instrument without taking off the string (that comes through the wood-work inclosing the whole) by which the weight hangs, that keeps the tube bearing lightly against the point of the screw; but between Aug. 31 and Sept. 2 I suspect that somebody had meddled with the string, and by pulling it had caused the telescope to strike against the end of the screw and blunted its point, whereby the whole was so much disordered, that when I came to make an observation again on Sept. 2d, I found a great and unusual disagreement in the several trials for adjusting a spot to the plumbline. For bringing the spot for β Drac. to it before the passage, the index stood at 9 rev. 18″,8; and after, at 9 rev. 22″,0; and at the passage, index was 5 rev. 9″,5; so that this observation differed 10″ from the last.

In adjusting the spot for γ Draconis, the disagreement was yet more remarkable, for the index before the passage was at 14 rev. 30″,8 . . 14 rev. 29″,5; and at the passage, 16 rev. 6″,3; and after the passage, at 14 rev. 20″ . . 14 rev. 19″,5, and 14 rev. 14″,5; from whence it was evident that the screw was greatly damaged, and that no observation then made with it could be of any use or depended upon. Therefore taking off the screw, &c. I carried it the next day to Mr. Graham, who rectified the point of it and cleaned it, and then the observations made the following days were found to agree very well with those of Aug. 31st; so that the forementioned irregularities, doubtless proceeded from something that had been done to the instrument between Aug. 31st and Sept. 2d.

But that it may not be subject to the like accident for the future, I shall take care to leave the instrument so that it may not be again disordered in the same manner.

N. B. When the screw was cleaned and put in again with the wheel-work that shews the number of revolutions, the beginning of the divisions was not made to correspond exactly with the same part of the screw as before, but 2 or 3 threads forwarder; so that the part of the screw which before answered to 3 or 4 revolutions, now perhaps corresponds to 0 or 1 revolution. But as I have not been able to discover the least inequality in the threads of the screw in different parts of it, this remark may upon that account seem needless. However, I have thought proper to make it, in case we should hereafter have occasion to know what alteration was made in the disposition of the screw at this time.

1737.		Rev. ″		Rev. ″	Rev. ″
Jan. 3.	γ Drac.	5 23,0 [8]	-	3 4,5	2 18,5
4.	β —	2 24,5 [8]	-	6 15,2	3 24,7
	γ —	8 22,0 [8]	-	6 2,5	2 19,5
9.	Capella	5 2,5 [8] st.	-	1 22,7	3 13,8
18.	———	2 30,2 [8]	-	29 17,7	3 12,5
19.	β Drac.	0 16,5 [7]	-	4 3,7	3 21,2
	γ —	6 8,5 [8]	-	3 17,7	2 24,8
	Capella	7 8,0 [8]	-	3 28,8	3 13,2

Jan. 19th, I brought 34° 45′ to the plumbline; index standing as follows:

° ′	Rev. ″		Rev. ″
34,45	2 18,7	repeated another day.	1 6,3
34,55	20 12,5		19 0,1
Diff.	17 27,8		17 27,8

These two are the spots to which the plumbline is adjusted for γ Ursæ Maj. and α Cassiop.; which stars not being far from the equinoctial colure, and nearly the same distance from the pole, their difference of declination being carefully observed by the instrument, will be a proper mean for determining the precession of the equinox.

		Rev. ″		Rev. ″	Rev. ″
June 22.	ζ Urs. M.	8 10,5 [8]	-	29 32,5	8 12,0
	η ———	9 13,0 [8]	-	0 13,2	8 33,8
	° ′				
	39,20	9 10,9	Diff. = 8 rev. 31″,6.		
	39,15	0 13,3			
July 1.	η Urs. M.	9 12,3 [8]	-	0 11,3	9 1,0
	39,20	9 9,0	Diff. = 8 rev. 31″,7.		
	39,15	0 11,3			

Then I put on a new wire or plumbline, and adjusting 39° 15′ to it, the index stood at 0 rev. 12″,5, or about 1″ more than with the old one; but there was about the same difference in adjusting the spot with the old wire before and after the observation.

1737.		Rev. "		Rev. "	Rev. "
July 3.	η Urs. M.	9 12,7 [8]	-	0 12,2	9 0,5
	° ′				
	39,20	9 9,7	Diff.=8 rev. 31″,5.		
	39,15	0 12,2			
6.	ζ Urs. Maj.	10 10,0 [8]	-	1 32,0	8 12,0
	η ———	9 33,0 [8]	-	0 32,7	9 0,3
	39,20	9 30,5	Diff.=8 rev. 31″,8.		
	39,15	0 32,7			
Sept. 1.	39,20	10 6,5; so that it has altered 10″ since July 6.			
	β Drac.	29 15,1 [8]	-	3 32,6	4 17,5
	γ —	4 29,8 [9]	-	3 2,3	1 27,5
2.	β —	3 14,7 [8]	-	7 32,7	4 18,0
	γ —	7 26,0 [8]	-	5 33,7	1 26,3
4.	β —	0 24,3 [6]	clouds	5 7,0	4 16,7
6.	ε Urs. M.	13 4,5 [8]	-	4 25,2	8 13,3
				17 forsan 13″,8	
q.	ζ ———	9 31,0 [8]	faint -	1 7,2	8 23,8
	η ———	10 30,3 [8]	-	1 20,8	9 9,5
	β Drac.	0 12,0 [8]	-	4 30,0	4 18,0
	γ —	8 6,9 [9]	st. -	6 13,4	1 27,5
7.	β —	0 23,3 [8]	st. -	5 5,8	4 16,5
	γ —	7 32,5 [6]	-	6 4,7	1 27,8
8.	β —	1 9,8 [8]	-	5 27,8	4 18,0
	γ —	9 25,5 [8]	-	7 32,7	1 26,8
10.	γ —	9 30,5 [8]	-	8 2,2	1 28,3
Dec. 12.	γ —	10 29,7 [8]	-	8 16,0	2 13,7

1738, June 17th, instrument as left Dec. 12; and the same spot being brought to the plumbline, index stood at 8 rev. 9″; so that it had altered only 7″ in half a year.

						° ′
June 18.	ζ Urs. Maj.	10 16,0 [4]	-	10 18,5	0 2,5	33 40
				1 22,5	8 27,5	33 35
	η ———	8 5,0 [8]	-	7 18,5	0 20,5	39 20
				28 20,5	9 18,5	39 15
19.	Capella	6 2,0 [6]	faint -	2 18,0	3 18,0	
27.	ζ Urs. M.	8 21,5 [4]	faint -	8 25,0	0 3,5	
				29 29,0	8 26,5	

1738.		Rev. ″		Rev. ″	Rev. ″	° ′
	η Urs. M.	6 31,2 [7]	-	27 16,0	9 15,2	39 15
				6 14,0	0 17,2	39 20
June 29.	ζ ———	3 0,5 [6]	-	3 3,5	0 3,0	33 40
	η ———	4 25,7 [7]	-	4 7,2	0 18,5	south 39 20
30.	Capella	7 31,7 [7]	-	4 13,7	3 18,0	
July 2.	ζ Urs. M.	5 5,0 [6] faint	-	5 6,3	0 1,3	
				26 10,0	8 29,0	
	η ———	8 22,8 [7]	-	8 4,8	0 18,0	
				29 6,8	9 16,0	
15.	ζ ———	5 31,0 [3] dub.	-	5 33,7	0 2,7	
				27 03,0	8 28,0	
	η ———	8 20,5 [4] faint	-	8 2,3	0 18,2	
				29 4,3	9 16,2	
25.	β Drac.	1 24,8 [7]	-	6 1,6	4 10,8	
	γ —	8 32,7 [8]	-	6 32,7	2 0,0	
28.	β —	1 27,5 [8] st.	-	6 4,8	4 11,3	
	γ —	10 28,7 [8] st.	-	8 30,5	1 32,2	
29.	β —	5 1,5 [7] flutt.	-	9 12,0	4 10,5	
	γ —	9 2,8 [6] flutt.	-	7 5,1	1 31,7	

Sept. 9th, the instrument was fixed as left July 29; but the same spot being adjusted to the plumbline, index stood at 7 rev. 8″; differing only 3″.

Sept. 9.	γ Drac.	7 26,8 [7]	-	5 32,5	1 28,3
12.	γ —	6 9,7 [8]	-	4 16,0	1 27,7
13.	β —	2 27,7 [7]	-	7 8,2	4 14,5
	γ —	11 27,2 [8]	-	9 33,4	1 27,8
14.	γ —	3 27,2 [8]	-	1 31,5	1 29,7
15.	γ —	3 28,0 [8] not altered.		1 32,7	1 29,3
16.	γ —	4 20,0 [8]	-	2 24,8	1 29,2
23.	η Urs. M.	3 10,2 [4] faint	-	2 11,2	0 33,0
	β Drac.	1 14,0 [3]	-	5 27,3	4 13,3

Sept. 23, I left a spot at 3 rev. 1″,0; and found (the plummet being rectified to the same spot) Dec. 22d, the index at 3 rev. 7″,5; differ. being 6″½.

Dec. 22.	β Cass.	6 10,8 [8] st.	-	14 15,3	8 4,5
	λ —	13 15,8 [6]	-	16 8,8	2 27,0
	θ Pers.	9 2,3 [6]	-	14 0,8	4 32,5
	τ —	12 20,5 [8]	-	16 4,2	3 17,7
	γ —	5 2,0 [8]	-	12 21,0	7 19,0
23.	β Cass.	4 19,5 [8]	-	12 23,5	8 4,0

1738.		Rev. ″		Rev. ″	Rev. ″
	α Cass.	10 15,0 [8] st.	-	15 23,2	5 8,2
	τ Pers.	8 11,5 [8]	-	11 28,0	3 16,5
	γ —	4 28,5 [8]	-	12 12,5	7 18,0
	α —	10 4,5 [8]	-	11 27,0	1 22,5
Dec. 24.	τ —	8 32,5 [8]	-	12 14,5	3 16,0
	γ —	6 17,6 [8]	-	14 2,8	7 19,2
	α —	14 14,0 [8]	-	16 3,3	1 23,3
28.	β Cass.	9 14,7 [8]	-	17 16,5	8 1,8
	λ —	9 0,5 [8]	-	11 24,0	2 23,5
	α —	12 0,5 [8]	-	17 8,5	5 8,0
	τ Pers.	5 7,0 [8]	-	8 22,2	3 15,2
	γ —	1 25,5 [8]	-	9 9,5	7 18,0
30.	γ Drac.	7 28,0 [8]	-	5 4,5	2 23,5
	Capella	8 27,0 [6]	-	5 21,8	3 5,2
	18 Camel.	4 26,5 [6]	-	7 7,5	2 15,0
	δ Aurig.	9 6,5 [8]	-	9 7,8	0 1,3
1739.	35 Camel.	12 12,0 [8]	-	10 17,5	1 28,5
Jan. 14.	γ Drac.	13 7,5 [8]	-	10 13,5	2 28,0
	Capella	9 6,5 [8]	-	6 8,5	2 32,0
	18 Camel.	6 23,0 [6]	-	9 6,3	2 17,3
	δ Aurig.	7 24,0 [8]	-	7 29,0	0 5,0
	35 Camel.	3 32,0 [6]	-	2 5,8	1 26,2
15.	γ Drac.	5 1,5 [7]	-	2 7,3	2 28,2
24.	β —	28 24,0 [8]	-	2 0,5	3 10,5
	γ —	6 9,7 [8]	-	3 13,2	2 30,5
Feb. 2.	Capella	5 6,5 [8]	-	2 6,2	3 0,3
	δ Aurig.	1 17,2 [8]	-	1 24,2	0 7,0
	35 Camel.	4 3,5 : : dub.	-	2 13,5	1 24,0
3.	Capella	7 9,3 [8] st.	-	4 9,3	3 0,0
4.	———	6 8,5 [9] st.	-	3 8,8	2 33,7
	18 Camel.	2 9,0 [6]	-	4 27,3	2 18,3
	δ Aurig.	4 24,3 [8]	-	4 31,6	0 7,3
	35 Camel.	6 19,3 [8]	-	4 29,8	1 23,5
6.	Capella	9 17,7 [6]	-	6 17,5	3 0,2
	18 Camel.	3 20,0 : :		6 4,5	2 18,5
	δ Aurig.	4 8,0 [7]	-	4 14,8	0 6,8
	35 Camel.	7 21,0 [7]	-	5 31,8	1 23,2

April 19. The same spot being brought to the line, index was 6 1,5; Diff. = 3″,7.

April 19.	γ Urs. Maj.	12 28,7 [8]	-	4 29,7	7 33,0

1739.		Rev. ″		Rev. ″	Rev. ″
	ε Urs. Maj.	5 4,5 [8]	-	4 25,3	0 13,2
				25 27,5	9 11,0
	ζ ———	6 27,5 [8]	-	27 3,0	9 24,5
	Alcore	0 14,5 -	-	27 3,0	
	η Urs. M.	7 30,5 [8]	-	6 14,0	1 16,5
				27 16,0	10 14,5
24.	γ ——	8 5,0 [3] cl. ::	-	0 8,5	7 30,5
25.	ε ——	9 17,5 [5] cl. q.	-	9 8,7	0 8,8
				0 11,0	9 6,5
	ζ ——	10 17,0 [6]	-	0 29,0	9 22,0
				9 25,0	0 26,0
	η ——	9 21,2 [6]	-	29 9,5	10 11,7
				8 7,8	1 13,4
26.	γ ——	9 6,5 [4] :	-	1 11,5	7 29,0
				10 8,5	1 2,0
	ε ——	11 33,0 [8] st.	-	2 23,8	9 9,2
				11 21,5	0 11,5
	ζ Urs. M.	11 9,2 [8] st.	-	1 22,0	9 21,2
				10 18,5	0 24,7
	Alcore	4 30,5	-	1 22,0	
				10 18,5	
	η Urs. M.	9 6,5 [8] st.	-	28 29,5	10 11,0
				7 27,8	1 12,7

Aug. 18. The same spot being brought to the plumbline, index was 28 13,5; the difference being 16″.

18.	γ Drac.	7 3,5 [8]	-	5 3,5	2 0,0
19.	β —	28 18,5 [8] st.	-	2 26,8	4 8,3
	γ —	4 9,5 [8] st.	-	2 9,5	2 0,0
20.	β —	29 9,4 [8] st.	-	3 17,6	4 8,2
	γ —	10 5,5 [7]	-	8 4,5	2 1,0
24.	β —	26 26,2 [8] st.	-	0 33,7	4 7,5
	γ —	1 29,3 [7]	-	29 29,4	1 33,9
26.	β —	28 14,0 [8] st.	-	2 22,0	4 8,0
	γ —	2 13,0 [8]	-	0 13,3	1 33,7
27.	β —	0 12,0 [8]	-	4 20,3	4 8,3
	γ —	5 31,3 [8] st.	-	3 31,8	1 33,5
29.	β —	26 27,0 [6] hazy	-	1 0,2	4 7,2
	γ —	8 6,6 [8] st.	-	6 7,4	1 33,2
30.	β —	0 8,2 [7]	-	4 15,5	4 7,3
	γ —	1 19,2 [7]	-	29 19,2	2 0,0

1739.		Rev. ″			Rev. ″	Rev. ″
Aug. 31.	η Urs. M.	5 13	circiter	-	4 1,3	1 11,7
	β Drac.	2 13,5 [8]	st.	-	6 21,3	4 7,8
	γ —	10 1,2 [7]	st.	-	8 1,4	1 33,8
Sept. 2.	β —	3 13,6 [7]	st.	-	7 21,2	4 7,6
	γ —	8 23,7 [7]	st.	-	6 24,4	1 33,3
3.	η Urs. Maj.	5 5,3 [6]	faint	-	3 25,8	1 13,5
1740.					24 28,0	10 11,3
Jan. 20.	β Drac.	28 23,5 [6]		-	1 29,7	3 6,2
Very hard frost all this month.	γ —	3 19,5 [6]		-	0 20,8	2 32,7
	γ Pers.	28 5,2 [6]		-	6 7,2	8 2,0
	α —	7 1,0 [6]		-	9 6,0	2 5,0
	9 Aurig.	10 6,5 [6]		-	9 13,8	0 26,7
	Capella	9 24,5 [6]		-	6 29,7	2 28,8
	18 Camel.	4 24,2 [3]		-	7 14,2	2 24,0
	δ Aurig.	7 23,0 [6]		-	7 32,0	0 9,0
	35 Camel.	7 10,2 [6]		-	5 21,6	1 22,6
21.	β Cass.	28 16,0 [3]		-	7 1,0	8 19,0
22.	τ Pers.	3 23,7 [6]		-	7 21,7	3 32,0
	γ —	28 7,5 [8]		-	6 8,2	8 0,7
	α —	2 33,5 [8]		-	5 3,8	2 4,3
26.	β Drac.	1 33,0 [3]		-	5 3,5	3 4,5
	γ —	7 16,8 [8]		-	4 14,5	3 2,3

Broke the wire near the bottom, and pieced it again.

29.	θ Pers.	1 23,0 [6]		-	7 5,0	5 16,0

Melted the wire and put on a new one, (wire all used now.)

	Capella	4 14,4 [8]		-	1 18,1	2 30,3
	18 Camel.	3 18,0 [3]		-	6 7,5	2 23,5
May 30.	ε Urs. Maj.	7 4,0 [8]	st.	-	6 9,5	0 28,5
	ζ ———	8 8,5 [8]	st.	-	7 1,5	1 7,0
	η ———	8 17,5 [8]		-	6 24,7	1 26,8
31.	ε ———	8 29,0 [4]	v. haz.		7 33,0	0 30,0
	ζ ———	5 15,7 [8]		-	4 9,0	1 6,7
	η ———	5 25,0 [5]	flutt.	-	3 32,0	1 27,0
June 1.	ε ———	6 15,8 [8]		-	5 20,8	0 29,0
	ζ ———	5 17,0 [6]		-	4 10,2	1 6,8
	η ———	4 6,7 [6]		-	2 13,7	1 27,0
2.	α Cass.	28 29,0 [6]		-	3 30,0	5 1,0
	Capella	9 13,0 [4]	haz.	-	6 9,0	3 4,0
	γ Urs. M.	14 21,5 [6]		-	15 3,5	0 16,0
					6 7,0	8 14,5

1740.		Rev. ″		Rev. ″	Rev. ″
	ε Urs. M.	6 24,7 [8]	-	5 29,7	0 29,0
	ζ ——	8 11,3 [8]	-	7 4,0	1 7,0
	η ——	7 25,8 [8]	-	5 32,0	1 27,8
June 3.	Capella	8 16,7 [4] v. haz.		5 11,2	3 5,5
	γ Urs. M.	4 19,5 [4] haz.	-	5 0,5 26 4,0	0 15,0 8 15,5
	ε ——	0 8,7 [8]	-	29 13,7	0 29,0
	ζ ——	3 13,5 [7]	-	2 7,0	1 6,0
	η ——	2 31,0 [8]	-	1 5,0	1 26,0
4.	Capella	5 31,5 [3] v. haz.		2 31,2	3 0,3
	ε Urs. M.	4 1,0 [6]	-	3 7,7	0 27,3
	ζ ——	3 12,0 [8]	-	2 6,2	1 5,8
	η ——	3 32,0 [8]	-	2 6,0	1 26,0
6.	γ ——	2 27,2 [6]	-	3 8,2	0 15,0
	ε ——	7 15,0 [6]	-	6 21,0	0 28,0
	η ——	7 31,0 [8]	-	6 4,0	1 27,0
10.	ε ——	6 20,5 [7]	-	5 27,2	0 27,3
	ζ ——	7 4,5 [8]	-	5 33,0	1 5,5
	η ——	5 12,7 [8]	-	3 22,3	1 24,4
Aug. 9.	β Drac.	1 17,5 [8]	-	5 18,0	4 0,5
	γ —	5 23,0 [4]	-	3 17,5	2 5,5
18.	β —	3 11,6 [8] st.	-	7 12,6	4 1,0
	γ —	2 6,7 [8] st.	-	0 2,5	2 4,2
19.	β —	0 0,7 [8] st.	-	4 1,7	4 1,0
	γ —	5 16,0 [8] st.	-	3 11,8	2 4,2
21.	β —	0 10,1 [8]	-	4 12,3	4 2,2
	γ —	6 3,5 [8]	-	4 0,3	2 3,2
24.	β —	29 27,9 [8] st.	-	3 29,1	4 1,2
	γ —	4 9,0 [8] st.	-	2 4,8	2 4,2
25.	β —	29 4,3 [8] st.	-	3 6,3	4 2,0
	γ —	4 16,0 [6]	-	2 13,0	2 3,0
28.	β —	1 25,0 [8] st.	-	5 26,5	4 1,5
	γ —	6 26,0 [8] st.	-	4 22,5	2 3,5
29.	β —	3 20,7 [8] st.	-	7 22,4	4 1,7
	γ —	9 30,0 [8]	-	7 25,6	2 4,4
30.	γ —	9 30,5 [8]	-	7 26,7	2 3,8
Sept. 5.	β —	1 13,2 [4] haz.	-	5 13,2	4 0,0
	γ —	4 0,8 [8] st.	-	1 31,2	2 3,6
8.	β —	2 17,7 [8] st.	-	6 18,7	4 1,0
	γ —	7 24,6 [8] st.	-	5 20,4	2 4,2

1740.		Rev. ″		Rev. ″	Rev. ″
20.	β Drac.	1 22,7 [8] st.	-	5 21,7	3 33,0
1741.	γ —	7 24,0 [8] flutt.	-	5 19,5	2 4,5
Jan. 26.	β —	0 31,7 [8]	-	3 28,7	2 31,0
	γ —	6 14,7 [8]	-	3 9,0	3 5,7
	Capella	5 11,2 [8]	-	2 22,5	2 22,7
Aug. 27.	β Drac.	30 29,8 [8]	-	4 25,0	3 29,2
	γ —	4 20,8 [8]	-	2 12,5	2 8,3
28.	β —	1 16,8 [6]	-	5 12,0	3 29,2
	γ —	8 22,9 [8]	-	6 14,5	2 8,4
29.	γ Urs. Maj.	7 5,5 [6] flutt.	-	6 24,2	0 15,3
	β Drac.	3 27,5 [8]	-	7 22,3	3 28,8
	γ —	2 25,2 [8]	-	0 17,0	2 8,2
31.	γ Urs. Maj.	1 14,2 [6] flutt.	-	0 30,7	0 17,5
	ε ———	5 5,0 [8]	-	3 13,7	1 25,3
	β Drac.	0 24,0 [8]	-	4 20,0	3 30,0
	γ —	6 24,5 [8] st.	-	4 16,8	2 7,7
Sept. 1.	ε Urs. M.	3 15,7 [6]	-	1 25,5	1 24,2
	γ Drac.	27 28,5 [8]	-	25 19,8	2 8,7
2.	β —	2 5,5 [8]	-	6 1,2	3 29,7
	γ —	6 9,2 [8]	-	4 1,0	2 8,2
3.	ε Urs. M.	7 10,8 [8]	-	5 20,2	1 24,6
	ζ ———	11 1,5 [6] flutt.	-	9 1,5	2 0,0
4.	ζ ———	5 33,5 [6] flutt.	-	3 32,5	2 1,0
	η ———	6 16,5 [6]	-	3 33,5	2 17,0
6.	β Drac.	29 11,5 [8] st.	-	3 6,5	3 29,0
	γ —	4 16,7 [8]	-	2 9,2	2 7,5
20.	γ —	4 20,2 [8]	-	2 11,8	2 8,4
21.	ε Urs. M.	5 2,0 [6] flutt.	-	3 5,0	1 31,0
	ζ ———	5 19,0 [6] flutt.	-	3 11,5	2 7,5
	η ———	7 5,2 [8]	-	5 16,2	2 23,0
	β Drac.	28 0,8 [8]	-	1 28,8	3 28,0
	γ —	4 17,3 [8] st.	-	2 8,6	2 8,7
23.	ε Urs. M.	1 13,5 [8]	-	29 15,0	1 32,5
	ζ ———	29 11,3 [8]	-	27 3,8	2 7,5
	η ———	30 23,7 [8]	-	28 0,5	2 23,2
1742.	γ Drac.	4 16,7 [8] st.	-	2 8,1	2 8,6
Sept. 5.	β —	29 15,3 [7]	-	3 5,0	3 23,7
	γ —	5 19,0 [8] st.	-	3 9,5	2 9,5
6.	γ Urs. M.	2 0,0 [4] flutt.	-	0 30,0	1 4,0
	ε ———	1 24,6 [8]	-	29 14,1	2 10,5
	ζ Urs. M.	3 7,0 [4] flutt.	-	0 18,0	2 23,0

1742.		Rev. ″		Rev. ″	Rev. ″
	β Drac.	1 3,7 [8] st.	-	4 27,0	3 23,3
1743.					
Sept. 2.	β —	1 24,7 [8] st.	-	5 7,7	3 17,0
	γ —	7 18,2 [8] st.	-	5 3,7	2 14,5
3.	β —	25 22,5 [8]	-	29 6,0	3 17,5
1745.					
Sept. 2.	β —	1 16,5 [6]	-	4 23,0	3 6,5
	γ —	9 21,5 [4]	-	7 1,7	2 19,8
3.	γ —	1 15,5 [9] st.	-	28 29,7	2 19,8
5.	ε Urs. Maj.	5 17,5 [5]	-	1 7,3	4 10,2
	η ———	11 9,0 [6] haz.	-	6 11,2	4 31,8
	β Drac.	6 2,5 [8] st.	-	9 9,3	3 06,8
	γ —	9 18,5 [8]	-	6 33,2	2 19,3

By observations made at Greenwich with the mural quadrant, between the 10th and 14th of September 1745, the difference between the apparent zenith distances of γ and β Draconis was found to be 58′ 16″; which, being by the preceding observations at Wansted and the experiments made December 27th, 1734, equal to 103 rev. 32″ by the screw, shews that one revolution of the screw is 33″,636. This result differs but $\frac{1}{10}$th of a second in six revolutions of the screw, from what was before collected from those experiments. I may therefore suppose that 92 divisions on the plate of the screw exactly correspond to 91″ of a degree, or 93 div. to 92″.

1746.						
Sept. 15.	β Drac.	29 15,0 [4] cl.	-	2 20,2	3 5,2	
	γ —	14 32,0 [6]	-	12 12,2	2 19,8	
19.	ε Urs. M.	13 6,7 [6]	-	8 4,5	5 2,2	
	ζ ——	7 7,0 [4] faint	-	2 0,0	5 7,0	
	η ——	7 27,8 [4] cl.	-	2 4,5	5 23,3	
	β Drac.	1 27,7 [6]	-	4 33,2	3 5,5	
20.	β Urs. M.	4 33,0 [4]	-	6 5,5	1 6,5	north
	γ ——	7 0,5 : :	-	3 3,7	3 30,8	
	ζ ——	5 23,0 [4]	-	0 11,5	5 11,5	
	η ——	6 29,5 [6]	-	1 7,0	5 22,5	
	β Drac.	0 30,0 [8]	-	4 0,0	3 4,0	
	γ —	6 10,0 [8]	-	3 25,0	2 19,0	
23.	γ Urs. M.	9 18,5 [4]	-	5 20,8	3 31,7	
	ε ——	6 2,0 [6]	-	0 32,5	5 3,5	
1747. Feb. 27.	β Cass.	1 9,0 [8]	-	4 31,5 5	3 22,5 3 27,5	north 32 15
	α —	5 23,7 [8]	-	6 23,2	0 33,5	34 50

1747.		Rev. ″		Rev. ″	Rev. ″	
	α Pers.	2 26,0 [8]	too near	8 6,5 : :	5 14,5	
	Capella	7 27,5 [8]	too bright	6 20,5	1 7,0	
	δ Aurigæ	5 18,0 [6]	-	6 23,2	1 5,2	north
	35 Camel.	4 33,7 [8]	-	4 1,0 q.	0 32,7	
	β Urs. M.	3 20,0 [7]	-	4 30,0 : :	1 10,0	north 32 15

° ′ Rev. ″ ° ′ Rev. ″
34 50 | 3 23,2 | 34,55 | 12 19,2 Diff.=8 30 windy.

γ Urs. M.	8 33,7 [8]	-	4 19,7	4 14,0
ε ———	6 32,0 [4] cl.	-	1 6,5	5 25,5

° ′ Rev. ″ ° ′ Rev. ″
32 15 | 1 10,7 | 32 20 | 10 8,7 Diff.=8 32,0.
34 50 | 0 31,5 | 34 55 | 9 28,2 Diff.=8 30,7.

I found upon examination, that the north end of the arch on which the points are made is at present about $\frac{1}{40}$th of an inch farther from the centre of the axis than the south end.

Aug. 31.	γ Drac.	4 18,5 [8]	-	1 33,5	2 19,0

Aug. 31. I found the shutter open, the string being broken; but no rain seemed to have fallen upon the instrument, because the papers on the arc had not been wetted.

Sept. 1.	β Drac.	0 23,3 [8]	-	3 25,8	3 2,5

° ′ Rev. ″ ° ′ Rev. ″
32 15 | 29 29,8 | 32 20 | 8 28,0 Diff.=8 32,2

γ Drac.	7 20,2 [9]	-	5 1,0	2 19,2

The nut that moves the limb to or from the plumbline not turning without great force, I oiled the screw, but not finding it to work any better, I endeavoured to take off the whole in order to rectify what was amiss; but upon trial, I found that whilst the telescope remained suspended, I could only unscrew the female part of the screw that was fastened to the sliding piece that carries the rollers. But this being detached, I could then easily push the limb in or out, so as to bring it to a proper distance from the plumbline. I likewise rotated the telescope about, (by near $\frac{1}{2}$ a turn of the screw at the top,) so as to bring the south roller to bear against the fixed brass arch, when the telescope was nearly vertical; and afterwards brought the top of the telescope a little more easterly, that the plumbline might be near enough to the limb at the south end.

° ′ Rev. ″ ° ′ Rev. ″ Rev. ″
32,30 | 28 30,3 | 32 35 | 7 27,5 Diff.=8 31,2.

1747.		Rev. ″		Rev. ″	Rev. ″
Sept. 2.	ζ Urs. M.	5 0,0 [4] faint	-	29 10,0	5 24,0
	η ———	6 26,7 [6]	-	0 21,2	6 5,5
	β Drac.	0 27,0 [9]	-	3 30,5	3 3,5
	γ —	6 30,7 [10]	-	4 11,7	2 19,0
	Entering	6 25,5			
	Going out	6 28,0			

	° ′	Rev. ″	° ′	Rev. ″	° ′	Rev. ″	Rev. ″
3.	34 45	26 20	34 50	5 16	34 55	14 13	8 30,0 8 31,0

° ′	Rev. ″	° ′	Rev. ″	Rev. ″
37 30	25 16,1	37 45	21 7,5	Diff.=26 25,4
37 45	24 32,5	38 0	21 24,2	Diff.=26 25,7
38 0	24 3,6	38 15	20 31,0	Diff.=26 27,4
38 15	24 17,8	38 25	12 13,0	Diff.=17 29,2

Diff. between 37° 30′ and 38° 25′ is=98 5,7

3.	ε Urs. M.	4 26,0 [3] cl.	-	29 2,5	5 23,5
	η ———	6 17,7 [6]	-	0 13,7	6 4,0

By the experiments made Sept. 3d, and the observations made this month on β and γ Drac., their apparent distance from each other was 103 rev. 26″,8 by the instrument at Wansted. The apparent distance of the same stars was at the same time observed at Greenwich to be (by the mural quadrant) 58′ 9″½. This gives one revolution of the screw = 33″,6213. Hence 90 divisions on the index of the micrometer is 89″.

The importance of the Wansted Observations made it right in printing them to adhere as closely as possible to the original; but as some variations have been thought advisable, it is proper to state the full extent of them.

It was found necessary sometimes to make a slight transposition; as for example, in the observation of ζ Cephei in page 214, where 19 0,9 is made to range with the column to which it belongs, instead of being left, as it was written, immediately after 30′ 6″.

The particular division on the limb with which the position of the stars was compared is repeatedly given in the earlier part of the collection. Of these there were three which have been corrected: in p. 210, 35° 45′ is assigned to σ Cass. although 34° 45′ is the quantity annexed to the observation in the manuscript; in p. 211, for λ Cass. 36° 55′ has been in the same manner substituted for 34° 55′; and in p. 216, 32° 30′ instead of 33° 30′ has been annexed to ε Ursæ Majoris.

Again, we read in the manuscript,

1727,	α Cass.	1728,	β Drac.	1728,	β Drac.	1728,	21 Can. Venat.
Sept. 11.	2υ —	Feb. 16.	ι Drac.	March 7.	ι —	Ap. 16.	ζ ——
	θ —		γ —		γ —		
	ξ —						
1728,	21 Can. Venat.	1728,	ι Hercul.	1729.	38 Camel.		
May 5.	ζ ——	Aug. 12.	ξ ——	Oct. 8.			
	η ——		γ Drac.				

Now in each of these instances there is an oversight which is so evident that there could be no doubt about correcting it, as may be seen in the printed text, and in pages 204, 214, 1π has been given, although π simply appears in the MS. The same ought to have been done in p. 206. but the error was not observed in time.

There are, however, two other stars which could not be so easily disposed of. In 1727, we find observations on Sept. 9, 14, 15 and 19, of what is called 136 Draconis, and on the 30th of Aug. and 1st of Sept. we meet with 138 Draconis, although the whole constellation contains but 80 stars in Flamsteed's Catalogue, to which Bradley always refers. The numbers are too clearly written for it to be possible that any mistake has been made in reading them, and some other cause must be sought for to solve the difficulty. We may assume that the observations were registered in the order in which they were made, and from thence it appears that both the stars in question passed the meridian after d Draconis and before ϰ Cygni. Their right ascensions therefore must lie between 277° and 288°: moreover, 136 was compared with the approximate N. P. D. of 34° 40′ [a], and 138 with that of 37°.[b] These quantities will answer very well to the 49th and 51st of Draco in Flamsteed, and there is a circumstance which might very probably have occasioned the mistake which appears to have been made. In the 3d vol. of the Historia Cœlestis there is no column of numbers annexed to the British Catalogue, and it is necessary to reckon on from the beginning of each constellation to find the particular star which may be wanted. Now Ursa Major, which immediately precedes Draco, contains 87 stars, and $87+49=136$, $87+51=138$. It is not difficult, therefore, to imagine that the reckoning by some oversight might have been begun with Ursa instead of Draco, and the difference is immediately accounted for. There are likewise some loose sheets of paper on which Bradley had written his observations (up to Sept. 15, 1727) before he copied them out more fully in his book. In these memoranda 138 is mentioned as observed on the 30th of August, at 18h. 58′ S. T. which will answer very nearly to the R. A. of 51 Draconis. All therefore seems to point to the same corrections; but as they rest only on conjecture, they have not been admitted into the text.

In p. 223, line 15, 1′ is added to $1''\frac{1}{2}$, which, by some evident omission, is the whole which is stated in the MS. And in p. 284, line 8, 5 is substituted for 4 rev.

In printing the observations themselves, the greatest care has been taken to preserve every figure exactly as it was written. It may be thought that this has been done too scrupulously, when, as in page 235, line 9, the 0 is retained in the first place of the seconds; but it was observed that the cipher was generally introduced in these cases to obliterate what had been found to be wrong, (as, in the instance just alluded to, the quantities had been originally written 14″,6 and 15″,8,) it appeared therefore that there was a possible use in retaining it.

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[a] See page 211.

[b] See page 207.

The difference of the quantities in the first and second columns is given in the third: after the proof sheets had been compared with the original, the actual subtraction was therefore made for every single observation, and the differences for each particular star were compared together; by this means some discrepancies were detected, but when they were found to belong to the manuscript, which in these cases was always referred to, no correction was attempted. Generally, indeed, none could have been devised with any certainty. For example, on April 17, 1728, the true difference for ψ Ursæ Maj. is 0 23,3 and not 0 23,0; but there are no means of ascertaining whether the mistake was made in the subtraction, or in copying out the entries from the paper on which the observation was originally noted. Again, on Sept. 12, 1731, the subtraction for η Ursæ is right; but the quantity in the third column ought to be 6 15,5 instead of 5 15,5. We can, however, advance no farther, for there is the general difficulty in determining whether it is the first or the second column to which the correction ought to be applied.

Demonstration of the Rules relating to the Apparent Motion of the Fixed Stars upon account of the Motion of Light.

IF we suppose the distance of the fixed stars from the sun to be so great that the diameter of the earth's orbit viewed from them would not subtend a sensible angle, or which amounts to the same, that their annual parallax is quite insensible; it will then follow that a line drawn from the earth in any part of its orbit to a fixed star, will always, as to sense, make the same angle with the plane of the ecliptic, and the place of the star, as seen from the earth, would be the same as seen from the sun placed in the focus of the ellipsis described by the earth in its annual revolution, which place may therefore be called its true or real place.

But if we further suppose that the velocity of the earth in its orbit bears any sensible proportion to the velocity with which light is propagated, it will thence follow that the fixed stars (though removed too far off to be subject to a parallax on account of distance) will nevertheless be liable to an aberration, or a kind of parallax, on account of the relative velocity between light and the earth in its annual motion.

For if we conceive (as before) the true place of any star to be that in which it would appear viewed from the sun, the visible place to a spectator moving along with the earth, will be always different from its true, the star perpetually appearing out of its true place more or less, according as the velocity of the earth in its orbit is greater or less; so that when the earth is in its perihelion, the star will appear farthest distant from its true place, and nearest to it when the earth is in its aphelion; and the apparent distance in the former case will be to that in the latter in the reciprocal proportion of the distances of the earth in its perihelion and its aphelion. When the earth is in any other part of its orbit, its velocity being always in the reciprocal proportion of the perpendicular let fall from the sun to the tangent of the ellipse at that point where the earth is, or in the direct proportion of the perpendicular let fall upon the same tangent from the other focus, it thence follows that the apparent distance of a star from its true place, will be always as the perpendicular let fall from the upper focus upon the tangent of the ellipse. And hence it will be found likewise, that (supposing a plane passing through the star parallel to the earth's orbit) the locus or visible place of the star on that plane will always be in the circumference of a circle, its true place being in that diameter of it which is parallel to the shorter axis of the earth's orbit, in a point that divides that diameter into two parts, bearing the same proportion to each other, as the greatest and least distances of the earth from the sun.

Let PTA (Fig. 6.) represent the ellipse described by the earth about the sun, placed in the focus at S; F the other focus, A the aphelion, and P the perihelion of the earth. Upon the greater axis of the ellipse (as a diameter) describe a circle PNOA. Let T represent the place of the earth in its orbit, NTO the tangent at the point T cutting the circle in N and O, then by the property of the ellipse the lines SN, FO drawn from the foci S, F to the points N and O will be perpendicular to the tangent NO, and the velocity of the earth in its orbit will be reciprocally as SN, or directly as FO. We may therefore consider the earth when in the point T as moving in the direction of the tangent NTO with a velocity proportional to FO, and since the alteration of a star's place on account of the motion of light is proportional to the earth's velocity, (that of light being supposed always the same,) and this alteration being made that way towards which the earth moves; if we conceive S to represent the true place of a star, and through S draw SM parallel to NTO, the line in which the earth is moving when 'tis at the point T, the star will appear somewhere in the line SM, and if SM be always taken proportional to FO or the earth's velocity, then M will represent the point in which the star will appear.

Now SM and FO being by construction always at right angles to each other, and bearing a constant proportion to one another, the extremities of those lines must necessarily describe similar figures about the respective points F and S. But the point O is always in the circumference of the circle PNO: therefore the point M will be likewise in the circumference of a circle MRQV.

And the lines SM, SR, SV, having a given proportion to the lines FO, FA, FP, &c. and always at right angles with them, it follows that the diameter RV of the circle MRQV will be divided in the point S in the same proportion as the diameter PA of the circle PNOA; that is, in the proportion of the earth's perihelion distance to its aphelion distance. Hence if we suppose SM = FO and at right angles to it, the locus of the point M will be a circle whose diameter RV = the greater axis of the ellipse, and the point S, representing the true place of the star, will divide the diameter RV, that is at right angles to the greater axis of the earth's orbit, in the same proportion as that axis itself is divided in the point S, considered as its focus. So that SA = SV, and SP = SR, and when the earth is in its aphelion A, the visible place of the star will be at R, its visible distance being = SR = FA; but when the earth is in its perihelion at P, the star's visible place will be at V, and its visible distance from its true place S will be SV. Hence then it appears, that if we conceive planes parallel to that of the ecliptic passing through any fixed stars, the locus visibilis in such planes of each star will be the circumference of a given circle, each star having an apparent motion about its true place, which may be represented by a point in that diameter of the circle that is parallel to the lesser axis of the earth's orbit, which divides it in the proportion as perihelion distance of the earth to its aphelion distance, and the star will appear in that extremity of the diameter which is least distant from the point representing the true place when the earth is in its aphelion, and at the other extremity when the earth is in its perihelion; that is, when the earth is at A, that the star will appear as at R; and when at P, the star will appear at V.

In both these cases the visible place of the star (considered as viewed from the point S, which represents its true place, and about which it seems to have an annual motion in the circumference of the circle RVM) is just 90 degrees behind the sun's true place; but when the sun is in any other part of its orbit, the star's visible place is either more or less than 90 degrees behind the sun's true place; viz. more whilst the earth moves from its perihelion to its aphelion, and less whilst 'tis moving from the aphelion to the perihelion. Thus the angle BSM, which represents the difference between the star's visible place and the sun's, is greater than 90°, the earth at T being going towards the aphelion.

The difference between the angle BSM and 90° is always equal to half the angle STF made by the lines TF, TS drawn from the earth at T to the foci of its orbit. For MSQ being parallel to the tangent NTO, and SN perpendicular to it, the angle BSM = QST is greater than the right angle QSN by the angle NST = by the property of the ellipse to half the angle STF.

Hence the distance of the earth[a] from its aphelion or perihelion being given and the excentricity of its orbit, the angle NST may be found, which, added to or subtracted from 90 degrees, as the case requires, will give the angle MSB.

From the same data may be found the perpendicular FO, which is proportional to the earth's velocity at T, and to which SM is in a constant ratio.

Knowing therefore the diameter $RV = 40''\frac{1}{2}$, we may find SM and the angle BSM, having the sun's place, &c.

The following Table contains the equation, which, added to or subtracted from the sun's place, will give the longitude of a point 90° or 3^s before M, as also the line SM in such parts as the diameter RV contains $40''\frac{1}{2}$, which is supposed equal to the diameter of the circle described by a star on account of the motion of light.

Dist. from perihelion, or angle PST.		The angle made by the tangent at the earth, or the angle NTS.			The logarithm of FO proportional to the earth's velocity at T.	Dist. from perihelion, or angle PST.		Sun's true place in the Ecliptic.		The Equation to be applied to the Sun's place.		The Logarithm of SM.	SM.	Sun's Longitude in the Ecliptic.	
										Subtract					
S.	°	°	′	″				S.	°	′	″		″	S.	°
0	0	90	0	0	007286			9	8	0	0	31371	20,59	9	8
	10	89	50	0	007226		20		18	10	0	31365	20,59		28
	30	89	40	30	006858		10		28	19	35	31328	20,57		18
1	0	89	31	20	006334	11	0	10	8	28	40	31276	20,55	8	8
	10	89	23	10	005619		20		18	36	55	31204	20,51		28
	20	89	16	0	004734		10		28	44	5	31116	20,47		18
2	0	89	10	0	003703	10	0	11	8	49	55	31013	20,42	7	8
	10	89	5	40	002560		20		18	54	20	30898	20,37		28
	20	89	2	50	001333		10		28	57	5	30776	20,31		18
3	0	89	1	50	000060	9	0	0	8	58	10	30648	20,25	6	8
	10	89	2	30	998783		20		18	57	25	30521	20,19		28
	20	89	5	0	997534		10		28	55	0	30396	20,14		18
4	0	89	9	10	996355	8	0	1	8	50	50	30278	20,08	5	8
	10	89	15	0	995293		20		18	45	0	30172	20,03		28
	20	89	22	10	994367		10		28	37	50	30079	19,99		18
5	0	89	30	30	993609	7	0	2	8	29	30	30003	19,95	4	8
	10	89	39	50	993064		20		18	20	10	29949	19,93		28
	20	89	49	40	992720		10		28	10	15	29914	19,91		18
6	0	90	0	0	992589	6	0	3	8	0	0	29901	19,91	3	8
								Loc. Sol.		Add.				Loc. Sol.	

In this Table the sun is supposed to be in its apogee when its apparent place is 3^s 8°.

In γ Draconis this correction may be neglected, the difference arising therefrom scarce exceeding $\frac{1}{10}$th of a second.

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[a] "Sun" in MS.

Let it be required to find the situation of the point M and the distance SM, when the sun's longitude is $10^{s}\,15^{\circ}$.

Here the equation to be subtracted being $34'\,25''$[b], the equated place of the sun will be $10^{s}\,14^{\circ}\,25'\,35''$, and the longitude of the point M will be $7^{s}\,14^{\circ}\,25'\,35''$, and $SM = 20'',52$.

Now since 'tis supposed that the plane in which the circle MRBV lies, and in which the point M moves, passes through the star parallel to the plane of the ecliptic, if we imagine S the true place of the star, and QSO (fig. 7.) the intersection of this plane, with another at right angles to it passing through the pole of the ecliptic and the star's true place, which may be called the plane of the star's longitude, when the point M coincides with the points O and Q, the star's visible longitude will be the same as its true; but in any other situation of the point M the visible longitude will be different from the true. When, therefore, the sun's place, equated as before, precedes the star's true longitude just 3^{s} or 9^{s}, the star's visible longitude will be the same as its true, the parallax in longitude in both those cases vanishing, and the point M coinciding with the points O or Q; but in any other situation of the sun there will be a parallax of longitude, viz. to be added when the sun's place equated—star's longitude is between 3^{s} and 9^{s}, and the contrary when between 9^{s} and 3^{s}.

If through S be drawn CSN at right angles to QSO, this may represent the parallel of latitude passing through the star's true place; when, therefore, the point M coincides with N or C, the apparent latitude of the star will be the same as its true; but in any other situation of the point M, there will be a parallax of latitude to be subtracted whilst the point M is moving from N through O to C, and the contrary whilst M is moving from C through Q to N.

Now the longitude of the point M being always 90° behind the sun's equated place, when the sun's equated place is in O, that is, when the sun's equated place is the same with the star's true longitude, the point M will be at N, therefore then the true and visible latitude will be the same; as it will also when M comes to C, in which case the sun's equated place will be at Q. So that when the sun's equated place is either in conjunction with or opposition to the star in longitude, there will be no parallax of latitude, but in every other situation of the sun there will, the star's visible latitude being less than the true, while M is passing from N to C, that is, from the conjunction of sun's equated place with the star, to its opposition thereto; and the contrary, from the opposition to the conjunction.

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[b] The numbers in the original are $36'\,25''$; $10^{s}\,14^{\circ}\,23'\,25''$ and $7^{s}\,14_{\circ}\,23'\,35''$.

Whilst, therefore, the distance of the sun's equated place from the star's longitude is between 0^s and 6^s, the parallax must be subtracted, and the contrary whilst the sun's equated place from the star is greater than 6 and less than 12 signs.

Hitherto we have considered the apparent motion of the star about its true place, as made only in a plane parallel to the ecliptic, in which case it appears to describe a circle in that plane; but since, when we judge of the place and motion of a star, we conceive it to be in the surface of a sphere, whose centre is our eye, 'twill be necessary to reduce the motion in that plane to what it would really appear on the surface of such a sphere, or (which will be equivalent) to what it would appear on a plane touching such a sphere in the star's true place. Now in the present case, where we conceive the eye at an indefinite distance, this will be done by letting fall perpendiculars from each point of the circle on such a plane, which from the nature of the orthographic projection will form an ellipsis, whose greater axis will be equal to the diameter of that circle, and the lesser axis to the greater as the sine of the star's latitude to the radius, for this latter plane being perpendicular to a line drawn from the centre of the sphere through the star's true place, which line is inclined to the ecliptic in an angle equal to the star's latitude; the touching plane will be inclined to the plane of the ecliptic in an angle equal to the complement of the latitude. But it is a known proposition in the orthographic projection of the sphere, that *any circle inclined to the plane of the projection, to which lines drawn from the eye, supposed at an infinite distance, are at right angles, is projected into an ellipsis, having its longer axis equal to its diameter, and its shorter to twice the cosine of the inclination to the plane of the projection, half the longer axis or diameter being the radius.*

Such an ellipse will be formed in our present case, if from each point of the circle, as M, be let fall a perpendicular MD (fig. 8.) upon the line CSN, the common intersection of the planes; (which may likewise here be conceived as the parallel of latitude passing through the star's true place S) and DM be divided in G, so that DM : DG :: radius : the sine of the star's latitude, for the point G will by its motion describe an ellipsis whose greater axis is equal to the diameter of the circle CONQ, and whose lesser is to the greater as the sine of the star's latitude to the radius.

If the point S were in the centre of the circle, it would be so likewise of the ellipse, and in that case the star would appear to describe an ellipsis whose greater axis was to the less as the radius to the sine of its latitude, the star's true place being in the centre of that ellipse, the greater axis coinciding with the parallel of latitude drawn through the star; and this would be the case if the earth's velocity in its orbit was always the same; but since it is not, the point S representing the true place of the star will be always out of the centre of the circle OCQN, and likewise of the ellipse which the star seems to describe about its true place; and the longer axis, instead of coinciding with CSN, the parallel of latitude[c] drawn through the true place will be parallel to it; but in other respects the ellipse will be in every thing the same.

But without considering the situation of the centre of the ellipse, we may find the difference between the true and apparent place of the star at any given time, by supposing that M is the apparent place in the circle found as before described, this reduced to the plane touching the surface of the globe will be represented by the point G in the ellipse, so that SG will be the apparent distance of the star from its true place S, as seen from the earth or sun.

Now DGM being perpendicular to CSN the parallel of the star's true latitude, DG will be the parallax of latitude, and DS the arc intercepted between the circles of longitude passing through the true and apparent longitudes of the star, which arc will be to the parallax of longitude as the cosine of the star's latitude to the radius. Hence MS being given, and the angle MSO, DG and DS may be found, and therefore the parallax in latitude and longitude for any given time.

MS being the radius, DS is = sine of the angle DMS = MSO; but from what has already been said, the sine of MSO is = to the cosine of the sun's equated longitude from the star's = cosine ∠ BSO; therefore MS : DS : : rad. : cosine BSO; but it was before observed that DS is to parallax of longitude as the cosine of the star's latitude to the radius, therefore the cosine of the star's latitude is to the cosine of BSO, (= to sun's equated place from the star,) so is SM to the parallax of longitude.

Hence if SM were a constant quantity, that is, if the earth's velocity was always the same, the parallax of longitude upon account of the motion of light would be always as the cosine of the sun's longitude from the star's;

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[c] "Declination" in the MS.

whereas the parallax of longitude on account of distance would be as the sine of the sun from the star.

Again, MS : MD : : rad. : sine MSD, and by the construction

MD : GD : : rad. : sine latitude of the star; therefore

rad.^2 : sine MSD × sine latitude : : MS : GD = to the parallax of latitude, but the sine of DSM is equal to the sine of BSO, the sun's distance from the star; the angle DSM being = OST, the complement of BSO to 180°.

Hence if SM were a constant quantity, DG the parallax of latitude would be as the sine of sun from the star, whereas the parallax of latitude on account of distance is as the cosine of the sun's distance from the star.

Whilst the point M is passing from N through O to C, the apparent place of the star G is nearer the ecliptic than S, the true place, and the parallax of latitude is to be subtracted; and the contrary while it is passing from C through Q to N. Now SM being always at right angles to TSB, the sun's equated place, when M is at N, B will be at O; that is, M will be at N when the sun's equated place is the same as the star's longitude, therefore from the time of the conjunction of the sun (equated as before) with the star to its opposition, the parallax of latitude must be subtracted, and the contrary from the time of the opposition to the conjunction.

Whilst M is moving from O through C to Q, the apparent longitude will be greater than the true, or the parallax of longitude added to the true longitude will give the apparent; that is, whilst the sun's equated place is more than 3^s and less than 9^s from the star, and the contrary when more than 9^s and less than 3^s.

$$\text{Therefore the parallax of latitude}\begin{cases}\text{adds when } \odot - \text{star} = 6, 7, 8, 9, 10, 11, 0 \text{ signs}\\ \text{subtracts when } \odot - \text{star} = 0, 1, 2, 3, 4, 5, 6 \text{ signs}\end{cases}$$

$$\text{and parallax of longitude}\begin{cases}\text{adds when } \odot - \text{star} = 3, 4, 5, 6, 7, 8, 9 \text{ signs}\\ \text{subtracts when } \odot - \text{star} = 9, 10, 11, 0, 1, 2, 3 \text{ signs.}\end{cases}$$

In order to find the parallax of declination and right ascension, let us suppose as before M (Fig. 9.)[d] the apparent place of the star in the plane parallel to the ecliptic, the circle MBQN representing its motion in that plane, and G the star's apparent place on the surface of the globe, or a plane touching it in the star's true place, the ellipse GCN representing the locus of its apparent place on that plane.

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[d] On this figure the liberty has been taken of altering some of the marks of reference: the points in the engraving, which are distinguished by b, c, d, e, f, λ′, L′, K′, W′, are denoted in the MS. by 1, 5, 2, 3, 4, 2λ, 2L, 2K, 2W.

Through S the star's true place draw SK, making the angle CSK equal to the angle of position, or the angle at the star made by two great circles passing through the poles of the ecliptic and equator; then because CS (by what has been said already) represents the parallel of latitude, SK will represent the parallel of declination.

In like manner, making the angle OSR = CSK or the angle of position, SO representing the circle of longitude, SR will represent the circle of right ascension. The lines CS, SO being at right angles, and the lines KS, RS.

From G, the star's apparent place in the ellipse, let fall GI, GE perpendicular to SK, SR; then GI will be the parallax of declination or the difference between the true and apparent declination, and GE = IS will be the arc on the parallel of the star's declination intercepted between the circles of the star's true and apparent right ascension, and which will be to the parallax of right ascension as the cosine of the star's declination is to the radius.

The situation of the point M and the distance SM being found, as before is shewn, IG and GE may be found also. For if through K, where the parallel of declination SK cuts the ellipse, be drawn the line LKW perpendicular to SC the circle of latitude, meeting the circle in L, 'tis evident that when the point M coincides with L, the point G will be in K, that is, when the point M comes to L, the apparent declination of the star will be the same as its true.

Now the point L, or the situation of the point M when the star's true and apparent declination are the same, may be found, having the angle CSK or the angle of position and the star's latitude; for the tangent of CSK : the tangent of CSL (as KW to LW or) : : the sine of the star's latitude : the radius. If the line LS be continued till it meet the circle again in L′, a perpendicular let fall from thence on CSN the parallel in latitude will cut the ellipse in K′, the same point where the parallel of declination cuts it again; so that when the point M by its motion comes either to L or L′, the true and apparent declination will be the same, but in every other point 'twill be different.

The line MS was before shewn to be always at right angles to the sun's equated place, therefore if qSω be drawn perpendicular to LSL′, when the point M coincides with L the sun's equated place will be at q, and when M coincides with L′ the sun's equated place will be at ω. Now the angle OSω = QSq made by the lines SO, Sω supposed to pass through longitude of the star, and the sun's equated place is by construction equal to the angle LSC, which is therefore equal to the difference of longitude between the sun and star when there is no parallax of declination. If the longitude of the star be in the ascending semicircle of the ecliptic, (which is the case represented in the scheme,) the point ω will be farther back in the ecliptic than the point O, and the contrary in the descending semicircle; this rule holds if the star's latitude be north, but the contrary if south. An angle therefore equal to CSL (found as before described) subtracted from the star's longitude, (if in the ascending semicircle, or between 9^s and 3^s, or added to it if in the descending semicircle, or between 3^s and 9^s,) will give what for distinction I will call the star's equated longitude, or a place in the ecliptic with which the sun's equated place coinciding, the true and apparent declination of the star are the same.

The rule therefore for finding this point is this; let A be the angle of position, then say the sine of the star's latitude : radius : : tang. of A : tang. of B.

If the star's latitude be north, B must be added in the descending semicircle, and subtracted in the ascending, and the contrary if the star's latitude be south; and if the latitude of the star be north, and its longitude 9, 10, 11, 0, 1, 2, 3 signs subtracting B, but if the latitude be north and the longitude be 3, 4, 5, 6, 7, 8, 9 signs adding B to the star's longitude, you will have the point in the ecliptic where the sun is when the parallax of declination vanishes; but the contrary if the latitude be south.

If the point M does not coincide with L or L′, letting the perpendicular MD fall on the parallel of latitude, it will meet the ellipse in G, the star's visible place, from whence letting fall GI perpendicular to SK the parallel of declination, this will be the parallax of declination when the line of the sun's equated place is at right angles to SM.

If MP be let fall from M perpendicular to SL, then MP will be to GI (the parallax of declination) as the sine of LSC to the sine of KSC.[e]

For the perpendicular MGD cutting the lines LS, KS in the points α, β, $D\beta : D\alpha :: WK : WL :: DG : DM$ by construction; therefore $D\beta : DG ::$ $D\alpha : DM$ and $D\beta : DG\text{-}D\beta :: D\alpha : DM\text{-}D\alpha$; that is, $D\beta : \beta G :: D\alpha : \alpha M$, or $D\beta : D\alpha :: \beta G : \alpha M$ and $\frac{D\beta}{D\alpha}=\frac{\beta G}{\alpha M}$ or $\frac{D\alpha}{D\beta}=\frac{\alpha M}{\beta G}$.

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[e] A sentence has been omitted here, as it merely repeated what is contained in the present paragraph.

But $\frac{PM}{IG}=\frac{PM}{M\alpha}\times\frac{M\alpha}{G\beta}\times\frac{G\beta}{IG}$, therefore $\frac{PM}{IG}=\frac{PM}{M\alpha}\times\frac{D\alpha}{D\beta}\times\frac{G\beta}{GI}$; but because the triangle PMα is similar to αDS, and GIβ similar to βDS, $\frac{PM}{M\alpha}=\frac{DS}{S\alpha}$, and $\frac{G\beta}{GI}=\frac{S\beta}{SD}$; therefore $\frac{PM}{GI}=\frac{DS}{S\alpha}\times\frac{D\alpha}{D\beta}\times\frac{S\beta}{DS}=\frac{D\alpha}{S\alpha}\times\frac{S\beta}{D\beta}$. But S$\alpha$: Dα : : rad. : sine LSC, and Dβ : Sβ : : sine KSC : rad.; therefore $\frac{PM}{GI}=\frac{D\alpha}{S\alpha}\times\frac{S\beta}{D\beta}=\frac{\text{sine LSC}}{\text{rad.}}\times\frac{\text{rad.}}{\text{sine KSC}}=\frac{\text{sine LSC}}{\text{sine KSC}}$.

Again, SM : PM : : rad. : sine LSM.
But PM : GI : : sine LSC : sine KSC;
therefore SM : GI : : rad. × sine LSC : sine LSM × sine KSC,
or SM : GI : : $\frac{\text{rad.}\times\text{sine LSC}}{\text{sine KSC}}$: sine LSM.

Since then $\frac{\text{sine LSC}}{\text{sine KSC}}$ is a given quantity in the same star, the parallax of declination will be to SM always as the sine of the angle LSM; and if SM were a given quantity, (or the earth's velocity always the same,) the parallax of declination would be always as the sine of the distance of the point M from L or L′; and when greatest, would be to the semi transverse axis of the ellipse as the sine of KSC to the sine of LSC; that is, as the sine of the angle A to the sine of the angle B before-mentioned.

The point M being always 3^s backwarder in the ecliptic than the sun's equated place, and when M comes to L or L′, the sun's equated place will be at q or ω, as has been before explained, and SM being to parallax of declination GI always as $\frac{\text{rad.}\times\text{sine LSC}}{\text{sine KSC}}$ to sine of the distance of the point M from L or L′, it will follow that SM is always to GI as $\frac{\text{rad. sine LSC}}{\text{sine KSC}}$ is to the sine of the sun's distance from the points q and ω.

Whilst the sun passes from ω through C to q, the star by its apparent motion describes that portion of the ellipse K′NGK which lies nearest the equator, if the latitude and declination of the star are of the same denomination, that is, both north or both south; therefore, in that case, the parallax of declination subtracted from the true place gives the apparent. Whilst the sun, therefore, is passing from the conjunction with the point ω in the ecliptic (found as before shewn) to the opposition, the parallax of declination must be subtracted; but from the opposition to the conjunction, it must be added to the true place. This rule holds provided the star's latitude and declination are both of the same denomination, but if they are different, the parallax must be added from the conjunction to opposition, and subtracted from the opposition to the conjunction.

As an example to illustrate the foregoing rules, let it be required to find the parallax of declination for η Urs. Majoris, when it passed the meridian on March 21, 1727-8.

By Mr. Flamsteed's Catalogue, the star's place was then as follows:

R. Asc.	Decl. Bor.	Longit.	Lat. B.
$204^\circ\ 13'\ 0''$	$50^\circ\ 41'\ 30''$	$5^s\ 23^\circ\ 5'\ 40'$	$54^\circ\ 24'\ 30''$

Hence the angle of position or the angle A is found to be $= 38^\circ\ 38'\ 30''$, and the angle $B = 44^\circ\ 30'\ 50''$, and $\frac{\text{sine A}}{\text{sine B}} = \log.\ 9{,}949721$, hence the longitude of the point $\omega = 7^s\ 7^\circ\ 36'\ 30''$; the sun's true place $= 0^s\ 12^\circ\ 30'$, but his equated place $= 0^s\ 11^\circ\ 32'$; $MS = 20'',22$ $\log. MS = 1{,}30591$.

The distance of the sun's equated place from $\omega = 5^s\ 3^\circ\ 55'\tfrac{1}{2}$; hence the parallax of declination $7'',9$ to be subtracted, so that the star's apparent declination would be $50^\circ\ 41'\ 22'',1$.

If the parallax of declination had been sought upon supposition that the earth's velocity in its orbit was always the same, and without equating the sun's place, the parallax of declination would have been found $7,''7$, differing not a quarter of a second from the truth.

As the parallax of declination may be found by the foregoing rules, so the parallax of right ascension may be by means of other rules now to be investigated.

Let RSr represent the circle of right ascension drawn through the star's true place cutting the ellipse in R,r. Through R draw $xR\lambda$ parallel to SO, meeting the circle in λ, from whence draw $\lambda S\lambda'$. Then, from what has been before shewn, it follows that when the point M coincides with λ, the star's apparent place will be at R; consequently its apparent right ascension will be the same as its true, or the parallax of right ascension will vanish. Now the point λ may be thus found: the angle $OSR = KSC$ the angle of position being known and the latitude of the star, the cotangent of OSR will be to the cotangent of OSλ :: Rx : λx; that is, as the sine of the star's latitude to the radius. The angle OSλ thus found being subtracted from the star's longitude if in the ascending, and added if in the descending semicircle, will give the longitude of the point λ, or the longitude of the point M when the true and apparent right ascensions are the same; and since the point M is always 3^s behind the sun's equated place, 3^s added to the longitude of the point λ will give the sun's equated place in the ecliptic when the true and apparent right ascensions are the same.

If the point M does not coincide with λ, or its opposite point λ′, there will be a parallax of right ascension, which may be found by knowing the angle λSM, or the distance of M from λ.

For let as before G be the apparent place of the star in the ellipse, then GE let fall perpendicular to SR will be the arc of the parallel of declination intercepted by the two (meridians or) circles of right ascension passing through true and apparent places of the star, and the parallax of right ascension will be to GE as the radius to the cosine of the star's declination.

From M let fall Mπ perpendicular to Sλ; also through G draw the line Gbde parallel to CS, (and perpendicular to MD, OS,) meeting the lines SM, Sλ, SR in the points b, d, e, and the line SO in c; also from b draw bf, perpendicular to Sλ, or parallel to Mπ.

Then $\frac{M\pi}{GE} = \frac{M\pi}{bf} \times \frac{bf}{bd} \times \frac{bd}{Ge} \times \frac{Ge}{GE}$.

But $\frac{M\pi}{bf} = \left(\frac{SM}{Sb} = \frac{MD}{GD}\right) = \frac{\text{rad.}}{\text{sine lat.}}$, and $\frac{bf}{bd} = \frac{\text{cosine OS}\lambda}{\text{rad.}}$.

And $\frac{bd}{Ge} = \frac{\text{sine lat.}}{\text{rad.}}$, and $\frac{Ge}{GE} = \frac{\text{rad.}}{\text{cosine OSR}}$. Therefore

$$\frac{M\pi}{GE} = \frac{\text{rad.}}{\text{sine lat.}} \times \frac{\text{cosine OSL}}{\text{rad.}} \times \frac{\text{sine lat.}}{\text{rad.}} \times \frac{\text{rad.}}{\text{cosine OSR.}} = \frac{\text{cosine OS}\lambda}{\text{cosine OSR}};$$

that is, cosine OSλ : cosine OSR :: Mπ : GE;
but rad. : sine MSπ :: SM : Mπ;
and cosine decl. : rad. :: GE : x = difference of right asc.

Hence cos. decl. × cos. OSλ : cosine OSR × sine MSπ :: SM : x, or the parallax of right ascension. But because in the same star $\frac{\text{cosine dec.} \times \text{cosine OS}\lambda}{\text{cosine OSR}}$

is a given quantity, it follows that if that given quantity be called z, then SM will be always to x (the parallax of right ascension) as z to the sine of the angle MSλ; so that if SM was a constant quantity, or the earth's velocity always the same, x would be always as the sine of the angle MSλ.

But the sun's equated place being always 90° before the point M, it will follow that x will be as the cosine of the sun's distance from the point λ before found; or if 90° be added to λ, then x will be always as the sun's distance from that point. Note; that the point λ thus found is that point of the ecliptic that has the same right ascension with the star.

While the point M is moving from λ through C to λ', the star's apparent right ascension is greater than its true, or the parallax must be added; that is, while the sun's distance from λ is more than 3^s and less than 9^s, the parallax of right ascension is to be added; but if it be more than 9^s and less than 3^s it is to be subtracted.

Hence the star's right ascension is greatest when it passes the meridian at midnight, and least when it passes at noon.

Hence the rule for finding the parallax in right ascension is this:

Let A be the angle of position as before.

Then sine lat. : rad. : : (cotang. A : cotang. C) : : tang. C : tang. A,
or rad. : sine lat. : : tang. A : tang. C.

The angle C subtracted from the star's longitude, if the star has north latitude; and in the ascending semicircle, or added if in the descending, will give the point λ; but the contrary if the star hath south latitude.

Take z : MS : : cosine A : cosine C.

Then cosine decl. : cosine distance from λ : : $z : x =$ parallax of right asc. to be added when the sun's distance from λ is between 3^s and 9^s, and subtracted when between 9^s and 3^s.

Let it be required to find the parallax in right ascension of η Ursæ M. March 21, 1728.

Here the angle $C = 33° 2'$, therefore the longitude of the point $\lambda = 6^s\ 26°\ 37'\ 30''$, and the sun's equated place being $0^s\ 11°\ 32'$, the sun from $\lambda = 5^s\ 14°\ 54'\frac{1}{2}$; hence the parallax of right ascension to be added is $28''\frac{1}{2}$, and the apparent right ascension of the star $204°\ 13'\ 28''\frac{1}{2}$.

Had the parallax been sought without any correction of the sun's place, or allowance for the inequality of the earth's motion, it would have been found $7'',3$, or about half a second different from the truth.

For common purposes we may without sensible error suppose the earth's motion equable and neglect the corrections, and then the rule for the parallax of right ascension will be this:

Sine lat. : rad. : : cotang. A : cotang. C, or rad : sine lat. : : tang. A : tang. C; then long. star $\mp$ C = long. of λ.

Cosine C : cosine A : : semi transverse axis : z.

And cosine decl. : cosine $(\odot - \lambda)$: : z : x = parallax of right ascension.

REDUCTION

OF THE

WANSTED OBSERVATIONS.

γ Draconis south of 38° 25′.

−0″,821 annual precession.

19″,3 maximum of aberration in declination.

3ˢ 2° 20′=12ˢ− right ascension of the star.

8ˢ 28° 6′=longitude of the sun's place when the aberration in declination is 0[a].

γ Drac.	Measured Dist.	Reduced Dist.	Precession.	Aberration.	Corrected Dist.	Nutation.	Mean Dist.	Argument of Nutation.	Moon's ascend. Node.	Argument of Aberration.	Sun's Longitude.
			−	+		−					
	″	″	″	″	″	″	″	S. ° ′	S. ° ′	S. ° ′	S. ° ′
1727, Aug. 19	72,6	71,8	0,3	18,0	89,5	9,0	80,5	2 24 38	11 22 17	8 8 39	5 6 45
20	71,3	70,5	0,3	18,1	88,3	9,0	79,3	24 34	22 14	9 37	7 43
21	71,5	70,7	0,3	18,2	88,6	9,0	79,6	24 31	22 11	10 35	8 41
22	70,0	69,2	0,3	18,3	87,2	9,0	78,2	24 28	22 8	11 33	9 39
23	71,4	70,6	0,3	18,3	88,6	9,0	79,6	24 25	22 5	12 31	10 37
25	71,6	70,8	0,3	18,6	89,1	9,0	80,1	24 18	21 58	14 27	12 33
:27	72,0	71,2	0,4	18,7	89,5	9,0	80,5	24 12	21 52	16 23	14 29
30	71,8	71,0	0,4	19,0	89,6	9,0	80,6	24 2	21 42	19 18	17 24
31	70,7	69,9	0,4	19,0	88,5	8,9	79,6	23 59	21 39	20 17	18 23
Sept. 2	71,7	70,9	0,4	19,1	89,6	8,9	80,7	23 53	21 33	22 13	20 19
3	71,3	70,5	0,4	19,1	89,2	8,9	80,3	23 50	21 30	23 12	21 18
4	71,7	70,9	0,4	19,2	89,7	8,9	80,8	23 47	21 27	24 10	22 16

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[a] These numbers are written in the original at the top of different columns without the particulars to which they refer. For the sake of perspicuity they have been collected together, and a short description has been added to each.

The heads of the columns are taken from the manuscript, with the exception of the four for the distances. The first of these is the quantity measured by the micrometer; thus γ Draconis was observed (see p. 203) Aug. 19, 1727. to be 2 rev. 4″,6 south of 38° 25′, which, reckoning each revolution of the screw to be 34,″ will make up 72″,6; but this is too much, and must be reduced (see p. 284) in the ratio of 34 to 33,62, which gives 71″,8; "∠ Corr." is placed over this quantity in the MS., but that title seems rather to belong to 89″,5, which results from the corrections being applied for precession and aberration; the further application for nutation gives the mean distance 80″,5, as in pp. 31, 32, 33, &c.

The first five columns are entirely in Bradley's handwriting, and the sun's longitude is all written by his nephew John Bradley; the rest is filled up partly by the one and partly by the other, but the larger portion by Bradley himself.

γ Drac.	Measured Dist.	Reduced Dist.	Precession.	Aberration.	Corrected Dist.	Nutation.	Mean Dist.	Argument of Nutation.	Moon's ascend. Node.	Argument of Aberration.	Sun's Longitude.
			−	+		−					
	"	"	"	"	"	"	"	s. ° ′	s. ° ′	s. ° ′	s. ° ′
1727, Sept. 7	70,9	70,1	0,4	19,3	89,0	8,9	80,1	2 23 37	11 21 17	8 27 6	5 25 12
: 9	70,0	69,2	0,4	19,3	88,1	8,9	79,2	23 31	21 11	29 3	27 9
: 14	70,5	69,7	0,4	19,3	88,6	8,9	79,7	23 15	20 55	9 3 57	6 2 3
: 15	70,2	69,4	0,4	19,2	88,2	8,9	79,3	23 12	20 52	4 56	3 2
19	71,3	70,5	0,4	19,1	89,2	8,9	80,3	22 59	20 39	8 51	6 57
23	71,5	70,7	0,4	18,8	89,1	8,9	80,2	22 46	20 26	12 48	10 54
24	71,6	70,8	0,4	18,8	89,2	8,9	80,3	22 43	20 23	13 46	11 52
: : 25	71,6	70,8	0,4	18,7	89,1	8,9	80,2	22 40	20 20	14 45	12 51
27	71,8	71,0	0,4	18,5	89,1	8,9	80,2	22 34	20 14	16 43	14 49
: 29	71,8	71,0	0,4	18,3	88,9	8,9	80,0	22 28	20 8	18 42	16 48
Oct. 8	72,5	71,7	0,5	17,1	88,3	8,9	79,4	21 59	19 39	27 38	25 44
: 13	74,0	73,2	0,5	16,3	89,0	8,9	80,1	21 43	19 23	10 2 36	7 0 42
20	75,2	74,4	0,5	14,9	88,8	8,9	79,9	21 21	19 1	9 36	7 42
: : 21	76,4	75,6	0,5	14,7	89,8	8,9	80,9	21 18	18 58	10 36	8 42
Nov. 14	81,5	80,6	0,5	8,2	88,3	8,8	79,5	20 5	17 45	11 4 44	8 2 50
17	82,5	81,6	0,5	7,3	88,4	8,8	79,6	19 52	17 32	7 46	5 52
Dec. 5	88,5	87,5	0,6	1,3	88,2	8,8	79,4	18 54	16 34	26 0	24 6
: 6	88,3	87,3	0,6	+1,0	87,7	8,8	78,9	18 51	16 31	27 1	25 7
: 15	91,8	90,8	0,6	−2,1	88,1	8,8	79,3	18 23	16 3	0 6 12	9 5 18
: 27	97,0	95,9	0,6	6,1	89,2	8,8	80,4	17 44	15 24	18 23	17 29
28	96,2	95,2	0,6	6,4	88,2	8,8	79,4	17 41	15 21	19 24	18 30
29	96,7	95,7	0,6	6,7	88,4	8,8	79,6	17 38	15 18	20 25	19 31
1728, Jan. 16	103,5	102,4	0,7	12,1	89,6	8,8	80,8	16 42	14 22	1 8 41	10 7 47
: : 23	104,5	103,4	0,7	13,8	88,9	8,7	80,2	16 20	14 0	15 45	14 51
24	104,9	103,8	0,7	14,1	89,0	8,7	80,3	16 17	13 57	16 45	15 51
25	104,0	102,9	0,7	14,3	87,9	8,7	79,2	16 13	13 53	17 46	16 52
Feb. 10	109,0	107,8	0,8	17,3	89,7	8,7	81,0	15 23	13 3	2 3 53	11 2 59
16	110,5	109,3	0,8	18,2	90,3	8,7	81,6	15 3	12 43	9 54	9 0
March 7	110,5	109,3	0,8	19,3	89,2	8,6	80,6	14 0	11 40	29 46	28 52
manè. 18	109,9	108,7	0,8	19,0	88,9	8,6	80,3	13 28	11 8	3 10 37	0 9 43
20	109,5	108,3	0,8	18,8	88,7	8,6	80,1	13 22	11 2	12 35	11 41
m. 22	109,7	108,5	0,8	18,7	89,0	8,6	80,4	13 15	10 55	14 33	13 39
24	108,2	107,0	0,8	18,5	87,7	8,6	79,1	13 9	10 49	16 30	15 36
Apr. n. 6	107,0	105,8	0,9	16,7	88,2	8,6	79,6	12 25	10 5	4 0 11	28 17
: : 7	107,0	105,8	0,9	16,5	88,4	8,6	79,8	12 22	10 2	1 10	29 16
May : : 5	100,5	99,4	0,9	10,2	88,3	8,5	79,8	10 52	8 32	28 10	1 26 16
6	99,5	98,4	0,9	9,9	87,6	8,5	79,1	10 49	8 29	29 7	27 13
: : 7	98,3	97,2	0,9	9,6	86,7	8,5	78,2	10 46	8 26	5 0 6	28 11
: : 10	99,0	97,9	0,9	8,7	88,3	8,5	79,8	10 37	8 17	2 57	2 1 3
June : 4	89,8	88,8	1,0	1,1	86,7	8,4	78,3	9 17	6 57	26 48	24 54
5	89,6	88,6	1,0	−0,8	86,8	8,4	78,4	9 14	6 54	27 45	25 51
15	88,5	87,5	1,0	+2,4	88,9	8,4	80,5	8 44	6 22	6 7 15	3 5 21
: 21	84,5	83,6	1,0	4,3	86,9	8,4	78,2	8 26	6 6	12 57	11 3
: 22	84,6	83,7	1,0	4,6	87,3	8,4	78,9	8 23	6 3	13 55	12 1
25	83,0	82,1	1,0	5,6	86,7	8,4	78,3	8 11	5 51	16 45	14 51
30	83,0	82,1	1,0	7,1	88,2	8,3	79,9	7 55	5 35	21 31	19 37
July : : 2	83,0	82,1	1,0	7,7	88,8	8,3	80,5	7 49	5 29	23 25	21 31
3	82,5	81,6	1,1	8,0	88,5	8,3	80,2	7 46	5 26	24 22	22 28

γ Drac.	Measured Dist.	Reduced Dist.	Precession.	Aberration.	Corrected Dist.	Nutation.	Mean Dist.	Argument of Nutation.	Moon's ascend. Node.	Argument of Aberration.	Sun's Longitude.
			—	+		—					
	″	″	″	″	″	″	″	S. ° ′	S. ° ′	S. ° ′	S. ° ′
1728, July : : 4	82,5	81,6	1,1	8,2	88,7	8,3	80,4	2 7 43	11 5 23	6 25 19	3 23 25
: : 5	79,7	78,8	1,1	8,5	86,2	8,3	77,9	7 39	5 19	26 16	24 22
13	77,8	77,0	1,1	10,7	86,6	8,3	78,3	7 13	4 53	7 3 53	4 1 59
18	77,5	76,7	1,1	12,1	87,7	8,3	79,4	6 57	4 37	8 40	6 46
26	77,0	76,2	1,1	14,0	89,1	8,3	80,8	6 32	4 12	16 17	14 23
28	75,0	74,2	1,1	14,4	87,5	8,2	79,3	6 26	4 6	18 13	16 19
Aug. : : 1	75,0	74,2	1,1	15,2	88,3	8,2	80,1	6 13	3 53	22 3	20 9
2	75,0	74,2	1,1	15,4	88,5	8,2	80,3	6 10	3 50	23 0	21 6
5	74,0	73,2	1,1	16,0	88,1	8,2	79,9	6 0	3 40	25 53	23 59
: : 10	76,0	75,2	1,2	16,8	90,8	8,2	82,6	5 44	3 24	8 0 42	28 48
12	74,0	73,2	1,2	17,1	89,1	8,2	80,9	5 38	3 18	2 37	5 0 43
14	73,0	72,2	1,2	17,4	88,4	8,2	80,2	5 32	3 12	4 33	2 39
15	72,0	71,2	1,2	17,6	87,6	8,2	79,4	5 28	3 8	5 31	3 37
17	72,0	71,2	1,2	17,8	87,8	8,2	79,6	5 22	3 2	7 26	5 32
18	72,3	71,5	1,2	17,9	88,2	8,2	80,0	5 20	3 0	8 24	6 30
22	72,3	71,5	1,2	18,4	88,7	8,2	80,5	5 6	2 46	12 17	10 23
23	72,0	71,2	1,2	18,5	88,5	8,2	80,3	5 3	2 43	13 14	11 20
25	71,2	70,4	1,2	18,7	87,9	8,1	79,8	4 57	2 37	15 11	13 17
29	71,0	70,2	1,2	18,9	88,0	8,1	79,9	4 44	2 24	19 4	17 10
Sept. 1	71,0	70,2	1,2	19,1	88,1	8,1	80,0	4 34	2 14	21 59	20 5
3	71,0	70,2	1,2	19,2	88,2	8,1	80,1	4 28	2 8	23 56	22 2
4	71,0	70,2	1,2	19,3	88,3	8,1	80,2	4 25	2 5	24 55	23 1
6	71,0	70,2	1,2	19,3	88,3	8,1	80,2	4 20	2 0	26 52	24 58
9	70,5	69,7	1,2	19,3	87,8	8,1	79,7	4 9	1 49	29 47	27 53
10	71,0	70,2	1,2	19,3	88,3	8,1	80,2	4 6	1 46	9 0 46	28 52
11	70,5	69,7	1,2	19,3	87,8	8,1	79,7	4 3	1 43	1 45	29 51
15	71,3	70,5	1,2	19,2	88,5	8,1	80,4	3 50	1 30	5 40	6 3 46
: : 16	70,7	69,9	1,2	19,2	87,9	8,1	79,8	3 47	1 27	6 39	4 45
27	71,5	70,7	1,2	18,4	87,9	8,0	79,9	3 12	0 52	17 29	15 35
28	72,0	71,2	1,2	18,3	88,3	8,0	80,3	3 9	0 49	18 28	16 34
Oct. 16	75,3	74,5	1,3	15,6	88,8	8,0	80,8	2 11	10 29 51	10 6 21	7 4 27
18	75,0	74,2	1,3	15,2	88,1	7,9	80,2	2 5	29 45	8 21	6 27
22	75,0	74,2	1,3	14,3	87,2	7,9	79,3	1 52	29 32	12 21	10 27
Nov. 19	83,0	82,1	1,4	6,4	87,1	7,8	79,3	0 23	28 3	11 10 33	8 8 39
23	83,5	82,6	1,4	5,1	86,3	7,8	78,5	0 11	27 51	14 37	12 43
28	86,5	85,7	1,4	3,4	87,7	7,8	79,9	1 29 55	27 35	19 41	17 47
29	86,3	85,4	1,4	+3,1	87,1	7,8	79,3	29 52	27 32	20 41	18 47
Dec. 17	92,2	91,2	1,4	−3,0	86,8	7,7	79,1	28 58	26 38	0 9 0	9 8 6
19	92,5	91,5	1,4	3,7	86,4	7,7	78,7	28 52	26 32	11 2	10 8
20	93,3	92,3	1,5	4,0	86,8	7,7	79,1	28 49	26 29	12 3	11 9
Dr. H. 21	93,0	92,0	1,5	4,3	86,2	7,7	78,5	28 46	26 26	13 4	12 10
1729, Jan. : : 25	104,0	102,9	1,5	14,5	86,9	7,5	79,4	26 51	24 31	1 18 33	10 17 39
28	105,0	103,9	1,5	15,1	87,3	7,5	79,8	26 42	24 22	21 34	20 40
Feb. 5	107,3	106,1	1,6	16,7	87,8	7,5	80,3	26 16	23 56	29 37	28 43
6	107,5	106,3	1,5	16,8	88,0	7,5	80,5	26 13	23 53	2 0 37	29 43
7	107,1	105,9	1,6	16,9	87,4	7,5	79,9	26 10	23 50	1 38	11 0 44
: : 13	108,5	107,3	1,6	17,8	87,9	7,5	80,4	25 50	23 30	7 39	6 45
: 14	109,0	107,8	1,6	18,0	88,2	7,4	80,8	25 47	23 27	8 39	7 45

γ Drac.	Measured Dist.	Reduced Dist.	Precession.	Aberration.	Corrected Dist.	Nutation.	Mean Dist.	Argument of Nutation.	Moon's ascend. Node.	Argument of Aberration.	Sun's Longitude.
	″	″	— ″	— ″	″	— ″	″	S. ° ′	S. ° ′	S. ° ′	S. ° ′
1729, Feb. 21	110,0	108,8	1,6	18,7	88,5	7,4	81,1	1 25 25	10 23 5	2 15 38	11 14 44
: 25	108,5	107,3	1,6	18,9	86,8	7,4	79,4	25 12	22 52	19 37	18 43
26	108,5	107,3	1,6	19,0	86,7	7,4	79,3	25 9	22 49	20 37	19 43
27	108,5	107,3	1,6	19,1	86,6	7,4	79,2	25 6	22 46	21 36	20 42
March 1	110,0	108,8	1,6	19,2	88,0	7,4	80,6	25 0	22 40	23 35	22 41
5	108,8	107,6	1,6	19,3	86,7	7,4	79,3	24 47	22 27	27 33	26 39
6	109,5	108,3	1,6	19,3	87,4	7,4	80,0	24 44	22 24	28 32	27 38
17	108,7	107,5	1,6	19,1	86,8	7,3	79,5	24 9	21 49	3 9 24	0 8 30
: 18	110,0	108,8	1,6	19,0	88,2	7,3	80,9	24 6	21 46	10 22	9 28
manè. 20	108,0	106,8	1,7	18,8	86,3	7,3	79,0	24 0	21 40	12 21	11 27
April 16	105,5	104,4	1,7	14,9	87,8	7,2	80,6	22 33	20 13	4 9 34	1 7 40
May : 29	92,3	91,3	1,8	3,0	86,5	6,9	79,6	20 17	17 57	5 20 51	2 18 57
: 31	91,5	90,5	1,8	2,4	86,3	6,9	79,4	20 10	17 50	22 46	20 52
June 2	91,0	90,0	1,8	1,8	86,4	6,9	79,5	20 4	17 44	24 40	22 46
5	89,5	88,5	1,8	0,8	85,9	6,9	79,0	19 54	17 34	27 31	25 37
: 6	90,2	89,2	1,8	0,5	86,9	6,9	80,0	19 51	17 31	28 29	26 35
7	89,0	88,0	1,8	−0,2	86,0	6,9	79,1	19 48	17 28	29 25	27 31
9	90,0	89,0	1,8	+0,5	87,7	6,9	80,8	19 42	17 22	6 1 19	29 25
20	84,0	83,1	1,8	4,0	85,3	6,8	78,5	19 7	16 47	11 46	3 9 52
24	84,5	83,6	1,8	5,2	87,0	6,8	80,2	18 54	16 34	15 34	13 40
July : 1	81,5	80,6	1,9	7,3	86,0	6,7	79,3	18 32	16 12	22 14	20 20
10	79,0	78,1	1,9	9,9	86,1	6,7	79,4	18 3	15 43	7 0 48	28 54
27	74,8	74,0	1,9	14,1	86,2	6,6	79,6	17 9	14 49	17 1	4 15 7
28	75,0	74,2	1,9	14,3	86,6	6,6	80,0	17 6	14 46	17 59	16 5
29	75,5	73,7	1,9	14,6	86,4	6,6	79,8	17 3	14 43	18 56	17 2
Aug. q. 5	75,5	74,7	2,0	16,0	88,7	6,6	82,1	16 41	14 21	25 39	23 45
6	71,5	70,7	2,0	16,1	84,8	6,5	78,3	16 38	14 18	26 37	24 43
10	71,5	70,7	2,0	16,8	85,5	6,5	79,0	16 25	14 5	8 0 27	28 33
: 12	72,0	71,2	2,0	17,1	86,3	6,5	79,8	16 19	13 59	2 23	5 0 29
13	72,2	71,4	2,0	17,2	86,6	6,5	80,1	16 16	13 56	3 21	1 27
17	72,0	71,2	2,0	17,8	87,0	6,5	80,5	16 3	13 43	7 12	5 18
19	70,7	69,9	2,0	18,1	86,0	6,5	79,5	15 57	13 37	9 8	7 14
Dr. Hoadley 23	69,0	68,2	2,0	18,4	84,6	6,5	78,1	15 43	13 23	13 1	11 7
25	70,5	69,7	2,0	18,7	86,4	6,4	80,0	15 37	13 17	14 57	13 3
28	70,0	69,2	2,0	18,8	86,0	6,4	79,6	15 28	13 8	17 52	15 58
29	70,2	69,4	2,0	18,9	86,3	6,4	79,9	15 25	13 5	18 50	16 56
31	70,5	69,7	2,0	19,0	86,7	6,4	80,3	15 20	13 0	20 46	18 52
Sept. 2	70,5	69,7	2,0	19,1	86,8	6,4	80,4	15 12	12 52	22 43	20 49
8	70,2	69,4	2,1	19,3	86,6	6,4	80,2	14 53	12 33	28 35	26 41
9	70,2	69,4	2,0	19,3	86,7	6,3	80,4	14 50	12 30	29 33	27 39
13	69,6	68,8	2,1	19,3	86,0	6,3	79,7	14 37	12 17	9 3 28	6 1 34
18	69,8	69,0	2,1	19,1	86,0	6,3	79,7	14 21	12 1	8 23	6 29
26	70,5	69,7	2,1	18,5	86,1	6,3	79,8	13 55	11 35	16 16	14 22
Oct. 4	71,5	70,7	2,1	17,6	86,2	6,2	80,0	13 30	11 10	24 11	22 17
7	71,3	70,5	2,1	17,2	85,6	6,2	79,4	13 20	11 0	27 10	25 16
18	73,0	72,2	2,1	15,2	85,3	6,1	79,2	12 46	10 26	10 8 6	7 6 12
24	75,0	74,2	2,1	13,8	85,9	6,1	79,8	12 26	10 6	14 6	12 12
Nov. 21	82,5	81,6	2,2	5,8	85,2	5,9	79,3	10 57	8 37	11 12 20	8 10 26

γ Drac.	Measured Dist.	Reduced Dist.	Precession.	Aberration.	Corrected Dist.	Nutation.	Mean Dist.	Argument of Nutation.	Moon's ascend. Node.	Argument of Aberration.	Sun's Longitude.
			—	+		—					
	″	″	″	″	″	″	″	S. ° ′	S. ° ′	S. ° ′	S. ° ′
1729, Nov. : : 26	84,0	83,1	2,2	4,2	85,1	5,9	79,2	1 10 42	10 8 22	11 17 24	8 15 30
28	85,0	84,1	2,2	3,5	85,4	5,9	79,5	10 36	8 16	19 26	17 32
30	85,3	84,4	2,2	2,9	85,1	5,8	79,3	10 29	8 9	21 27	19 33
Dec. 3	86,0	85,1	2,2	1,9	84,8	5,8	79,0	10 19	7 59	24 30	22 36
6	87,5	86,6	2,3	+0,8	85,1	5,8	79,3	10 10	7 50	27 33	25 39
10	88,5	87,5	2,3	−0,5	84,7	5,8	78,9	9 57	7 37	0 1 38	9 0 44
15	90,5	89,5	2,3	2,2	85,0	5,8	79,2	9 41	7 21	6 43	5 49
1730, March 10	107,3	106,1	2,5	19,3	84,3	5,3	79,0	5 11	2 51	3 2 15	0 1 21
: 16	108,0	106,8	2,5	19,1	85,2	5,2	80,0	4 52	2 32	8 10	7 16
18	107,2	106,0	2,5	19,0	84,5	5,1	79,4	4 46	2 26	10 8	9 14
June 6	87,7	86,7	2,7	0,6	83,4	4,6	78,8	0 32	9 28 12	5 28 14	2 26 20
7	87,5	86,6	2,7	−0,3	83,6	4,6	79,0	0 28	28 8	29 11	27 17
Aug. 17	70,0	69,2	2,8	+17,7	84,1	4,0	80,1	0 26 43	24 23	8 7 0	5 5 5
18	70,3	69,5	2,8	17,9	84,6	4,0	80,6	26 40	24 20	7 56	6 2
23	69,0	68,2	2,8	18,4	83,8	4,0	79,8	26 24	24 4	12 47	10 53
24	69,2	68,4	2,8	18,5	84,1	4,0	80,1	26 21	24 1	13 45	11 51
25	69,3	68,5	2,8	18,6	84,5	4,0	80,5	26 18	23 58	14 43	12 49
26	69,0	68,2	2,8	18,7	84,1	4,0	80,1	26 15	23 55	15 41	13 47
31	69,0	68,2	2,8	19,0	84,4	4,0	80,4	25 58	23 38	20 32	18 38
Sept. 1	69,0	68,2	2,9	19,1	84,4	3,9	80,5	25 55	23 35	21 31	19 37
7	69,0	68,2	2,9	19,3	84,6	3,9	80,7	25 36	23 16	27 22	25 28
8	69,0	68,2	2,9	19,3	84,6	3,9	80,7	25 33	23 13	28 20	26 26
9	68,0	67,3	2,9	19,3	83,7	3,9	79,8	25 30	23 10	29 19	27 25
16	68,7	68,0	2,9	19,2	84,3	3,8	80,5	25 8	22 48	9 6 10	6 4 16
18	67,8	67,1	2,9	19,1	83,3	3,8	79,5	25 2	22 42	8 8	6 14
20	68,7	68,0	2,9	19,0	84,1	3,8	80,3	24 55	22 35	10 6	8 12
22	67,9	67,2	2,9	+18,9	83,2	3,8	79,4	24 49	22 29	12 4	10 10
Dec. 9	86,6	85,7	3,1	− 0,1	82,5	3,7	78,8	23 54	21 34	0 0 18	8 29 24
12	87,8	86,9	3,1	1,2	82,6	3,6	79,0	23 45	21 25	3 25	9 2 31
15	88,5	87,5	3,1	2,2	82,2	3,6	78,6	23 35	21 15	6 28	5 34
24	91,5	90,5	3,1	5,2	82,2	3,5	78,7	23 7	20 47	15 36	14 42
1731, Jan. : : : 1	95,0	94,0	3,1	7,8	83,1	3,0	80,1	19 28	17 8	23 45	22 51
: : 4	95,0	94,0	3,2	8,7	82,1	3,0	79,1	19 15	16 55	26 48	25 54
Feb. : : 3	105,0	103,9	3,2	16,2	84,5	2,7	81,8	17 43	15 23	1 27 5	10 26 11
6	105,0	103,9	3,2	16,7	84,0	2,7	81,3	17 33	15 13	2 0 6	29 12
7	105,7	104,5	3,2	−16,9	84,4	2,7	81,7	17 30	15 10	1 6	11 0 12
Aug. 14	69,3	68,5	3,7	+17,3	82,1	1,2	80,9	7 33	5 13	8 3 51	5 1 57
17	68,5	67,8	3,6	17,7	81,9	1,2	80,7	7 23	5 3	6 45	4 51
18	68,5	67,7	3,7	17,9	81,9	1,2	80,7	7 20	5 0	7 43	5 49
20	67,0	66,3	3,7	18,1	80,7	1,1	79,6	7 14	4 54	9 39	7 45
21	67,7	67,0	3,7	18,2	81,5	1,1	80,4	7 10	4 50	10 37	8 43
22	67,8	67,1	3,7	18,3	81,7	1,1	80,6	7 7	4 47	11 35	9 41
23	67,8	67,1	3,7	18,4	81,8	1,1	80,7	7 4	4 44	12 33	10 39
Sept. 3	67,2	66,5	3,7	19,2	82,0	1,0	81,0	6 29	4 9	23 14	21 20
7	67,0	66,3	3,7	19,3	81,9	1,0	80,9	6 16	3 56	27 8	25 14
10	66,5	65,8	3,7	19,3	81,4	1,0	80,4	6 7	3 47	9 0 4	28 10
12	67,0	66,3	3,7	19,3	81,9	0,9	81,0	6 1	3 41	2 1	6 0 7
13	67,0	66,2	3,7	19,3	81,8	0,9	80,9	5 58	3 38	3 0	1 6

γ Drac.	Measured Dist.	Reduced Dist.	Precession.	Aberration.	Corrected Dist.	Nutation.	Mean Dist.	Argument of Nutation.	Moon's ascend. Node.	Argument of Aberration.	Sun's Longitude.
			−	+		−					
	"	"	"	"	"	"	"	S. ° ′	S. ° ′	S. ° ′	S. ° ′
1731, Sept. 15	66,2	65,5	3,8	19,2	80,9	0,9	80,0	0 5 51	9 3 31	9 4 57	6 3 3
: 19	67,8	67,1	3,8	19,1	82,4	0,9	81,5	5 48	3 18	8 53	6 59
20	66,8	66,1	3,8	19,0	81,3	0,9	80,4	5 35	3 15	9 51	7 57
22	67,3	66,6	3,8	18,9	81,7	0,9	80,8	5 29	3 9	11 49	9 55
: : 25	67,5	66,7	3,8	18,7	81,6	0,8	80,8	5 19	2 59	14 47	12 53
Oct. 2	68,7	67,9	3,8	18,0	82,1	0,8	81,3	4 57	2 37	21 42	19 48
: : 4	69,0	68,2	3,8	+17,7	82,1	0,8	81,3	4 51	2 31	23 42	21 48
Dec. 18	88,0	87,0	3,9	− 3,1	80,0	−0,1	79,9	0 52	8 28 32	0 9 16	9 8 22
31	92,0	91,0	4,0	7,4	79,6	0,0	79,6	0 11	27 51	22 29	21 35
1732, Jan. 9	95,5	94,5	4,0	−10,1	80,4	0,0	80.4	11 29 43	27 23	1 1 36	10 0 42
Sept. 2	65,7	65,0	4,5	+19,1	79,6	+2,0	81,6	17 13	14 53	8 23 0	5 21 6
3	64,7	64,0	4,6	19,2	78,6	2,0	80,6	17 10	14 50	23 58	22 4
: 4	66,0	65,3	4,5	19,2	80,0	2,0	82,0	17 7	14 47	24 56	23 2
: 5	64,5	63,8	4,6	19,3	78,5	2,0	80,5	17 4	14 44	25 55	24 1
6	65,0	64,3	4,6	+19,3	79,0	2,0	81,0	17 0	14 40	26 53	24 59
1733, Jan. 21	96,0	95,0	4,9	−13,5	76,6	3,1	79,7	9 41	7 21	1 14 32	10 13 38
22	96,7	95,6	4,9	−13,8	76,9	3,1	80,0	9 38	7 18	15 33	14 39
Aug. 25	61,0	60,3	5,3	+18,6	73,6	4,7	78,3	10 28 15	7 25 55	8 15 0	5 13 5
26	62,0	61,3	5,4	18,7	74,6	4,7	79,3	28 12	25 52	15 57	14 3
29	61,5	60,8	5,3	19,0	74,5	4,8	79,3	28 2	25 42	18 52	16 58
1734, June 19	75,0	74,2	6,0	3,6	71,8	6,6	78,4	12 28	10 8	6 10 38	3 8 44
: 21	74,0	73,2	6,0	4,2	71,4	6,6	78,0	12 22	10 2	12 32	10 38
July 5	72,5	71,7	6,0	8,4	74,1	6,7	80,8	11 37	9 17	25 51	23 57
10	69,3	68,5	6,1	9,8	72,2	6,7	78,9	11 22	9 2	7 0 36	28 42
16	68,0	67,3	6,1	11,4	72,6	6,8	79,4	11 3	8 43	6 19	4 4 25
29	64,5	63,8	6,1	14,5	72,2	6,9	79,1	10 21	8 1	18 45	16 11
: : 31	65,0	64,3	6,1	14,9	73,1	6,9	80,0	10 15	7 55	20 40	18 46
Aug. 3	65,2	64,5	6,1	15,5	73,9	6,9	80,8	10 5	7 45	23 33	21 39
: : : 6	63,5	62,8	6,1	16,1	72,8	6,9	79,7	9 56	7 36	26 25	24 31
7	62,5	61,8	6,1	16,3	72,0	6,9	78,9	9 53	7 33	27 23	25 29
8	62,7	62,0	6,2	16,4	72,2	6,9	79,1	9 50	7 30	28 21	26 27
11	63,0	62,3	6,2	+16,9	73,0	6,9	79,9	9 40	7 20	8 1 14	29 20
Dec. 11	79,5	78,6	6,4	− 0,8	71,4	7,5	78,9	3 12	0 52	0 2 26	9 1 32
22	83,3	82,4	6,4	4,5	71,5	7,6	79,1	2 37	0 17	13 37	12 43
23	84,3	83,4	6,4	4,9	72,1	7,6	79,7	2 34	0 14	14 38	13 44
27	86,0	85,1	6,5	− 6,2	72,4	7,6	80,0	2 21	0 1	18 42	17 48
1735, Sept. 9	61,1	60,4	7,0	+19,3	72,7	8,5	81,2	9 18 48	6 16 28	8 29 7	5 27 13
10	60,7	60,0	7,0	19,3	72,3	8,5	80,8	18 45	16 25	9 0 6	28 12
13	59,5	58,9	7,0	19,3	71,2	8,5	79,7	18 35	16 15	3 2	6 1 8
1736, Aug. 31	59,8	59,1	7,8	19,1	70,4	9,0	79,4	8 29 54	5 27 34	8 21 4	5 19 10
Sept. 8	59,5	58,9	7,9	19,3	70,3	9,0	79,3	29 28	27 8	28 52	26 58
9	60,0	59,3	7,9	+19,3	70,7	9,0	79,7	29 23	27 3	29 51	27 57
1737, Jan. 3	86,5	85,6	8,1	− 8,6	68,9	8,9	77,8	23 17	20 57	0 26 20	9 25 26
4	87,5	86,6	8,1	8,8	69,7	8,9	78,6	23 14	20 54	27 21	26 27
19	92,8	91,8	8,2	−13,0	70,6	8,9	79,5	22 26	20 6	1 12 33	10 11 39
Sept. 1	61,5	60,8	8,7	+19,1	71,2	8,5	79,7	10 31	8 11	8 21 49	5 19 55
2	60,3	59,6	8,7	19,1	70,0	8,5	78,5	10 28	8 8	22 47	20 53
6	61,5	60,8	8,7	19,3	71,4	8,5	79,9	10 15	7 55	26 41	24 47

γ Drac.	Measured Dist.	Reduced Dist.	Precession.	Aberration.	Corrected Dist.	Nutation.	Mean Dist.	Argument of Nutation.	Moon's ascend. Node.	Argument of Aberration.	Sun's Longitude.
			−	+		+					
	"	"	"	"	"	"	"	S. ° ′	S. ° ′	S. ° ′	S. ° ′
1737, Sept. 7	61,8	61,1	8,7	19,3	71,7	8,5	80,2	8 10 12	5 7 52	8 27 39	5 25 45
8	60,8	60,1	8,7	19,3	70,7	8,5	79,2	10 9	7 49	28 38	26 44
10	62,3	61,6	8,7	+19,3	72,2	8,4	80,6	10 2	7 42	9 0 35	28 41
Dec. 12	81,7	80,8	8,9	− 1,3	70,6	8,2	78,8	5 7	2 47	0 3 44	9 2 50
1738, July 25	68,0	67,3	9,4	+13,6	71,5	7,2	78,7	7 23 12	4 20 52	7 14 57	4 13 3
28	66,2	65,5	9,4	14,3	70,4	7,2	77,6	23 2	20 42	17 49	15 55
: 29	65,7	65,0	9,4	14,5	70,1	7,2	77,3	22 59	20 39	18 46	16 52
Sept. 9	62,3	61,6	9,5	19,3	71,4	7,0	78,4	20 46	18 26	8 29 23	5 27 29
12	61,7	61,0	9,5	19,3	70,8	7,0	77,8	20 36	18 16	9 2 18	6 0 24
13	61,8	61,1	9,5	19,3	70,9	7,0	77,9	20 33	18 13	3 17	1 23
14	63,7	63,0	9,5	19,3	72,8	7,0	79,8	20 30	18 10	4 15	2 21
15	63,3	62,6	9,5	19,2	72,3	6,9	79,2	20 27	18 7	5 14	3 20
16	63,2	62,5	9,5	+19,2	72,2	6,9	79,1	20 24	18 4	6 13	4 19
Dec. 30	91,5	90,5	9,8	− 6,8	73,9	6,3	80,2	14 50	12 30	0 20 47	9 19 53
1739, Jan. 14	96,0	95,0	9,8	11,6	73,6	6,3	79,9	14 2	11 42	1 7 0	10 6 6
15	96,2	95,2	9,8	11,9	73,5	6,2	79,7	13 59	11 39	8 0	7 6
24	98,5	97,4	9,8	−14,1	73,5	6,2	79,7	13 31	11 11	17 6	16 12
Aug. 18	68,0	67,3	10,3	+17,9	74,9	4,8	79,7	2 36	0 16	8 7 46	5 5 52
19	68,0	67,3	10,3	18,0	75,0	4,8	79,8	2 33	0 13	8 44	6 50
20	69,0	68,2	10,3	18,1	76,0	4,8	80,8	2 30	0 10	9 42	7 48
24	67,9	67,2	10,3	18,5	75,4	4,8	80,2	2 17	3 29 57	13 34	11 40
26	67,7	67,0	10,3	18,7	75,4	4,8	80,2	2 11	29 51	15 31	13 37
27	67,5	66,8	10,3	18,7	75,2	4,8	80,0	2 8	29 48	16 29	14 35
29	67,2	66,5	10,3	18,9	75,1	4,8	79,9	2 1	29 41	18 25	16 31
30	68,0	67,3	10,3	18,9	75,9	4,8	80,7	1 58	29 38	19 23	17 29
31	67,8	67,1	10,3	19,0	75,8	4,7	80,5	1 55	29 35	20 21	18 27
Sept. 2	67,3	66,6	10,3	+19,1	75,4	4,7	80,1	1 49	29 29	22 19	20 23
1740, Jan. 20	100,7	99,6	10,6	−13,1	75,9	3,7	79,6	6 24 24	22 4	1 12 46	10 11 52
26	104,3	103,2	10,7	−14,5	78,0	3,7	81,7	24 5	21 45	18 53	17 59
Aug. 9	73,5	72,7	11,1	+16,7	78,3	2,1	80,4	13 42	11 22	7 29 49	4 27 55
18	72,2	71,4	11,1	18,0	78,3	2,1	80,4	13 13	10 53	8 8 30	5 6 36
19	72,2	71,4	11,1	18,1	78,4	2,0	80,4	13 10	10 50	9 28	7 34
21	71,2	70,4	11,1	18,3	77,6	2,0	79,6	13 4	10 44	11 24	9 30
24	72,2	71,4	11,1	18,6	78,9	2,0	80,9	12 54	10 34	14 18	12 24
25	71,0	70,2	11,1	18,7	77,8	2,0	79,8	12 51	10 31	15 16	13 22
28	71,5	70,7	11,1	18,9	78,5	2,0	80,5	12 41	10 21	18 11	16 17
29	72,4	71,6	11,1	18,9	79,4	2,0	81,4	12 38	10 18	19 9	17 15
30	71,8	71,0	11,1	19,0	78,9	2,0	80,9	12 35	10 15	20 8	18 14
Sept. 5	71,6	70,8	11,1	19,2	78,9	1,9	80,8	12 16	9 56	25 58	24 4
8	72,2	71,4	11,1	19,3	79,6	1,9	81,5	12 6	9 46	28 54	27 0
20	72,5	71,7	11,2	+19,0	79,5	1,8	81,3	11 37	9 17	9 10 40	6 8 46
1741, Jan. 26	107,7	106,5	11,5	−14,7	80,3	+0,7	81,0	4 42	2 22	1 19 39	10 18 45
Aug. 27	76,3	75,5	11,9	+18,8	82,4	−1,0	81,4	5 23 25	2 21 5	8 17 0	5 15 5
28	76,4	75,6	11,8	18,8	82,6	1,0	81,6	23 22	21 2	17 57	16 3
29	76,2	75,4	11,9	18,9	82,4	1,0	81,4	23 19	20 59	18 55	17 1
31	75,7	74,9	11,9	19,0	82,0	1,1	80,9	23 13	20 53	20 56	18 58
Sept. 1	76,7	75,9	11,9	19,1	83,1	1,1	82,0	23 10	20 50	21 51	19 57

γ Drac.	Measured Dist.	Reduced Dist.	Precession.	Aberration.	Corrected Dist.	Nutation.	Mean Dist.	Argument of Nutation.	Moon's ascend. Node.	Argument of Aberration.	Sun's Longitude.
			—	+		—					
	"	"	"	"	"	"	"	S. ° ′	S. ° ′	S. ° ′	S. ° ′
1741, Sept. 2	76,2	75,4	11,9	19,1	82,6	1,1	81,5	5 23 6	2 20 46	8 22 49	5 20 55
6	75,5	74,7	11,9	19,3	82,1	1,1	81,0	22 53	20 33	26 43	24 49
20	76,4	75,6	12,0	19,0	82,6	1,2	81,4	22 9	19 49	9 10 26	6 8 32
21	76,7	75,9	12,0	18,9	82,8	1,2	81,6	22 6	19 46	11 25	9 31
23	76,6	75,8	12,0	18,7	82,5	1,2	81,3	21 59	19 39	13 23	11 29
1742, Sept. 5	77,5	76,7	12,8	19,2	83,1	4,0	79,1	3 37	1 17	8 25 30	5 23 36
1743, Sept. 2	82,5	81,6	13,6	19,1	87,1	6,4	80,7	4 14 26	1 12 6	22 21	20 27
1745, Sept. 2	87,8	86,8	15,2	19,1	90,7	8,9	81,8	3 5 44	0 3 24	22 51	20 57
3	87,8	86,9	15,2	19,2	90,9	9,0	81,9	5 41	3 21	23 50	21 56
5	87,3	86,4	15,2	19,3	90,5	9,0	81,5	5 35	3 15	25 46	23 52
1746, Sept. 15	87,8	86,9	16,0	19,2	90,1	8,7	81,4	2 15 43	11 13 23	9 5 19	6 3 25
20	87,0	86,1	16,1	19,0	89,0	8,7	80,3	15 27	13 7	10 14	8 20
1747, Aug. 31	87,0	86,1	16,8	19,0	88,3	7,6	80,7	1 27 10	10 24 50	8 20 24	5 18 30
Sept. 1	87,2	86,2	16,8	19,1	88,5	7,6	80,9	27 7	24 47	21 24	19 30
2	87,0	86,1	16,8	19,1	88,4	7,5	80,9	27 4	24 44	22 22	20 28

β Draconis north of 37° 30′.

+3″,07 annual precession.

3^s 8° 48′ = 12^s − right ascension of the star.

8^s 22° 48′ = longitude of the sun's place when the aberration in declination is 0.

β Drac.	Measured Dist.	Reduced Dist.	Precession.	Aberration.	Corrected Dist.	Nutation.	Mean Dist.	Argument of Nutation.	Moon's ascend. Node.	Argument of Aberration.	Sun's Longitude.
			+	—		+					
	"	"	"	"	"	"	"	S. ° ′	S. ° ′	S. ° ′	S. ° ′
1727, Aug. 20	165,5	163,7	1,2	18,7	146,2	9,0	155,2	3 1 4	11 22 16	8 14 56	5 7 42
21	166,9	165,1	1,2	18,8	147,5	9,0	156,5	1 0	22 12	15 51	8 39
22	165,8	164,0	1,3	18,9	146,4	9,0	155,4	0 57	22 9	16 50	9 38
23	167,0	165,2	1,3	19,0	147,5	9,0	156,5	0 54	22 6	17 48	10 36
25	167,8	166,0	1,3	19,1	148,2	9,0	157,2	0 48	22 0	19 44	12 32
: 30	167,5	165,7	1,3	19,3	147,7	9,0	156,7	0 32	21 44	24 35	17 23
Sept. 2	166,2	164,4	1,3	19,3	146,4	9,0	155,4	0 22	21 34	27 30	20 18
3	166,8	165,0	1,4	19,4	147,0	9,0	156,0	0 19	21 31	28 29	21 17
4	165,8	164,0	1,4	19,4	146,0	9,0	155,0	0 16	21 28	29 27	22 15
7	166,0	164,2	1,4	19,4	146,2	9,0	155,2	0 6	21 18	9 2 23	25 11
15	166,8	165,0	1,5	19,1	147,4	9,0	156,4	2 29 41	20 53	10 13	6 3 1
23	165,2	163,4	1,5	18,4	146,5	9,0	155,5	29 15	20 27	18 5	10 53
24	164,7	162,9	1,5	18,4	146,4	9,0	155,4	29 12	20 24	19 4	11 52
Oct. 17	162,5	160,7	1,7	14,5	147,9	9,0	156,9	27 59	19 11	10 11 53	7 4 41
Nov. 14	154,6	152,9	2,0	6,6	148,3	9,0	157,3	26 22	17 42	11 10 0	8 2 48
: 17	153,8	152,1	2,0	— 5,7	148,4	9,0	157,4	26 21	17 33	13 3	5 51

β Drac.	Measured Dist.	Reduced Dist.	Precession.	Aberration.	Corrected Dist.	Nutation.	Mean Dist.	Argument of Nutation.	Moon's ascend. Node.	Argument of Aberration.	Sun's Longitude.
	″	″	+ ″	+ ″	″	+ ″	″	S. ° ′	S. ° ′	S. ° ′	S. ° ′
1727, Dec. 5	147,2	145,6	2,1	0,4	148,1	9,0	157,1	2 25 24	11 16 36	0 1 19	8 24 7
: 6	146,5	144,9	2,1	0,8	147,8	9,0	156,8	25 21	16 33	2 19	25 7
13	143,2	141,6	2,2	3,2	147,0	9,0	156,0	24 58	16 10	9 27	9 3 15
28	138,7	137,2	2,3	8,1	147,6	8,9	156,5	24 10	15 22	24 41	18 29
29	138,2	136,7	2,3	8,4	147,4	8,9	156,3	24 7	15 19	25 42	19 30
1728, Jan. 16	131,8	130,4	2,5	13,5	146,4	8,9	155,3	23 10	14 22	1 13 58	10 7 46
: 23	131,0	129,6	2,5	15,1	147,2	8,9	156,1	22 48	14 0	21 3	14 51
25	131,0	129,6	2,6	15,5	147,7	8,9	156,6	22 41	13 53	23 4	16 52
Feb. 10	127,5	126,1	2,7	18,1	146,9	8,9	155,8	21 51	13 3	2 9 10	11 2 58
16	125,0	123,6	2,8	18,8	145,2	8,9	154,1	21 32	12 44	15 12	9 0
March 7	126,0	124,6	2,9	19,3	146,8	8,9	155,7	20 28	11 40	3 5 4	28 52
m. 18	126,7	125,3	3,0	18,7	147,0	8,9	155,9	19 53	11 5	15 54	0 9 42
20	127,4	126,0	3,0	18,5	147,5	8,9	156,4	19 47	10 59	17 52	11 40
22	128,0	126,6	3,0	18,3	147,9	8,9	156,8	19 41	10 53	19 50	13 38
m. 24	128,5	127,1	3,1	18,0	148,2	8,9	157,1	19 35	10 47	21 47	15 35
Apr. n. 6	131,3	129,9	3,2	15,8	148,9	8,8	157,7	18 53	10 5	4 5 28	28 16
: : 7	129,7	128,3	3,2	15,6	147,1	8,8	155,9	18 50	10 2	6 27	29 15
May : 5	137,7	136,2	3,4	8,7	148,3	8,8	157,1	17 20	8 32	5 3 27	1 26 15
6	138,3	136,8	3,4	8,4	148,6	8,8	157,4	17 17	8 29	4 25	27 13
										5 23	28 11
: 10	136,5	135,0	3,5	+ 7,2	145,7	8,8	154,5	17 5	8 17	8 15	2 1 3
June 4	147,7	146,1	3,7	− 0,7	149,1	8,7	157,8	15 45	6 57	6 2 5	24 53
: 21	153,0	151,3	3,8	6,1	149,0	8,7	157,7	14 51	6 3	18 14	3 11 2
22	154,8	153,1	3,8	6,4	150,5	8,7	159,2	14 48	6 0	19 12	12 0
Ld.C.Cavendish 25	154,0	152,3	3,8	7,3	148,8	8,7	157,5	14 38	5 50	22 2	14 50
July 2	155,0	153,3	3,9	9,3	147,9	8,7	156,6	14 26	5 38	28 42	21 30
3	157,3	155,6	3,9	9,6	149,9	8,7	158,6	14 23	5 35	29 40	22 28
: 4	156,0	154,3	3,9	9,9	148,3	8,7	157,0	14 20	5 32	7 0 36	23 24
5	156,5	154,8	3,9	10,2	148,5	8,7	157,2	14 27	5 29	1 33	24 21
13	158,0	156,3	4,0	12,2	148,1	8,6	156,7	13 41	4 53	9 10	4 1 58
18	159,5	157,8	4,0	13,5	148,3	8,6	156,9	13 25	4 37	13 57	6 45
: 26	161,0	159,2	4,1	15,2	148,1	8,6	156,7	13 0	4 12	21 35	14 23
28	160,5	158,8	4,1	15,6	147,3	8,6	155,9	12 54	4 6	23 30	16 18
Aug. 1	161,7	159,9	4,1	16,3	147,7	8,6	156,3	12 41	3 53	27 20	20 8
2	161,0	159,3	4,2	16,5	147,0	8,6	155,6	12 38	3 50	28 18	21 6
5	163,0	161,2	4,2	17,0	148,4	8,6	157,0	12 28	3 40	8 1 11	23 59
10	162,0	160,2	4,2	17,7	146,7	8,6	155,3	12 12	3 24	5 59	28 47
12	163,0	161,2	4,2	18,9	147,4	8,6	156,0	12 6	3 18	7 55	5 0 43
15	164,0	162,2	4,3	18,3	148,2	8,6	156,8	11 56	3 8	10 48	3 36
18	163,5	161,7	4,3	18,6	147,4	8,5	155,9	11 47	2 59	13 42	6 30
21	165,5	163,7	4,3	18,9	149,1	8,5	157,6	11 37	2 49	16 36	9 24
23	164,0	162,2	4,3	19,0	147,5	8,5	156,0	11 31	2 43	18 34	11 22
25	164,0	162,2	4,4	19,1	147,5	8,5	156,0	11 25	2 37	20 28	13 16
: : 29	166,0	164,2	4,4	19,3	149,3	8,5	157,8	11 22	2 24	24 21	17 9
Sept. 3	165,0	163,2	4,4	19,4	148,2	8,5	156,7	10 56	2 8	29 13	22 1
6	165,0	163,2	4,4	19,4	148,2	8,5	156,7	10 47	1 59	9 2 9	24 57
9	164,3	162,5	4,5	19,3	147,7	8,5	156,2	10 37	1 49	5 4	27 52
10	164,3	162,5	4,5	19,3	147,7	8,5	156,2	10 34	1 46	6 3	28 51

β Drac.	Measured Dist.	Reduced Dist.	Precession.	Aberration.	Corrected Dist.	Nutation.	Mean Dist.	Argument of Nutation.	Moon's ascend. Node.	Argument of Aberration.	Sun's Longitude.
			+	−		+					
	″	″	″	″	″	″	″	S. ° ′	S. ° ′	S. ° ′	S. ° ′
1728, Sept. 11	164,7	162,9	4,5	19,2	148,2	8,5	156,7	2 10 31	11 1 43	9 7 2	5 29 50
15	165,0	163,2	4,5	19,0	148,7	8,5	157,2	10 18	1 30	10 57	6 3 45
16	163,3	161,5	4,5	18,9	147,1	8,5	155,6	10 15	1 27	11 56	4 44
28	162,0	160,2	4,6	17,7	147,1	8,5	155,6	9 37	0 49	23 45	16 33
Oct. : : 11	161,0	159,2	4,8	15,6	148,4	8,4	156,8	8 55	0 7	10 6 39	29 27
: : 22	159,5	157,8	4,8	13,1	149,5	8,4	157,9	8 20	10 29 32	17 38	7 10 26
Nov. 19	150,0	148,4	5,1	4,8	148,7	8,3	157,0	6 52	28 4	11 15 50	8 8 38
22	149,0	147,4	5,1	3,7	148,8	8,3	157,1	6 42	27 54	18 52	11 40
23	149,0	147,4	5,1	3,4	149,1	8,3	157,4	6 39	27 51	19 53	12 41
28	147,5	145,9	5,2	− 1,7	149,4	8,2	157,6	6 23	27 35	24 57	17 45
Dec. 19	140,5	139,0	5,3	+ 5,4	149,7	8,2	157,9	5 16	26 28	0 16 18	9 10 6
1729, Jan. : 20	130,0	128,6	5,6	14,6	148,8	8,1	156,9	3 35	24 47	1 18 47	10 12 35
: 25	129,5	128,1	5,6	15,7	149,4	8,1	158,0	3 19	24 31	23 50	17 38
28	128,5	127,1	5,6	16,2	148,9	8,0	156,9	3 9	24 21	26 51	20 39
Feb. 5	126,5	125,1	5,7	17,6	148,4	8,0	156,4	2 44	23 56	2 4 54	28 42
6	126,5	125,1	5,7	17,7	148,5	8,0	156,5	2 41	23 53	5 54	29 42
14	125,5	124,1	5,8	18,7	148,6	8,0	156,5	2 18	23 30	13 55	11 7 43
21	123,8	122,5	5,9	19,1	147,5	7,9	155,4	1 53	23 5	20 55	14 43
25	125,4	124,0	5,9	19,3	149,2	7,9	157,1	1 40	22 52	24 54	18 42
26	125,5	124,1	5,9	19,3	149,3	7,9	157,2	1 37	22 49	25 54	19 42
27	125,2	123,8	5,9	19,4	149,1	7,9	157,0	1 34	22 46	26 53	20 41
March 6	124,5	123,1	6,0	19,4	148,5	7,9	156,4	1 2	22 14	3 3 49	27 37
: 17	125,3	123,9	6,1	18,8	148,8	7,8	156,6	0 37	21 49	14 40	0 8 28
: 18	125,0	123,6	6,1	18,7	148,4	7,8	156,2	0 34	21 46	15 39	9 27
: : 20	126,5	125,1	6,1	18,5	149,7	7,8	157,5	0 28	21 40	17 37	11 25
: 24	127,5	126,1	6,2	18,1	150,4	7,8	158,2	0 14	21 26	21 33	15 21
Apr. 16	131,0	129,6	6,3	13,7	149,6	7,7	157,3	1 29 1	20 13	4 14 56	1 7 44
May 29	143,7	142,1	6,7	1,3	150,1	7,5	157,6	26 45	17 57	5 26 8	2 18 56
31	142,7	141,2	6,7	+ 0,7	148,6	7,5	156,1	26 38	17 50	28 2	20 50
June 2	142,6	141,0	6,7	0,0	147,7	7,5	155,2	26 32	17 44	29 57	22 45
5	145,5	143,9	6,8	− 0,9	149,8	7,5	157,3	26 22	17 34	6 2 48	25 36
7	145,2	143,6	6,8	1,6	148,8	7,5	156,3	26 16	17 28	4 42	27 30
er. 30	149,6	148,0	7,0	8,7	146,3	7,4		25 3	16 15	26 34	3 19 22
July 1	152,0	150,3	7,0	9,0	148,3	7,4	155,7	25 0	16 12	27 31	20 19
10	155,6	153,9	7,1	11,4	149,6	7,3	156,9	24 31	15 43	7 6 5	28 53
27	159,2	157,5	7,2	15,3	149,4	7,3	156,7	23 37	14 49	22 18	4 15 6
: 28	159,0	157,3	7,2	15,5	149,0	7,3	156,3	23 34	14 46	23 16	16 4
29	160,0	158,3	7,2	15,8	149,7	7,3	157,0	23 31	14 43	24 14	17 2
30	160,0	158,2	7,2	15,9	149,5	7,2	156,7	23 28	14 40	25 11	17 59
Aug. 5	159,7	158,0	7,2	16,9	148,3	7,2	155,5	23 19	14 21	8 0 56	23 44
6	160,5	158,8	7,3	17,1	149,0	7,2	156,2	23 6	14 18	1 54	24 42
: 10	163,0	161,2	7,3	17,7	150,8	7,2	158,0	22 53	14 5	5 44	28 32
: 12	161,0	159,2	7,3	18,0	148,5	7,2	155,7	22 46	13 58	7 40	5 0 28
13	161,8	160,0	7,3	18,1	149,2	7,2	156,4	22 43	13 55	8 38	1 26
17	162,0	160,2	7,3	18,5	149,0	7,1	156,1	22 31	13 43	12 30	5 18
Dr. Hoadley 23	161,7	159,9	7,4	19,0	148,3	7,1	155,4	22 11	13 23	18 18	11 6
: 25	162,7	160,9	7,4	19,1	149,2	7,1	156,3	22 5	13 17	20 15	13 3
28	162,8	161,0	7,5	19,2	149,3	7,1	156,4	21 56	13 8	23 9	15 57

β Drac.	Measured Dist.	Reduced Dist.	Precession.	Aberration.	Corrected Dist.	Nutation.	Mean Dist.	Argument of Nutation.	Moon's ascend. Node.	Argument of Aberration.	Sun's Longitude.
			+	−		+					
	"	"	"	"	"	"	"	S. ° ′	S. ° ′	S. ° ′	S. ° ′
1729, Aug. 29	162,5	160,7	7,5	19,3	148,9	7,1	156,0	1 21 52	10 13 4	8 24 7	5 16 55
Sept. 2	162,3	160,6	7,5	19,4	148,7	7,1	155,8	21 40	12 52	28 0	20 48
: 8	161,8	160,0	7,6	19,3	148,3	7,0	155,3	21 21	12 33	9 3 51	26 39
: 9	162,7	160,9	7,6	19,3	149,2	7,0	156,2	21 18	12 30	4 50	27 38
13	163,3	161,5	7,6	19,2	149,9	7,0	156,9	21 5	12 17	8 45	6 1 33
26	160,7	159,0	7,7	18,1	148,6	6,9	155,5	20 23	11 35	21 32	14 20
Oct. 4	160,8	159,1	7,8	16,9	150,0	6,9	156,9	19 58	11 10	29 27	22 15
7	160,0	158,3	7,8	16,4	149,7	6,9	156,6	19 48	11 0	10 2 27	25 15
10	159,0	157,3	7,8	15,8	149,3	6,9	156,2	19 39	10 51	5 25	28 13
14	159,0	157,3	7,9	15,0	150,2	6,8	157,0	19 26	10 38	9 24	7 2 12
18	158,0	156,3	7,9	14,1	150,1	6,8	156,9	19 14	10 26	13 23	6 11
Nov. 26	145,5	143,9	8,2	− 2,5	149,6	6,6	156,2	17 10	8 22	11 22 41	8 15 29
Dec. 3	144,5	142,9	8,3	0,0	151,2	6,6	157,8	16 47	7 59	29 48	23 36
: : 6	143,5	141,9	8,3	+ 1,0	151,2	6,5	157,8	16 38	7 50	0 2 51	26 39
1730, March 10	123,8	122,5	9,1	19,2	150,8	6,0	156,8	11 39	2 51	3 7 33	0 1 21
: : 16	125,0	123,6	9,1	18,9	151,6	5,9	157,5	11 20	2 32	13 27	7 15
18	124,2	122,8	9,2	+18,7	150,7	5,9	156,6	11 14	2 26	15 25	9 13
June 6	145,5	143,9	9,8	− 1,2	152,5	5,4	157,9	7 0	9 28 12	6 3 31	2 26 19
7	145,0	143,4	9,9	1,5	151,8	5,4	157,2	6 56	28 8	4 28	27 16
Aug. 18	160,5	158,8	10,4	18,6	150,6	4,9	155,5	3 8	24 20	8 13 13	5 6 1
23	161,0	159,2	10,5	19,0	150,7	4,9	155,6	2 52	24 4	18 4	10 52
24	161,5	159,7	10,5	19,1	151,1	4,9	156,0	2 49	24 1	19 2	11 50
25	161,0	159,3	10,5	19,1	150,7	4,9	155,6	2 46	23 58	20 0	12 48
26	161,0	159,2	10,5	19,2	150,5	4,9	155,4	2 42	23 54	20 58	13 46
31	161,5	159,7	10,6	19,4	150,9	4,8	155,7	2 26	23 38	25 50	18 38
Sept. 18	162,2	160,4	10,7	18,9	152,2	4,7	156,9	1 29	22 41	9 13 25	6 6 13
20	161,4	159,7	10,7	18,7	151,7	4,7	156,4	1 23	22 35	15 23	8 11
22	161,5	159,7	10,8	−18,4	152,1	4,7	156,8	1 16	22 28	17 21	10 9
Dec. 12	141,0	139,5	11,4	+ 2,9	153,8	4,1	157,9	0 26 59	18 11	0 9 42	9 2 30
15	139,7	138,2	11,5	4,0	153,7	4,1	157,8	26 50	18 2	12 45	5 33
18	137,7	136,2	11,5	4,9	152,6	4,0	156,6	26 40	17 52	15 47	8 35
: 28	135,5	134,0	11,6	8,2	153,8	4,0	157,8	26 5	17 17	25 58	18 46
1731, Feb. 6	124,3	122,9	12,0	+17,7	152,6	3,7	156,3	24 1	15 13	2 6 25	10 29 13
Aug. 4	158,5	156,8	13,4	−16,7	153,5	2,3	155,8	14 32	5 44	1 29 31	4 22 19
14	159,8	158,1	13,5	18,1	153,5	2,2	155,7	14 0	5 12	8 9 8	5 1 56
17	160,0	158,3	13,5	18,5	153,3	2,2	155,5	13 51	5 3	12 2	4 50
18	160,2	158,4	13,5	18,6	153,3	2,2	155,5	13 48	5 0	13 0	5 48
20	161,0	159,2	13,6	18,7	154,1	2,1	156,2	13 42	4 54	14 56	7 44
22	160,0	158,3	13,6	18,9	153,0	2,1	155,2	13 35	4 47	16 52	9 40
23	160,5	158,7	13,6	18,9	153,4	2,1	155,5	13 32	4 44	17 50	10 38
Sept. 3	161,0	159,2	13,7	19,4	153,5	2,0	155,5	12 57	4 9	28 31	21 19
7	161,0	159,3	13,7	19,4	153,6	2,0	155,6	12 44	3 56	9 32 25	25 13
10	161,0	159,2	13,7	19,3	153,6	2,0	155,6	12 35	3 47	5 21	28 9
12	160,0	158,3	13,8	19,2	152,9	2,0	154,9	12 29	3 41	7 18	6 0 6
: 13	160,0	158,3	13,8	19,2	152,9	1,9	154,8	12 26	3 38	8 17	1 5
15	161,5	159,7	13,8	19,1	154,4	1,9	156,3	12 19	3 31	10 14	3 2
: : 19	159,7	158,0	13,8	18,8	153,0	1,9	154,9	12 6	3 18	14 10	6 58
20	159,5	157,8	13,8	18,8	152,8	1,9	154,7	12 3	3 15	15 9	7 57

β Drac.	Measured Dist.	Reduced Dist.	Precession.	Aberration.	Corrected Dist.	Nutation.	Mean Dist.	Argument of Nutation.	Moon's ascend. Node.	Argument of Aberration.	Sun's Longitude.
			+	−		+					
	"	"	"	"	"	"	"	S. ° ′	S. ° ′	S. ° ′	S. ° ′
1731, Sept. 22	160,0	158,3	13,9	18,5	153,7	1,9	155,6	0 11 57	9 3 9	9 17 7	6 9 55
: 25	159,0	157,3	13,9	18,2	153,0	1,8	154,8	11 47	2 59	20 4	12 52
Oct. 4	158,3	156,6	13,9	−17,0	153,5	1,8	155,3	11 19	2 31	28 59	21 47
Dec. 18	138,0	136,5	14,6	+ 4,9	156,0	+1,1	157,1	7 20	8 28 32	0 15 33	9 8 21
1732, Sept. 2	159,8	158,1	16,7	−19,4	155,4	−1,0	154,4	11 23 38	14 50	8 28 17	5 21 5
3	160,5	158,8	16,8	19,4	156,2	1,0	155,2	23 34	14 46	29 15	22 3
4	160,6	158,8	16,8	19,4	156,2	1,0	155,2	23 31	14 43	9 0 14	23 2
5	161,0	159,2	16,8	19,4	156,6	1,0	155,5	23 28	14 40	1 12	24 0
6	160,8	159,1	16,8	19,4	156,5	1,0	155,5	23 25	14 37	2 11	24 59
1733, Jan. 22	126,8	125,4	17,9	15,1	158,2	2,2	156,0	16 6	7 18	1 21 50	10 14 38
Aug. 25	162,0	160,2	19,7	19,1	160,8	2,4	158,4	14 46	7 25 58	8 20 16	5 13 4
26	162,5	160,7	19,8	19,1	161,4	2,4	159,0	14 43	25 55	21 14	14 2
29	161,5	159,7	19,8	19,2	160,3	2,4	157,9	14 40	25 42	24 9	16 57
1734, June 19	147,0	145,4	22,3	5,3	162,4	5,9	156,5	10 18 56	10 8	6 15 55	3 8 43
21	148,5	146,9	22,3	5,9	163,3	5,9	157,4	18 50	10 2	17 49	10 37
July 5	151,0	149,4	22,4	10,0	161,8	6,0	155,8	18 5	9 17	7 1 8	23 56
10	153,0	151,3	22,4	11,3	162,4	6,0	156,4	17 49	9 1	5 53	28 41
16	154,5	152,8	22,5	12,8	162,5	6,1	156,4	17 30	8 42	11 36	4 4 24
29	158,2	156,5	22,6	15,7	163,4	6,2	157,2	16 49	8 1	24 1	16 49
: : 31	157,5	155,8	22,6	16,1	162,3	6,2	156,1	16 43	7 55	25 57	18 45
Aug. 3	157,8	156,1	22,6	16,6	162,1	6,2	155,9	16 33	7 45	28 49	21 37
6	159,0	157,3	22,7	17,1	162,9	6,2	156,7	16 24	7 36	8 1 42	24 30
7	159,0	157,3	22,7	17,2	162,8	6,2	156,6	16 21	7 33	2 40	25 28
8	160,0	158,3	22,7	−17,4	163,6	6,2	157,4	16 18	7 30	3 38	26 26
Dec. 11	141,2	139,7	23,7	+ 2,6	166,0	6,9	159,1	9 40	0 52	0 8 43	9 1 31
22	136,0	134,5	23,8	6,3	164,6	7,0	157,6	9 5	0 17	19 54	12 42
23	135,3	133,8	23,8	6,6	164,2	7,0	157,2	9 2	0 14	20 55	13 43
27	134,5	133,0	23,9	+ 7,9	164,8	7,0	157,8	8 49	0 1	24 59	17 47
1735, Sept. 10	159,2	157,5	26,0	-19,3	164,2	8,1	156,1	9 25 13	6 16 25	9 5 23	5 28 11
13	158,8	157,1	26,1	19,1	164,1	8,2	155,9	25 3	16 15	8 19	6 1 7
1736, Aug. 31	157,5	155,8	29,0	19,3	165,5	8,9	156,6	6 22	5 27 34	8 26 21	5 19 9
Sept. 8	157,8	156,1	29,1	19,3	165,9	9,0	156,9	5 56	27 8	9 40 9	26 57
9	157,3	155,6	29,1	−19,3	165,4	9,0	156,4	5 53	27 5	5 8	27 56
1737, Jan. 4	126,7	125,3	30,1	+10,4	165,8	9,0	156,8	8 29 41	20 53	1 3 37	9 26 25
19	123,2	121,9	30,2	+14,3	166,4	9,0	157,4	28 54	20 6	18 49	10 11 37
Sept. 1	153,5	151,8	32,1	−19,3	164,6	8,8	155,8	16 59	8 11	8 27 6	5 19 54
2	154,0	152,3	32,1	19,4	165,0	8,8	156,2	16 56	8 8	28 4	20 52
: 4	152,7	151,0	32,1	19,4	163,7	8,8	154,9	16 50	8 2	9 0 1	22 49
6	154,0	152,3	32,1	19,4	165,0	8,8	156,2	16 43	7 55	1 58	24 46
7	152,5	150,9	32,2	19,4	163,7	8,8	154,9	16 40	7 52	2 56	25 44
8	154,0	152,3	32,2	19,4	165,1	8,8	156,3	16 37	7 49	3 55	26 43
1738, July 25	146,8	145,2	34,8	14,9	165,1	7,8	157,3	7 29 40	4 20 52	7 20 14	4 13 2
28	147,3	145,7	34,9	15,5	165,1	7,8	157,3	29 30	20 42	23 6	15 54
: 29	146,5	144,9	34,9	15,7	164,1	7,7	156,4	29 27	20 39	24 3	16 51
Sept. 13	150,5	148,9	35,3	19,2	165,0	7,6	157,4	27 1	18 13	9 8 35	6 1 23
: : 23	149,3	147,7	35,4	−18,4	164,7	7,5	157,2	26 26	17 38	18 24	11 12
1739, Jan. 24	112,5	111,3	36,4	+15,3	163,0	6,9	156,1	20 8	11 20	1 23 23	10 16 11
Aug. 19	144,3	142,7	38,1	−18,6	162,2	5,7	156,5	9 1	0 13	8 14 1	5 6 49

β Drac.	Measured Dist.	Reduced Dist.	Precession.	Aberration.	Corrected Dist.	Nutation.	Mean Dist.	Argument of Nutation.	Moon's ascend. Node.	Argument of Aberration.	Sun's Longitude.
			+	−		−					
	″	″	″	″	″	″	″	S. ° ′	S. ° ′	S. ° ′	S. ° ′
1739, Aug. 20	144,2	142,6	38,1	18,7	162,0	5,7	156,3	7 8 58	4 0 10	8 14 59	5 7 47
24	143,5	142,0	38,2	19,0	161,2	5,7	155,5	8 45	3 29 57	18 51	11 39
26	144,0	142,4	38,2	19,1	161,5	5,6	155,9	8 39	29 51	20 48	13 36
27	144,3	142,7	38,2	19,2	161,7	5,6	156,1	8 36	29 48	21 47	14 34
: 29	143,2	141,6	38,2	19,2	160,6	5,6	155,0	8 29	29 41	23 42	16 30
30	143,3	141,8	38,2	19,3	160,7	5,6	155,1	8 26	29 38	24 40	17 28
31	143,8	142,2	38,3	19,3	161,2	5,6	155,6	8 23	29 35	25 39	18 27
Sept. 2	143,6	142,0	38,3	−19,4	160,9	5,6	155,3	8 16	29 28	27 35	20 23
1740, Jan. 20	108,2	107,0	39,4	+14,4	160,8	4,6	156,2	0 52	22 4	1 19 6	10 11 54
: : 26	106,5	105,3	39,5	+15,7	160,5	4,6	155,9	0 33	21 45	25 10	17 58
Aug. 9	136,5	135,0	41,1	−17,6	158,5	3,1	155,4	6 20 10	11 22	8 5 6	4 27 54
18	137,0	135,5	41,2	18,6	158,1	3,0	155,1	19 41	10 53	13 47	5 6 35
19	137,0	135,5	41,2	18,7	158,0	3,0	155,0	19 38	10 50	14 45	7 33
21	138,2	136,7	41,2	18,9	159,0	3,0	156,0	19 32	10 44	16 41	9 29
24	137,2	135,7	41,2	19,1	157,8	3,0	154,8	19 22	10 34	19 35	12 23
25	138,0	136,5	41,3	19,2	158,6	3,0	155,6	19 19	10 31	20 34	13 22
28	137,5	136,0	41,3	19,2	158,1	3,0	155,1	19 9	10 21	23 28	16 16
29	137,7	136,2	41,3	19,3	158,2	2,9	155,3	19 6	10 18	24 26	17 14
Sept. : 5	136,0	134,5	41,3	19,4	156,4	2,9	153,5	18 44	9 56	9 1 15	24 3
8	137,0	135,5	41,4	19,4	157,5	2,9	154,6	18 34	9 46	4 11	26 59
20	135,0	133,5	41,5	−18,7	156,3	2,8	153,5	17 56	9 8	15 57	6 8 45
1741, Jan. 26	99,0	97,9	42,6	+15,9	156,4	−1,8	154,6	11 14	2 26	1 25 56	10 18 44
Aug. 27	131,2	129,8	44,3	−19,2	154,9	0,0	154,9	5 29 53	2 21 5	8 22 16	5 15 4
28	131,2	129,8	44,3	19,2	154,9	0,0	154,9	29 50	21 2	23 14	16 2
29	130,8	129,4	44,3	19,3	154,4	0,0	154,4	29 47	20 59	24 12	17 0
31	132,0	130,6	44,4	19,3	155,7	0,0	155,7	29 40	20 52	26 9	18 57
Sept. 2	131,7	130,2	44,4	19,4	155,2	+0,1	155,3	39 34	20 46	28 6	20 54
6	131,0	129,6	44,4	19,4	154,6	0,1	154,7	29 21	20 33	9 2 0	24 48
21	130,0	128,6	44,5	18,5	154,6	0,2	154,8	28 33	19 45	16 42	6 9 30
1742, Sept. 5	125,7	124,3	47,4	19,4	152,3	3,1	155,4	10 5	1 17	0 47	5 23 35
6	125,3	123,9	47,5	19,4	152,0	3,1	155,1	10 1	1 13	1 45	24 33
1743, Sept. 2	119,0	117,7	50,5	19,3	148,9	5,7	154,6	4 20 54	1 12 6	8 27 38	20 26
3	119,5	118,2	50,5	19,4	149,3	5,7	155,0	20 51	12 3	28 36	21 24
1745, Sept. 2	108,5	107,3	56,6	19,4	144,5	8,8	153,3	3 12 12	0 3 24	28 8	20 56
5	108,8	107,6	56,6	19,4	144,8	8,8	153,6	12 2	3 14	9 1 3	23 51
1746, Sept. : 15	107,2	106,0	59,8	19,0	146,8	8,9	155,7	2 22 11	11 13 23	10 36	6 3 24
19	107,5	106,3	59,8	18,8	147,3	8,9	156,2	21 58	13 10	14 31	7 19
20	106,0	104,8	59,8	18,7	145,9	8,9	154,8	21 55	13 7	15 30	8 18
1747, Sept. 1	104,5	103,4	62,7	19,3	146,8	8,1	154,9	3 35	10 24 47	8 26 42	5 19 30
2	105,5	104,3	62,7	19,4	147,6	8,0	155,6	3 32	24 44	27 40	20 27

α Cassiopeæ south of 34° 55′ to Sept. 27, 1729 inclusive, and in 1730, June 7, 11, 19. North in the other observations.

+19″,93 annual precession.

16″,55 maximum of aberration.

11^{s} 23° 33′=12^{s}− right ascension of the star.

11^{s} 20° 2′=longitude of the sun's place when the aberration in declination is 0.

α Cass.	Measured Dist.	Reduced Dist.	Precession.	Aberration.	Corrected Dist.	Nutation.	Mean Dist.	Argument of Nutation.	Moon's ascend. Node.	Argument of Aberration.	Sun's Longitude.
			+	−		+					
	″	″	″	″	″	″	″	S. ° ′	S. ° ′	S. ° ′	S. ° ′
1727, Aug. 30	60,8	60,1	8,5	−0,7	67,9	2,3	70,2	11 15 17	11 21 44	5 27 38	5 17 40
Sept. 4	::56,3	55,7	8,7	+0,7	65,1	2,3	67,4	15 1	21 28	6 2 30	22 32
8	55,0	54,4	9,0	1,8	65,2	2,3	67,5	14 48	21 15	6 25	26 27
9	55,6	55,0	9,0	2,1	66,1	2,4	68,5	14 45	21 12	7 23	27 25
11	: 54,7	54,1	9,1	2,7	65,9	2,4	68,3	14 39	21 6	9 20	29 22
15	: 54,5	53,9	9,4	3,8	67,1	2,4	69,5	14 26	20 53	13 15	6 3 17
16	52,0	51,4	9,4	4,1	64,9	2,4	67,3	14 23	20 50	14 14	4 16
19	52,9	52,3	9,6	4,9	66,8	2,4	69,2	14 13	20 40	17 17	7 13
Oct. 8	46,8	46,3	10,6	9,7	66,6	2,6	69,2	13 13	19 40	7 5 59	26 1
12	45,5	45,0	10,8	10,6	66,4	2,6	69,0	13 0	19 27	9 58	7 0 0
13	f:45,0	44,5	10,8	10,9	66,2	2,6	68,8	12 57	19 24	10 57	0 59
q. 20	42,6	42,1	11,3	12,3	65,7	2,7	68,4	12 35	19 2	17 56	7 58
23	::42,0	41,5	11,4	12,8	65,7	2,7	68,4	12 25	18 52	20 56	10 58
26	: 40,0	39,6	11,6	13,4	64,6	2,7	67,3	12 16	18 43	23 56	13 58
Nov. 12	38,0	37,6	12,5	15,7	65,8	2,9	68,7	11 22	17 49	8 11 4	8 1 6
14	37,5	37,1	12,6	15,9	65,6	2,9	68,5	11 16	17 43	13 5	3 7
22	36,5	36,1	13,1	16,4	65,6	2,9	68,5	10 50	17 17	21 11	11 13
Dec. 5	36,2	35,8	13,8	16,5	66,1	3,1	69,2	10 9	16 36	9 4 22	24 24
12	34,7	34,3	14,2	16,2	64,7	3,1	67,8	9 46	16 13	11 28	9 1 30
13	34,2	33,8	14,2	16,2	64,2	3,1	67,3	9 43	16 10	12 29	2 31
14	34,2	33,8	14,3	16,1	64,2	3,1	67,3	9 40	16 7	13 31	3 33
21	: 35,0	34,6	14,6	15,5	64,7	3,2	67,9	9 18	15 45	20 38	10 40
29	35,0	34,6	15,1	14,6	64,3	3,2	67,5	8 52	15 19	28 46	18 48
1728, Jan. 3	36,0	35,6	15,3	13,7	64,6	3,3	67,9	8 36	15 3	10 4 50	24 52
16	38,0	37,6	16,1	11,3	65,0	3,4	68,4	7 55	14 22	17 1	10 7 3
20	38,1	37,7	16,3	10,6	64,6	3,4	68,0	7 42	14 9	21 4	11 6
Feb. 2	40,5	40,1	17,0	7,2	64,3	3,5	67,8	7 1	13 28	11 4 11	24 13
3	42,5	42,0	17,1	7,0	66,1	3,5	69,6	6 58	13 25	5 11	25 13
5	41,5	41,1	17,2	+ 6,5	64,8	3,5	68,3	6 52	13 19	7 12	27 14
March 12	::51,5	50,9	19,1	− 3,7	66,3	3,8	70,1	4 57	11 24	0 13 3	0 3 5
21	::51,3	50,7	19,6	6,2	64,1	3,9	68,0	4 28	10 55	22 54	12 56
23	51,4	50,8	19,7	6,7	63,8	3,9	67,7	4 22	10 49	24 51	14 53
April 7	54,6	54,0	20,5	10,3	64,2	4,0	68,2	3 31	9 58	1 9 30	29 32
18	57,6	57,0	21,1	12,5	65,6	4,1	69,7	3 0	9 27	20 9	1 10 11
May 8	59,0	58,4	22,2	15,5	65,1	4,2	69,3	1 55	8 23	2 9 23	29 25
June 7	56,8	56,2	23,8	16,4	63,6	4,5	68,1	0 21	6 48	3 7 59	2 28 1

α Cass.	Measured Dist.	Reduced Dist.	Precession.	Aberration.	Corrected Dist.	Nutation.	Mean Dist.	Argument of Nutation.	Moon's ascend. Node.	Argument of Aberration.	Sun's Longitude.
	″	″	+ ″	− ″	″	+ ″	″	S. ° ′	S. ° ′	S. ° ′	S. ° ′
1728, June 17	56,0	55,4	24,4	15,9	63,9	4,5	68,4	10 29 49	11 6 16	3 17 30	3 7 32
23	56,7	56,1	24,7	15,3	65,5	4,6	70,1	29 30	5 57	23 12	13 14
24	56,0	55,4	24,7	15,2	64,9	4,6	69,5	29 27	5 54	24 9	14 11
28	54,0	53,4	25,0	14,8	63,6	4,6	68,2	29 14	5 41	27 57	17 59
July 5	53,4	52,8	25,4	13,6	64,6	4,6	69,2	28 52	5 19	4 4 36	24 38
manè. 27	48,0	47,5	26,6	9,6	64,5	4,8	69,3	27 42	4 9	25 35	4 15 37
29	46,8	46,3	26,7	9,1	63,9	4,8	68,7	27 35	4 3	27 30	17 32
31:	46,5	46,0	26,8	− 8,7	64,1	4,8	68,9	27 29	3 56	29 24	19 26
Sept. 13	33,0	32,6	29,2	+ 3,4	65,2	5,1	70,3	25 9	1 36	6 12 3	6 2 5
17:	33,0	32,6	29,4	4,6	66,6	5,2	71,8	24 57	1 24	15 58	6 0
18	30,0	29,7	29,5	4,8	64,0	5,2	69,2	24 54	1 21	16 57	6 59
19	29,5	29,2	29,5	5,1	63,8	5,2	69,0	24 51	1 18	17 56	7 58
20	29,8	29,5	29,6	5,3	64,4	5,2	69,6	24 48	1 14	18 55	8 57
Oct. 15	21,3	21,1	30,9	11,4	63,4	5,4	68,8	23 28	10 29 55	7 13 42	7 3 44
18	20,0	19,8	31,1	12,2	63,1	5,4	68,5	23 18	29 45	17 42	7 44
31	17,6	17,4	31,8	14,2	63,4	5,5	68,9	22 37	29 4	28 43	18 45
Nov. 17	15,0	14,8	32,7	16,1	63,6	5,6	69,2	21 43	28 10	8 16 53	8 6 55
22	14,0	14,8	33,0	16,3	64,1	5,6	69,7	21 27	27 54	21 56	11 58
23:	13,0	12,8	33,1	16,4	62,3	5,6	67,9	21 24	27 51	22 57	12 59
29	14,5	14,3	33,4	16,5	64,2	5,6	69,8	21 5	27 32	29 2	19 4
30	13,5	13,3	33,4	16,5	63,2	5,7	68,9	21 1	27 28	9 0 3	20 5
Dec. 1	13,2	13,1	33,5	16,5	63,1	5,7	68,8	20 58	27 25	1 4	21 6
4	13,5	13,3	33,6	16,5	63,4	5,7	69,1	20 49	27 16	4 7	24 9
9	11,8	11,7	33,9	16,4	62,0	5,7	67,7	20 33	27 0	9 12	29 14
11	12,8	12,7	34,0	16,2	62,9	5,7	68,6	20 27	26 54	11 14	9 1 16
13	12,2	12,1	34,1	16,1	62,3	5,7	68,0	20 21	26 48	13 16	3 18
15	12,3	12,2	34,2	16,0	62,4	5,8	68,2	20 14	26 41	15 18	5 20
16	12,0	11,8	34,3	15,9	62,0	5,8	67,8	20 11	26 38	16 18	6 20
1729, Jan. 25	17,5	17,3	36,5	9,2	63,0	6,0	69,0	18 4	24 31	10 26 53	10 16 55
Feb. 25	24,3	24,0	38,2	0,6	62,8	6,2	69,0	16 25	22 52	11 27 58	11 18 0
26	23,0	22,7	38,2	+ 0,3	61,2	6,2	67,4	16 22	22 49	28 59	18 59
March 17:	29,5	29,2	39,3	− 5,0	63,5	6,3	69,8	15 22	21 49	0 18 44	0 8 46
18:	31,5	31,2	39,3	5,3	65,2	6,3	71,5	15 19	21 46	19 43	9 45
April 9:	33,0	32,7	40,6	10,7	62,6	6,5	69,1	14 9	20 36	1 11 12	1 1 14
June 3	35,7	35,4	43,6	16,5	62,5	6,8	69,3	11 14	17 41	3 3 57	2 23 59
4	36,7	36,4	43,6	16,5	63,5	6,8	70,3	11 11	17 38	4 54	24 56
5	36,5	36,2	43,7	16,4	63,5	6,8	70,3	11 8	17 35	5 51	25 53
6	35,6	35,3	43,7	16,4	62,6	6,8	69,4	11 4	17 31	6 48	26 50
7	36,5	36,2	43,8	16,4	63,6	6,8	70,4	11 1	17 28	7 45	27 47
8:	37,0	36,7	43,8	16,4	64,1	6,8	70,9	10 58	17 25	8 42	28 44
11	35,5	35,2	44,0	16,3	62,9	6,8	69,7	10 47	17 15	11 33	3 1 35
24	33,0	32,7	44,7	−15,2	62,2	6,9	69,1	10 7	16 34	21 54	11 56
Sept. 27	5,5	5,4	49,9	+ 7,1	62,4	7,4	69,8	5 5	11 32	6 25 35	6 15 37
Oct. 14	1,0	1,0	50,8	11,2	61,0	7,4	68,4	4 11	10 38	7 12 27	7 2 29
15	1,0	1,0	50,8	11,4	61,2	7,4	68,6	4 8	10 35	13 27	3 29
17	0,7	0,7	50,9	11,8	62,0	7,5	69,5	4 2	10 29	15 27	5 29
Nov. 23 :	9,0	8,9	53,0	16,4	60,5	7,6	68,1	2 4	8 31	8 22 42	8 12 44
28	8,2	8,1	53,3	16,5	61,7	7,6	69,3	1 48	8 15	27 46	17 48

α Cass.	Measured Dist.	Reduced Dist.	Precession.	Aberration.	Corrected Dist.	Nutation.	Mean Dist.	Argument of Nutation.	Moon's ascend. Node.	Argument of Aberration.	Sun's Longitude.
			+	+		+					
	"	"	"	"	"	"	"	S. ° ′	S. ° ′	S. ° ′	S. ° ′
1729, Dec. 2	9,3	9,2	53,5	16,5	60,8	7,6	68,4	10 1 36	10 8 3	9 2 51	8 22 53
3	9,5	9,4	53,5	16,5	60,6	7,7	68,3	1 33	8 0	3 51	23 53
5	8,5	8,4	53,6	16,5	61,7	7,7	69,4	1 26	7 53	4 53	24 55
10	10,8	10,7	53,9	16,4	59,6	7,7	67,3	1 10	7 37	9 58	9 0 0
11	10,0	9,9	54,0	16,3	60,4	7,7	68,1	1 7	7 34	10 59	1 1
1730, June : 7	14,0	13,8	63,7	16,4	61,1	8,3	69,4	9 21 41	9 28 8	3 7 31	2 27 33
11	14,0	13,8	63,9	16,3	61,4	8,4	69,8	21 29	27 56	11 20	3 1 22
19	13,3	13,2	64,4	15,8	61,8	8,4	70,2	21 3	27 30	18 56	8 58
Dec. 9	31,0	30,8	73,8	16,4	59,4	8,8	68,2	11 54	18 21	9 9 42	8 29 44
11	31,5	31,3	73,9	16,3	58,9	8,8	67,7	11 47	18 14	10 44	9 0 46
12	32,5	32,2	74,0	16,2	58,0	8,8	66,8	11 44	18 11	11 45	1 47
1732, Jan. 8	49,7	49,2	95,4	+12,9	59,1	8,9	68,0	8 20 59	8 27 26	10 8 55	28 57
April 17	27,5	27,2	100,8	−12,3	61,3	8,7	70,0	15 41	22 8	1 19 3	1 9 15
Dec. 30	68,0	67,3	114,8	+14,2	61,7	8,0	69,7	2 4	8 31	10 0 34	9 20 36
1733, Jan. 21	65,5	64,8	116,0	+10,0	61,2	7,9	69,1	0 54	7 21	22 52	10 12 54
1734, June 12	q63,0	62,3	143,7	−16,3	65,1	5,0	70,1	7 4 4	7 10 31	3 12 18	3 2 20
13	63,5	62,8	143,7	−16,2	64,7	5,0	69,7	4 0	10 27	13 15	3 17
Dec. 11	106,6	105,4	153,6	+16,3	64,5	3,7	68,2	6 24 25	0 52	9 10 46	9 0 48
22	106,0	104,8	154,2	15,3	64,7	3,6	68,3	23 50	0 17	21 57	11 59
25	107,0	105,8	154,4	15,0	63,6	+3,6	67,2	23 41	7 0 8	25 0	15 2
1738, Dec. 23	178,2	176,3	234,0	15,2	72,9	−7,2	65,7	4 6 25	4 12 52	22 59	13 1
28	178,0	176,1	234,3	+14,6	72,8	7,3	65,5	6 9	12 36	28 4	18 6
1740, June 2	171,0	169,1	262,7	−16,5	77,1	−8,9	68,2	3 8 31	3 14 58	3 3 19	2 23 21
1747, Feb. 27	336,0	332,3	397,1	+ 0,1	64,9	+4,7	69,6	10 28 11	11 4 38	11 29 35	11 19 37

β Cassiopeæ north of 32° 20′.

−20″,053 annual precession.

17″,51 maximum of aberration.

0ˢ 1° 11′=12ʳ − right ascension of the star.

11ˢ 14° 53′=longitude of the sun's place when the aberration in declination is 0.

β Cass.	Measured Dist.	Reduced Dist.	Precession.	Aberration.	Corrected Dist.	Nutation.	Mean Dist.	Argument of Nutation.	Moon's ascend. Node.	Argument of Aberration.	Sun's Longitude.
			−	+		−					
	"	"	"	"	"	"	"	S. ° ′	S. ° ′	S. ° ′	S. ° ′
1727, Aug. 22	33,0	32,6	8,1	1,5	26,0	1,0	25,0	11 23 20	11 22 9	5 25 1	5 9 54
30	33,3	32,9	8,6	− 0,8	23,5	1,1	22,4	22 55	21 44	6 2 46	17 39
Sept. 4	38,4	38,0	8,8	2,3	26,9	1,1	25,8	22 39	21 28	7 38	22 31
8	40,7	40,3	9,1	3,5	27,7	1,2	26,5	22 26	21 15	11 33	26 26
9	42,0	41,5	9,1	3,8	28,6	1,2	27,4	22 23	21 12	12 30	27 23
11	::41,9	41,4	9,2	4,4	27,8	1,2	26,6	22 17	21 6	14 28	29 21
15	43,5	43,0	9,4	5,5	28,1	1,2	26,9	22 1	20 50	18 23	6 3 16
16	44,3	43,8	9,5	5,8	28,5	1,3	27,2	21 58	20 47	19 22	4 15

β Cass.	Measured Dist.	Reduced Dist.	Precession.	Aberration.	Corrected Dist.	Nutation.	Mean Dist.	Argument of Nutation.	Moon's ascend. Node.	Argument of Aberration.	Sun's Longitude.
			—	—		—					
	″	″	″	″	″	″	″	S. ° ′	S. ° ′	S. ° ′	S. ° ′
1727, Sept. 19	43,8	43,3	9,7	6,7	26,9	1,3	25,6	11 21 51	11 20 40	6 22 19	6 7 12
27	45,7	45,2	10,1	8,8	26,3	1,3	25,0	21 26	20 15	7 0 11	15 4
29	47,4	46,9	10,2	9,3	27,4	1,4	26,0	21 20	20 9	2 10	17 3
Oct. 8	49,7	49,2	10,7	11,5	27,0	1,4	25,6	20 51	19 40	11 6	25 59
13	::51,7	51,1	11,0	12,6	27,5	1,5	26,0	20 35	19 24	16 5	7 0 58
20	: 53,3	52,7	11,4	14,0	27,3	1,6	25,7	20 13	19 2	23 4	7 57
f. 23	::54,3	53,7	11,5	14,5	27,7	1,6	26,1	20 3	18 52	26 4	10 57
26	55,0	54,4	11,7	15,0	27,7	1,6	26,1	19 54	18 43	29 4	13 57
Nov. 14	57,8	57,2	12,8	17,1	27,3	1,7	25,6	18 53	17 42	8 18 12	8 3 5
22	60,0	59,3	13,2	17,4	28,7	1,8	26,9	18 28	17 17	26 19	11 12
Dec. 5	: 59,6	59,0	13,9	17,3	27,8	1,9	25,9	17 46	16 36	9 9 29	24 22
6	60,8	60,1	14,0	17,2	28,9	1,9	27,0	17 44	16 33	10 30	25 23
12	60,3	59,6	14,3	16,8	28,5	2,0	26,5	17 24	16 13	16 36	9 1 29
13	60,7	60,0	14,3	16,7	29,0	2,0	27,0	17 21	16 10	17 37	2 30
: 14	60,2	59,5	14,4	16,6	28,5	2,0	26,5	17 18	16 7	18 39	3 32
21	60,2	59,5	14,8	15,8	28,9	2,0	26,9	16 56	15 45	25 46	10 39
29	59,0	58,4	15,2	14,5	28,7	2,1	26,6	16 30	15 19	10 3 54	18 47
1728, Jan. 3	58,0	57,4	15,5	13,6	28,1	2,1	26,0	16 14	15 3	8 57	23 50
16	55,4	54,8	16,2	10,7	27,9	2,2	25,7	15 33	14 22	22 9	10 7 2
20	55,0	54,4	16,4	9,7	28,3	2,3	26,0	15 20	14 9	26 12	11 5
Feb. 2	53,5	52,9	17,1	6,2	29,6	2,4	27,2	14 39	13 28	11 9 18	24 11
3	51,6	51,0	17,2	5,9	27,9	2,4	25,5	14 36	13 25	10 19	25 12
5	51,0	50,4	17,3	– 5,3	27,8	2,4	25,4	14 30	13 19	12 20	27 13
March 21	42,0	41,5	19,7	+ 7,9	29,7	2,8	26,9	12 7	10 56	0 28 1	0 12 54
23	: 40,2	39,8	19,9	8,5	28,4	2,8	25,6	12 0	10 49	29 59	14 52
April 7	: 37,3	36,9	20,6	12,1	28,4	2,9	25,5	11 13	10 2	14 37	29 30
18	: 35,4	35,0	21,3	14,3	28,0	3,0	25,0	10 38	9 27	25 17	1 10 10
May 8	: 31,0	30,7	22,4	16,8	25,1	3,1	22,0	9 34	8 23	2 14 31	29 24
June 7	: 36,5	36,1	24,0	17,1	29,2	3,4	25,8	7 59	6 48	3 13 7	2 28 0
8	: 37,0	36,6	24,0	17,1	29,7	3,4	26,3	7 56	6 45	14 4	28 57
17	38,2	37,8	24,6	16,3	29,5	3,4	26,1	7 27	6 16	22 37	3 7 30
h. 23	: 37,3	36,9	24,9	15,5	27,5	3,5	24,0	7 8	5 57	28 19	13 12
h. 24	: 38,0	37,6	24,9	15,4	28,1	3,5	24,6	7 5	5 54	29 16	14 9
28	39,5	39,1	25,2	14,8	28,7	3,5	25,2	6 52	5 41	4 3 5	17 58
July 5	: 40,0	39,6	25,5	13,7	27,8	3,6	24,2	6 30	5 19	9 44	24 37
manè. 27	46,5	46,0	26,7	8,8	28,1	3,7	24,4	5 23	4 12	5 0 42	4 15 35
m. 29	47,0	46,5	26,8	8,3	28,0	3,8	24,2	5 17	4 6	2 37	17 30
m. 31	::48,5	48,0	26,9	+ 7,8	28,9	3,8	25,1	5 11	4 0	4 32	19 25
Sept. 13	63,7	63,0	29,4	– 4,9	28,7	4,1	24,6	2 47	1 36	6 17 11	6 2 4
m. 18	66,0	65,3	29,6	6,3	29,4	4,1	25,3	2 31	1 20	22 6	6 59
n. 20	66,2	65,5	29,7	7,1	28,7	4,2	24,5	2 25	1 14	24 3	8 56
Oct. 15	::73,0	72,2	31,2	13,2	27,8	4,3	23,5	1 6	10 29 55	7 18 49	7 3 42
18	75,5	74,7	31,3	13,8	29,6	4,4	25,2	0 56	29 45	21 49	6 42
19	75,7	74,9	31,4	14,0	29,5	4,4	25,1	0 53	29 42	22 49	7 42
31	78,0	77,2	32,0	15,6	29,6	4,5	25,1	0 15	29 4	8 4 51	19 44
Nov. 17	81,5	80,6	32,9	17,3	30,4	4,6	25,8	10 29 21	28 10	22 0	8 6 53
22	83,5	82,6	33,2	17,4	32,0	4,6	27,4	29 5	27 54	27 4	11 57
23	82,7	81,8	33,2	17,5	31,1	4,6	26,5	29 2	27 51	28 5	12 58

β Cass.	Measured Dist.	Reduced Dist.	Precession.	Aberration.	Corrected Dist.	Nutation.	Mean Dist.	Argument of Nutation.	Moon's ascend. Node.	Argument of Aberration.	Sun's Longitude.
	″	″	— ″	— ″	″	— ″	″	S. ° ′	S. ° ′	S. ° ′	S. ° ′
1728, Nov. 29	83,0	82,1	33,6	17,4	31,1	4,7	26,4	10 28 41	10 27 30	9 4 7	8 19 2
30	83,5	82,6	33,7	17,4	31,5	4,7	26,8	28 38	27 27	5 10	20 3
Dec. 1	83,0	82,1	33,7	17,4	31,0	4,7	26,3	28 35	27 24	6 11	21 4
4	82,3	81,4	33,9	17,3	30,2	4,7	25,5	28 27	27 16	9 14	24 7
9	83,0	82,1	34,2	17,0	30,9	4,7	26,2	28 11	27 0	14 19	29 12
11	83,5	82,6	34,3	16,8	31,5	4,8	26,7	28 5	26 54	16 21	9 1 14
13	82,5	81,6	34,4	16,7	30,5	4,8	25,7	27 59	26 48	18 23	3 16
16	83,0	82,1	34,6	16,4	31,1	4,8	26,3	27 49	26 38	21 26	6 19
21	83,3	82,4	34,9	15,7	31,8	4,8	27,0	27 33	26 22	26 35	11 28
1729, Jan. 22	78,0	77,2	36,6	− 9,0	31,6	5,0	26,6	25 51	24 40	10 28 59	10 13 52
Feb. 26	68,7	68,0	38,5	+ 1,2	30,7	5,3	25,0	24 0	22 49	0 4 5	11 18 58
March 17	: 63,5	62,8	39,6	6,8	30,0	5,4	24,6	23 0	21 49	23 51	0 8 44
18	: 64,5	63,8	39,6	7,1	31,3	5,4	25,9	22 57	21 46	24 50	9 43
April 9	59,0	58,4	40,9	12,4	29,9	5,6	24,3	21 47	20 36	1 15 19	1 0 12
June 3	58,5	57,9	43,8	17,3	31,4	5,9	25,5	18 52	17 41	3 9 4	2 23 57
4	57,5	56,9	43,9	17,3	30,3	5,9	24,4	18 49	17 38	10 1	24 54
5	57,4	56,8	43,9	17,3	30,2	5,9	24,3	18 46	17 35	10 58	25 51
6	57,0	56,4	44,0	17,2	29,6	5,9	23,7	18 43	17 32	11 56	26 49
7	57,7	57,1	44,0	17,2	30,3	5,9	24,4	18 39	17 28	12 53	27 46
8	: 57,0	56,4	44,1	17,1	29,4	6,0	23,4	18 36	17 25	13 50	28 43
11	59,0	58,4	44,2	16,9	31,1	6,0	25,1	18 26	17 15	16 41	3 1 34
24	59,5	58,9	45,0	+15,4	29,3	6,0	23,3	17 45	16 34	29 2	13 55
Oct. 10	94,0	93,0	50,9	−12,1	30,0	6,7	23,3	12 2	10 51	7 13 36	6 28 29
: 14	95,0	94,0	51,1	12,9	30,0	6,7	23,3	11 49	10 38	17 35	7 2 28
15	97,0	95,9	51,1	13,2	31,6	6,7	24,9	11 46	10 35	18 35	3 28
17	97,8	96,7	51,2	13,5	32,0	6,7	25,3	11 40	10 29	20 35	5 28
Nov. 23	104,5	103,4	53,4	17,4	32,6	6,9	25,7	9 42	8 31	8 27 50	8 12 43
28	105,2	104,1	53,6	17,5	33,0	6,9	26,1	9 26	8 15	9 2 54	17 47
Dec. 3	105,0	103,9	53,9	17,3	32,7	7,0	25,7	9 10	7 59	7 59	22 52
10	105,5	104,4	54,3	17,0	33,1	7,0	26,1	8 48	7 37	15 6	29 59
11	106,7	105,5	54,3	−16,9	34,3	7,0	27,3	8 45	7 34	16 7	9 1 0
1730, June 7	79,5	78,6	64,1	+17,1	31,6	7,8	23,8	9 29 19	9 28 8	3 12 39	2 27 32
11	80,0	79,1	64,3	16,9	31,7	7,9	23,8	29 7	27 56	16 27	3 1 20
19	79,5	78,6	64,8	+16,1	29,9	7,9	22,0	28 41	27 30	4 4 3	18 56
Dec. 9	128,0	126,6	74,3	−17,0	35,3	8,5	26,8	19 35	18 24	9 13 50	8 28 43
11	127,3	125,9	74,4	16,9	34,6	8,5	26,1	19 29	18 18	15 52	9 0 45
12	127,8	126,4	74,4	16,8	35,2	8,5	26,7	19 26	18 15	16 53	1 46
: 24	126,0	124,6	75,1	15,3	34,2	8,5	25,7	18 34	17 23	29 5	13 58
1732, Jan. 8	145,0	143,6	96,0	12,6	35,0	9,0	26,0	8 28 37	8 27 26	10 14 3	28 56
Dec. 30	164,5	162,7	115,6	14,2	32,9	8,4	24,5	9 39	8 28	5 41	20 34
1733, Jan. 1	164,2	162,4	115,7	−13,9	32,8	8,4	24,4	9 36	8 25	7 43	22 36
1734, June 12	158,5	156,8	144,6	+16,8	29,0	6,0	23,0	7 11 42	7 10 31	3 17 26	3 2 19
13	159,0	157,3	144,6	+16,7	29,4	6,0	23,4	11 39	10 28	18 23	3 16
Dec. 11	204,0	201,8	154,6	−16,9	30,3	4,8	25,5	2 3	0 52	9 15 54	9 0 47
22	203,5	201,3	155,1	15,6	30,6	4,7	25,9	1 28	0 17	27 5	11 58
25	203,0	200,8	155,2	15,2	30,4	−4,7	25,7	1 19	0 8	10 0 7	15 0
1738, Dec. 22	276,5	273,5	235,4	15,6	22,5	+6,5	29,0	4 14 6	4 12 55	9 27 6	11 59
23	276,0	273,0	235,5	15,5	22,0	6,5	28,5	14 3	12 52	28 7	13 0

β Cass.	Measured Dist.	Reduced Dist.	Precession.	Aberration.	Corrected Dist.	Nutation.	Mean Dist.	Argument of Nutation.	Moon's ascend. Node.	Argument of Aberration.	Sun's Longitude.
			−	−		+					
	″	″	″	″	″	″	″	S. ° ′	S. ° ′	S. ° ′	S. ° ′
1738, Dec. 28	273,8	270,8	235,7	14,7	20,4	6,5	26,9	4 13 47	4 12 36	10 3 12	9 18 5
1740, Jan. 21	291,0	287,8	257,1	− 9,5	21,2	+8,3	29,5	3 23 11	3 22 0	27 17	10 12 10
1747, Feb. 27	433,5	428,8	399,5	+ 1,4	30,7	−3,7	27,0	11 5 49	11 4 38	0 4 43	11 19 36

Capella south of 44° 15′.

+5″,42 annual precession.

8″,07 maximum of aberration.

9ˢ 15° 44′ = 12ˢ − right ascension of the star.

2ˢ 1° 15′ = longitude of the sun's place when the aberration in declination is 0.

Capella.	Measured Dist.	Reduced Dist.	Precession.	Aberration.	Corrected Dist.	Nutation.	Mean Dist.	Argument of Nutation.	Moon's ascend. Node.	Argument of Aberration.	Sun's Longitude.
			+	−		+					
	″	″	″	″	″	″	″	S. ° ′	S. ° ′	S. ° ′	S. ° ′
1727, Aug. 22	159,6	157,9	2,2	8,0	152,1	8,9	161,0	9 7 53	11 22 9	3 8 51	5 10 6
m. 25	159,3	157,6	2,3	7,9	152,0	8,9	160,9	7 44	22 0	11 45	13 0
26	159,5	157,8	2,3	7,9	152,2	8,9	161,1	7 41	21 57	12 43	13 58
Sept. 4	159,0	157,3	2,4	7,6	152,1	8,9	161,0	7 12	21 28	21 29	22 44
5	159,7	158,0	2,4	7,5	152,9	8,9	161,8	7 9	21 25	22 27	23 42
7	160,2	158,5	2,5	7,5	153,5	8,9	162,4	7 3	21 19	24 24	25 39
8	159,3	157,6	2,5	7,4	152,7	8,9	161,6	6 59	21 15	25 23	26 38
9	160,0	158,3	2,5	7,3	153,5	8,9	162,4	6 56	21 12	26 21	27 36
10	160,1	158 4	2,5	7,2	153,7	8,9	162,6	6 53	21 9	27 20	28 35
m. : 14	158,1	156,4	2,6	7,0	152,0	8,9	160,9	6 40	20 56	4 1 15	6 2 30
n. : 23	158,3	156,6	2,7	6,2	153,1	8,9	162,0	6 12	20 28	10 6	11 21
27	158,3	156,6	2,8	5,8	153,6	9,0	162,6	5 59	20 15	14 2	15 17
Oct. : 13	155,0	153,3	3,0	4,0	152,3	9,0	161,3	5 8	19 24	29 56	7 1 11
: 20	154,6	152,9	3,1	− 3,2	152,8	9,0	161,8	4 46	19 2	5 6 54	8 9
Nov. : 14	152,3	150,6	3,5	+ 0,3	154,4	9,0	163,4	3 27	17 43	6 2 3	8 3 18
+ 16	153,9	152,2	3,5	0,6	156,3	9,0	165,3	3 20	17 36	4 5	5 20
Dec. 5	148,9	147,3	3,8	3,2	154,3	9,0	163,3	2 20	16 36	23 20	24 35
9	149,8	148,2	3,8	3,7	155,7	9,0	164,7	2 7	16 23	27 24	28 39
12	149,8	148,2	3,9	4,1	156,2	9,0	165,2	1 58	16 14	7 0 26	9 1 41
13	147,5	145,9	3,9	4,2	154,0	9,0	163,0	1 55	16 11	1 27	2 42
21	148,4	146,8	4,0	5,1	155,9	9,0	164,9	1 29	15 45	9 36	10 51
1728, Jan. 12	145,3	143,7	4,4	7,1	155,2	9,0	164,2	0 19	14 35	8 1 56	10 3 11
16	144,3	142,7	4,4	7,4	154,5	9,0	163,5	0 6	14 22	5 59	7 14
24	143,2	141,6	4,5	7,8	153,9	9,0	162,9	8 29 41	13 57	14 4	15 19
26	143,0	141,4	4,6	7,8	153,8	9,0	162,8	29 34	13 50	16 6	17 21
27	143,6	142,0	4,6	7,9	154,5	9,0	163,5	29 27	13 47	17 6	18 21
Feb. 2	142,4	140,9	4,7	8,0	153,6	9,0	192,6	29 12	13 28	23 9	24 24

Capella.	Measured Dist.	Reduced Dist.	Precession.	Aberration.	Corrected Dist.	Nutation.	Mean Dist.	Argument of Nutation.	Moon's ascend. Node.	Argument of Aberration.	Sun's Longitude.
			+	+		+					
	″	″	″	″	″	″	″	S. ° ′	S. ° ′	S. ° ′	S. ° ′
1728, Feb. 3	142,7	141,2	4,7	8,0	153,9	9,0	162,9	8 29 9	11 13 25	8 24 10	10 25 25
: : 4	142,0	140,5	4,7	8,0	153,2	9,0	162,2	29 6	13 22	25 10	26 25
5	143,0	141,4	4,7	8,0	154,1	9,0	163,1	29 3	13 19	26 10	27 25
14	142,5	141,0	4,8	8,0	153,8	9,0	162,8	28 34	12 50	9 5 11	11 6 26
18	141,6	140,1	4,9	8,0	153,0	9,0	162,0	28 21	12 37	9 12	10 27
21	141,7	140,2	5,0	7,9	153,1	9,0	162,1	28 12	12 28	12 12	13 27
March 2	143,2	141,6	5,1	7,5	154,2	9,0	163,2	27 43	11 59	22 9	23 24
6	143,5	141,9	5,2	7,2	154,3	9,0	163,3	27 30	11 46	26 7	27 22
7	144,0	142,4	5,2	7,2	154,8	9,0	163,8	27 27	11 43	27 6	28 21
8	143,9	142,3	5,2	7,1	154,6	9,0	163,6	27 24	11 40	28 5	29 20
12	143,3	141,7	5,3	6,9	153,9	9,0	162,9	27 11	11 27	10 2 2	0 3 17
18	143,0	141,4	5,3	6,4	153,1	9,0	162,1	26 52	11 8	7 56	9 11
19	144,8	143,2	5,3	6,3	154,8	9,0	163,8	26 49	11 5	8 55	10 10
21	144,4	142,8	5,3	6,1	154,2	9,0	163,2	26 43	10 59	10 53	12 8
22	144,7	143,1	5,4	6,0	154,5	9,0	163,5	26 40	10 56	11 52	13 7
April 16	147,7	146,1	5,8	3,3	155,2	9,0	164,2	25 10	9 36	11 6 13	1 7 28
23	148,1	146,5	5,8	2,4	154,7	9,0	163,7	24 58	9 14	12 58	14 13
28	149,0	147,4	5,9	1,7	155,0	9,0	164,0	24 42	8 58	17 47	19 2
May 7	150,8	149,2	6,0	0,5	155,7	9,0	164,7	24 13	8 29	26 25	27 40
: : 8	149,5	147,9	6,1	0,4	154,4	9,0	163,4	24 10	8 26	27 23	28 38
: 10	152,0	150,3	6,1	+ 0,1	156,5	9,0	165,5	24 4	8 20	29 19	2 0 34
June 5	154,0	152,3	6,5	− 3,3	155,5	8,9	164,4	22 41	6 57	0 25 3	26 18
7	155,2	153,5	6,6	3,5	156,6	8,9	165,5	22 35	6 51	26 57	28 12
12	156,0	154,3	6,6	4,1	156,8	8,9	165,7	22 19	6 35	1 1 42	3 2 57
17	156,0	154,3	6,7	4,7	156,3	8,9	165,2	22 3	6 19	6 27	7 42
22	156,2	154,5	6,7	5,2	156,0	8,9	164,9	21 47	6 3	11 12	12 27
: 23	156,8	155,1	6,8	5,3	156,6	8,9	165,5	21 44	6 0	12 9	13 24
: 24	155,0	153,3	6,8	5,4	154,7	8,9	163,6	21 41	5 57	13 6	14 21
: 25	156,5	154,8	6,8	5,5	156,1	8,9	165,0	21 35	5 54	14 3	15 18
July 4	155,0	153,3	7,0	6,3	154,0	8,9	162,9	21 6	5 25	22 37	23 52
: 5	155,5	153,8	7,0	6,4	154,4	8,9	163,3	21 3	5 22	23 34	24 49
6	156,0	154,3	7,0	6,5	154,8	8,9	163,7	21 0	5 19	24 31	25 46
12	155,5	153,8	7,1	6,9	154,0	8,9	162,9	20 41	5 0	2 0 14	4 1 29
13	156,0	154,3	7,1	7,0	154,4	8,9	163,3	20 38	4 57	1 11	2 26
19	156,7	155,0	7,1	7,4	154,7	8,9	163,6	20 19	4 38	6 54	8 9
20	155,5	153,8	7,2	7,4	153,6	8,9	162,5	20 15	4 34	7 52	9 7
27	155,5	153,8	7,3	7,8	153,3	8,9	162,2	19 53	4 12	14 33	15 48
29	155,8	154,1	7,3	7,8	153,6	8,9	162,5	19 47	4 6	16 28	17 43
Aug. 3	154,5	152,8	7,4	8,0	152,2	8,9	161,1	19 31	3 50	21 16	22 31
: 7	153,0	151,3	7,5	8,0	150,8	8,9	159,7	19 18	3 37	25 7	26 22
: 8	153,5	151,8	7,5	8,1	151,2	8,8	160,0	19 15	3 34	26 4	27 19
13	151,3	149,7	7,6	8,1	149,2	8,8	158,0	18 59	3 18	3 0 53	5 2 8
19	151,3	149,7	7,6	8,0	149,3	8,8	158,1	18 40	2 59	6 40	7 55
m. : 22	153,7	152,0	7,7	8,0	151,7	8,8	160,5	18 31	2 50	9 35	10 50
26	153,5	151,8	7,8	7,9	151,7	8,8	160,5	18 18	2 37	13 27	14 42
27	153,5	151,8	7,8	7,9	151,7	8,8	160,5	18 15	2 34	14 25	15 40
31	153,5	151,8	7,8	7,7	151,9	8,8	160,7	18 2	2 21	18 17	19 32
Sept. : : 12	150,7	149,1	8,0	7,1	150,0	8,7	158,7	17 24	1 43	4 0 2	6 1 17

Capella.	Measured Dist.	Reduced Dist.	Precession.	Aberration.	Corrected Dist.	Nutation.	Mean Dist.	Argument of Nutation.	Moon's ascend. Node.	Argument of Aberration.	Sun's Longitude.
			+	−		+					
	″	″	″	″	″	″	″	S. ° ′	S. ° ′	S. ° ′	S. ° ′
1728, Sept. :: 13	149,7	148,1	8,0	7,0	149,1	8,7	157,8	8 17 21	11 1 40	4 1 1	6 2 16
:: 16	147,5	145,9	8,1	6,8	147,2	8,7	155,9	17 11	1 30	3 58	5 13
: 18	150,5	148,9	8,1	6,6	150,4	8,7	159,1	17 5	1 24	5 55	7 10
: 22	152,0	150,3	8,1	6,3	152,1	8,7	160,8	16 52	1 11	9 51	11 6
26	149,0	147,4	8,2	5,9	149,7	8,7	158,4	16 39	0 58	13 48	15 3
30	148,7	147,1	8,3	5,4	150,0	8,6	158,6	16 27	0 46	17 46	19 1
Oct. 1	149,5	147,9	8,3	− 5,3	150,9	8,6	159,5	16 23	0 42	18 45	20 0
Nov. 16	146,5	144,9	9,0	+ 0,7	154,6	8,6	163,2	13 57	10 28 16	6 4 50	8 6 5
19	146,0	144,4	9,0	1,1	154,5	8,6	163,1	13 48	28 7	7 52	9 7
+0″,1 Dec. 1	144,5	142,9	9,2	2,8	155,0	8,6	163,6	13 10	27 29	20 3	21 18
9	142,3	140,7	9,3	3,8	153,9	8,6	162,5	12 44	27 3	28 10	29 25
: 13	142,0	140,4	9,4	4,3	154,2	8,6	162,8	12 31	26 50	7 2 15	9 3 30
16	141,0	139,4	9,4	4,6	153,5	8,6	162,1	12 22	26 41	5 17	6 32
24	141,0	139,4	9,5	5,6	154,5	8,6	163,1	11 57	26 16	13 25	14 40
1729, Jan. 19	138,4	136,8	9,9	7,6	154,4	8,5	162,9	10 34	24 50	8 9 48	10 11 3
25	138,0	136,4	10,0	7,8	154,3	8,5	162,8	10 14	24 30	15 51	17 6
Feb. 1	137,0	135,4	10,1	8,0	153,6	8,4	162,0	9 52	24 8	22 55	24 10
6	137,2	135,6	10,2	8,1	154,0	8,4	162,4	9 36	23 52	27 56	29 11
7	137,8	136,2	10,2	8,1	154,6	8,4	163,0	9 33	23 49	28 57	11 0 12
12	137,0	135,4	10,3	8,0	153,8	8,4	162,1	9 17	23 33	9 3 57	5 12
14	137,7	136,1	10,3	8,0	154,5	8,4	162,9	9 11	23 27	5 58	7 13
15	136,7	135,1	10,3	8,0	153,5	8,4	161,9	9 8	23 24	6 58	8 13
24	136,3	134,7	10,5	7,9	153,2	8,4	161,6	8 39	22 55	15 56	17 11
25	137,3	135,7	10,5	7,8	154,1	8,4	162,5	8 36	22 52	16 56	18 11
26	137,5	135,9	10,5	7,7	154,2	8,4	162,6	8 33	22 49	17 55	19 10
27	137,3	135,7	10,5	7,7	154,2	8,4	162,6	8 30	22 46	18 55	20 10
28	138,0	136,4	10,6	7,6	154,7	8,4	163,1	8 27	22 43	19 54	21 9
March 9	137,7	136,1	10,7	7,1	154,0	8,3	162,3	7 58	22 14	28 50	0 0 5
10	138,0	136,4	10,7	7,0	154,2	8,3	162,5	7 55	22 11	29 49	1 4
:: 17	137,5	135,9	10,8	6,5	153,3	8,3	161,6	7 33	21 49	10 6 43	7 58
+0″,1 25	139,0	137,4	10,9	5,8	154,2	8,3	162,5	7 7	21 23	14 35	15 50
April 18	143,0	141,4	11,3	+ 3,0	155,7	8,2	163,9	5 51	20 7	11 7 55	1 9 10
May 14	146,0	144,4	11,6	− 0,4	155,6	8,1	163,7	4 28	18 44	0 2 54	2 4 9
26	148,4	146,8	11,8	2,0	156,6	8,1	164,7	3 50	18 6	14 21	15 36
: 31	149,0	147,4	11,9	2,7	156,6	8,0	164,6	3 34	17 50	20 3	21 18
June :: 3	148,7	147,1	12,0	3,0	156,1	8,0	164,1	3 25	17 41	22 55	24 10
:: 4	149,2	147,6	12,0	3,1	156,5	8,0	164,5	3 22	17 38	23 52	25 7
6	149,5	147,9	12,0	3,4	156,5	8,0	164,5	3 15	17 31	25 46	27 1
:: 7	150,0	148,4	12,1	3,5	157,0	8,0	165,0	3 12	17 28	26 43	27 58
20	150,5	148,9	12,3	5,0	156,2	8,0	164,2	2 31	16 47	1 9 4	3 10 19
23	150,2	148,6	12,3	5,3	155,6	8,0	163,6	2 21	16 37	11 55	13 10
24	150,0	148,4	12,3	5,4	155,3	8,0	163,3	2 18	16 34	12 52	14 7
26	151,0	149,4	12,3	5,6	156,1	8,0	164,1	2 12	16 28	14 47	16 2
q. 29	148,5	146,9	12,4	5,9	153,4	7,9	161,3	2 2	16 18	17 37	18 52
30	152,0	150,3	12,4	6,0	156,7	7,9	164,6	1 59	16 15	18 34	19 49
July 1	151,0	149,4	12,4	6,1	155,7	7,9	163,6	1 56	16 12	19 32	20 47
12	153,5	151,8	12,6	6,9	157,5	7,9	165,4	1 21	15 37	29 59	4 1 14

Capella.	Measured Dist.	Reduced Dist.	Precession.	Aberration.	Corrected Dist.	Nutation.	Mean Dist.	Argument of Nutation.	Moon's ascend. Node.	Argument of Aberration.	Sun's Longitude.
			+	−		+					
	″	″	″	″	″	″	″	s. ° ′	s. ° ′	s. ° ′	s. ° ′
1729, July 14	153,5	151,8	12,6	7,1	157,3	7,9	165,2	8 1 14	10 15 30	2 1 54	4 3 9
21	152,7	151,0	12,7	7,5	156,2	7,9	164,1	0 52	15 8	8 35	9 50
Aug. 7	152,5	150,8	13,0	8,0	155,8	7,8	163,6	7 29 58	14 14	24 53	26 8
11	150,8	149,2	13,0	8,1	154,1	7,8	161,9	29 45	14 1	28 43	29 58
: : 13	150,5	148,9	13,0	8,1	153,8	7,8	161,6	29 39	13 55	3 0 39	5 1 54
20	152,0	150,4	13,2	8,0	155,6	7,7	163,3	29 17	13 33	7 24	8 39
22	150,5	148,9	13,2	8,0	154,1	7,7	161,8	29 11	13 27	9 21	10 36
24	152,3	150,6	13,2	7,9	155,9	7,7	163,6	29 4	13 20	11 17	12 32
31	149,5	147,9	13,3	7,7	153,5	7,7	161,2	28 42	12 58	18 4	19 19
Sept. 1	147,5	145,9	13,3	7,7	151,5	7,7	159,2	28 39	12 55	19 3	20 18
2	149,2	147,6	13,3	7,6	153,3	7,7	161,0	28 36	12 52	20 1	21 16
: 3	148,5	146,9	13,4	7,6	152,7	7,7	160,4	28 32	12 48	21 0	22 15
9	149,7	148,1	13,5	7,3	154,3	7,7	162,0	28 13	12 29	26 51	28 6
19	149,0	147,4	13,6	6,6	154,4	7,6	162,0	27 42	11 58	4 6 40	6 7 55
: : 29	143,7	142,1	13,8	5,6	150,3	7,6	157,9	27 10	11 26	16 31	17 46
Oct. 8	146,5	144,9	13,9	4,6	154,2	7,5	161,7	26 41	10 57	25 27	26 42
20	146,0	144,4	14,1	− 3,1	155,4	7,5	162,9	26 3	10 19	5 7 25	7 8 40
Nov. 25	142,5	141,0	14,6	+ 1,9	157,5	7,3	164,8	24 9	8 25	6 13 43	8 14 58
Dec. 2	140,2	138,7	14,7	2,9	156,3	7,3	163,6	23 46	8 2	20 49	22 4
10	138,7	137,2	14,8	3,9	155,9	7,2	163,1	23 21	7 37	28 57	9 0 12
1730, Jan. 31	135,3	133,8	15,6	8,0	157,4	7,0	164,4	20 36	4 52	8 21 39	10 22 54
Feb. 21	133,5	132,0	16,0	7,9	155,9	6,9	162,8	19 33	3 49	9 12 42	11 13 57
24	134,0	132,5	16,0	7,8	156,3	6,8	163,1	19 20	3 36	15 42	16 57
March 3	134,2	132,7	16,1	+ 7,5	156,3	6,8	163,1	18 58	3 14	22 39	23 54
May 25	142,3	140,8	17,3	− 1,8	156,3	6,3	162,6	14 34	9 28 50	0 13 9	2 14 24
27	144,7	143,1	17,3	2,1	158,3	6,3	164,6	14 28	28 44	16 1	17 16
June 6	146,0	144,4	17,5	3,5	158,4	6,2	164,6	13 56	28 12	25 32	26 47
19	148,0	146,4	17,7	5,0	158,1	6,2	164,3	13 15	27 31	1 7 53	3 9 8
July 14	149,3	147,7	18,1	7,0	158,8	6,0	164,8	11 55	26 11	2 1 41	4 2 56
15	148,5	146,9	18,1	7,1	157,9	6,0	163,9	11 52	26 8	2 38	3 53
17	150,2	148,6	18,1	7,3	159,4	6,0	165,4	11 46	26 2	4 32	5 47
25	149,0	147,4	18,2	7,7	157,9	5,9	163,8	11 20	25 36	12 11	13 26
Aug. 5	149,0	147,4	18,4	8,0	157,8	5,9	163,7	10 46	25 2	22 43	23 58
6	148,5	146,9	18,4	8,0	157,3	5,9	163,2	10 42	24 58	23 41	24 56
: 9	148,5	146,9	18,4	8,1	157,2	5,9	163,1	10 33	24 49	26 34	27 49
: 12	147,5	145,9	18,5	8,1	156,3	5,8	162,1	10 23	24 39	29 27	5 0 42
13	149,0	147,4	18,5	8,1	157,8	5,8	163,6	10 20	24 36	3 0 25	1 40
16	148,0	146,4	18,5	8,1	156,8	5,8	162,6	10 11	24 27	3 18	4 33
18	147,0	145,4	18,6	8,0	156,0	5,8	161,8	10 4	24 20	5 14	6 29
20	146,5	144,9	18,6	8,0	155,5	5,8	161,3	9 58	24 14	7 10	8 25
24	147,3	145,7	18,7	8,0	156,4	5,7	162,1	9 45	24 1	11 3	12 18
: 25	145,5	143,9	18,7	7,9	154,7	5,7	160,4	9 42	23 58	12 1	13 16
26	146,5	144,9	18,7	7,9	155,7	5,7	161,4	9 39	23 55	12 59	14 14
29	146,5	144,9	18,7	7,8	155,8	5,7	161,5	9 29	23 45	15 54	17 9
Sept. : 3	144,7	143,1	18,8	7,6	154,3	5,7	160,0	9 13	23 29	20 47	22 2
8	147,7	146,1	18,9	− 7,3	157,7	5,7	163,4	8 58	23 13	25 39	26 54
Dec. 24	135,5	134,0	20,5	+ 5,5	160,0	5,0	165,0	3 18	17 34	7 12 54	9 14 9

Capella.	Measured Dist.	Reduced Dist.	Precession.	Aberration.	Corrected Dist.	Nutation.	Mean Dist.	Argument of Nutation.	Moon's ascend. Node.	Argument of Aberration.	Sun's Longitude.
			+	+		+					
	″	″	″	″	″	″	″	S. ° ′	S. ° ′	S. ° ′	S. ° ′
1730, Dec. 27	135,3	133,8	20,5	5,8	160,1	4,9	165,0	7 3 8	9 17 24	7 15 58	9 17 13
1731, Jan. 3	134,5	133,0	20,6	6,5	160,1	4,9	165,0	2 45	17 1	23 5	24 20
18	133,7	132,2	20,7	7,5	160,4	4,8	165,2	1 58	16 14	8 8 18	10 9 33
: : 24	133,5	132,0	20,8	7,8	160,6	4,7	165,3	1 39	15 55	14 21	15 36
Feb. : : : 3	132,0	130,6	21,1	8,0	159,7	4,6	164,3	1 7	15 23	24 26	25 41
5	134,0	132,5	21,1	8,1	161,7	4,6	166,3	1 1	15 17	26 26	27 41
6	131,5	130,1	21,1	8,1	159,3	4,6	163,9	0 58	15 13	27 27	28 42
April 27	138,2	136,7	22,4	+ 1,9	161,0	4,0	165,0	6 26 43	10 59	11 16 7	1 17 22
Aug. 9	146,0	144,4	23,9	− 8,0	160,3	3,3	163,6	21 13	5 29	2 26 20	4 27 35
10	146,5	144,9	23,9	8,1	160,7	3,3	164,0	21 10	5 26	27 18	28 33
17	146,5	144,9	24,0	8,1	160,8	3,2	164,0	20 47	5 3	3 4 2	5 5 17
19	146,0	144,4	24,0	8,0	160,4	3,2	163,6	20 41	4 57	5 58	7 13
20	145,5	143,9	24,1	8,0	160,0	3,2	163,2	20 38	4 54	6 56	8 11
21	145,7	144,1	24,1	8,0	160,2	3,2	163,4	20 35	4 51	7 54	9 9
Sept. 9	146,5	144,9	24,4	7,3	162,0	3,0	165,0	19 34	3 50	26 22	27 37
: : 10	144,5	142,9	24,4	7,3	160,0	3,0	163,0	19 31	3 47	27 22	28 37
: : 12	145,3	143,7	24,4	7,1	161,0	3,0	164,0	19 25	3 41	29 19	6 0 34
er. 13	147,3	145,7	24,4	7,0	163,1	3,0	166,1	19 22	3 38	4 0 18	1 33
22	145,5	143,9	24,6	− 6,4	162,1	2,9	166,0	18 53	3 9	9 8	10 23
1732, Jan. 4	134,0	132,5	26,1	+ 6,5	165,1	2,1	167,2	13 22	8 27 38	7 23 51	9 25 6
8	134,0	132,5	26,2	6,8	165,5	2,0	167,5	13 6	27 22	27 54	29 9
April 7	133,8	132,3	27,5	4,3	164,1	1,3	165,4	8 24	22 40	10 27 31	0 28 46
17	138,0	136,5	27,7	3,1	167,3	1,2	168,5	7 52	22 8	11 7 12	1 8 27
22	136,5	135,0	27,7	2,5	165,2	+1,2	166,4	7 36	21 52	12 2	13 17
1733, Jan. 31	127,2	125,8	31,8	+ 8,0	165,6	−1,2	164,4	5 22 34	6 50	8 21 55	10 23 10
1734, June 11	139,0	137,5	39,4	− 4,1	172,8	5,0	167,8	4 26 18	7 10 34	1 0 19	3 1 34
12	139,0	137,5	39,4	4,1	172,8	5,0	167,8	26 15	10 31	1 16	2 31
July 3	139,5	138,0	39,8	6,3	171,5	5,1	166,4	25 8	9 24	21 13	22 28
11	140,5	139,0	39,9	6,8	172,2	5,2	167,0	24 42	8 58	28 50	4 0 5
16	140,0	138,5	39,9	7,2	171,2	5,2	166,0	24 27	8 43	2 3 36	4 51
Aug. 3	137,0	135,5	40,2	7,9	167,8	5,3	162,5	23 29	7 45	20 49	22 4
6	140,0	138,5	40,2	8,0	170,7	5,4	165,3	23 20	7 36	23 41	24 56
7	139,2	137,7	40,2	− 8,0	169,9	5,4	164,5	23 17	7 33	24 39	25 54
Dec. 22	124,0	122,6	42,6	+ 5,3	170,5	6,2	164,3	16 1	0 17	7 10 55	9 12 10
25	125,7	124,3	42,6	5,6	172,5	8,9	163,6	15 52	0 8	13 57	15 12
1737, Jan. 9	115,8	114,5	53,6	7,0	175,1	8,9	166,2	3 6 22	5 20 38	29 44	10 0 59
18	114,5	113,3	53,5	7,5	174,3	8,9	165,4	5 53	20 9	8 8 51	10 6
19	115,2	114,0	53,5	+ 7,6	175,1	9,0	166,1	5 50	20 6	9 52	11 7
1738, June 19	120,0	118,7	61,2	− 4,8	175,1	8,4	166,7	2 8 30	4 22 46	1 7 56	3 9 11
30	120,0	118,7	61,4	− 6,0	174,1	8,4	165,7	7 55	22 11	18 24	8 19 39
Dec. 30	107,2	106,0	64,1	+ 6,1	176,2	7,7	168,5	1 28 14	12 30	7 19 4	9 20 19
1739, Jan. 14	100,0	98,9	64,3	7,3	170,5	7,6	162,9	27 26	11 42	8 3 17	10 4 32
Feb. 2	102,3	101,2	64,6	8,0	173,8	7,5	166,3	26 26	10 42	23 29	24 44
3	102,0	100,9	64,6	8,0	173,5	7,5	166,0	26 23	10 39	24 29	25 44
4	101,7	100,6	64,6	8,0	173,2	7,5	165,7	26 20	10 36	25 30	26 45
6	102,2	101,1	64,7	8,1	173,9	7,5	166,4	26 14	10 30	27 30	28 45
1740, Jan. 20	96,8	95,7	69,8	7,6	173,1	5,5	167,6	7 48	3 22 4	10 8	11 23

Capella.	Measured Dist.	Reduced Dist.	Precession.	Aberration.	Corrected Dist.	Nutation.	Mean Dist.	Argument of Nutation.	Moon's ascend. Node.	Argument of Aberration.	Sun's Longitude.
			+	+		−					
	"	"	"	"	"	"	"	S. ° ′	S. ° ′	S. ° ′	S. ° ′
1740, Jan. 29	98,3	97,2	70,0	7,9	175,1	5,5	169,6	1 7 19	3 21 35	8 19 13	10 20 28
: June 2	106,0	104,8	71,8	− 3,0	173,6	4,6	169,0	0 42	14 58	0 22 17	2 23 32
:: 3	107,5	106,3	71,8	3,2	174,9	4,6	170,3	0 39	14 55	23 14	24 29
:: 4	102,3	101,2	71,8	− 3,3	169,7	4,6	165,1	0 36	14 52	24 11	25 26
1741, Jan. 26	90.7	89,7	75,3	+ 7,9	172,9	−2,8	170,1	0 18 6	2 22	8 16 58	10 18 13
1747, Feb. 27	41,0	40,5	108,0	+ 7,7	156,2	+8,9	165,1	8 20 22	11 4 38	9 18 33	11 19 48

α Persei south of 41° 5′, to July 11, 1734, inclusive, afterwards north.

+13″,825 annual precession.

11″,43 maximum of aberration.

10^{s} 13° 35′ = 12^{s} − right ascension of the star.

0^{s} 24° 42′ = longitude of the sun's place when the aberration in declination is 0.

α Pers.	Measured Dist.	Reduced Dist.	Precession.	Aberration.	Corrected Dist.	Nutation.	Mean Dist.	Argument of Nutation.	Moon's ascend. Node.	Argument of Aberration.	Sun's Longitude.
			+	−		+					
	"	"	"	"	"	"	"	S. ° ′	S. ° ′	S. ° ′	S. ° ′
1727, Aug. 22	102,0	100,9	5,6	8,0	98,5	7,3	105,8	10 5 44	11 22 9	4 15 18	5 10 0
Oct. :: 13	93,4	92,4	7,6	+ 1,3	101,3	7,5	108,8	2 59	19 24	6 6 24	7 1 6
20	91,7	90,7	7,9	2,5	101,1	7,6	108,7	2 37	19 2	13 23	8 5
Nov. 14	87,5	86,5	8,8	7,1	102,4	7,7	110,1	1 17	17 42	7 8 32	8 3 14
:: 22	86,0	85,1	9,1	8,3	102,5	7,7	110,2	0 52	17 17	16 38	11 20
Dec. 12	83,0	82,1	9,9	10,5	102,5	7,8	110,3	9 29 48	16 13	8 6 45	9 1 37
: 21	81,0	80,1	10,2	11,1	101,4	7,8	109,2	29 20	15 45	16 5	10 47
29	80,3	79,4	10,5	11,3	101,3	7,9	109,2	28 54	15 19	24 12	18 54
1728, Jan. 16	80,0	79,1	11,2	11,2	101,5	7,9	109,4	27 57	14 22	9 12 27	10 7 9
Feb. 2	79,3	78,4	11,8	10,0	100,2	8,0	108,2	27 3	13 28	29 37	24 19
3	79,7	78,8	11,9	9,8	100,5	8,0	108,5	27 0	13 25	10 0 38	25 20
: 4	79,2	78,3	11,9	9,7	100,0	8,0	108,0	26 57	13 22	1 38	26 20
: 5	80,5	79,6	12,0	9,6	101,2	8,0	109,2	26 53	13 18	2 38	27 20
16	82,0	81,1	12,4	8,1	101,6	8,1	109,7	26 18	12 43	13 40	11 8 22
: 21	81,2	80,3	12,6	7,6	100,5	8,1	108,6	26 3	12 28	18 40	13 22
March 23	86,5	85,6	13,7	+ 2,1	101,4	8,2	109,6	24 24	10 49	11 19 19	0 14 1
April 7	88,5	87,5	14,3	− 0,8	101,0	8,2	109,2	23 37	10 2	0 3 58	28 40
25	91,0	90,0	14,9	4,2	100,7	8,3	109,0	22 39	9 4	21 22	1 16 4
May 8	93,8	92,8	15,5	6,4	101,9	8,3	110,2	21 58	8 23	1 4 49	29 31
June 17	96,0	95,0	17,0	10,9	101,1	8,5	109,6	19 51	6 16	2 12 55	3 7 37
22	96,5	95,5	17,2	11,1	101,6	8,5	110,1	19 35	6 0	17 41	12 23
: 23	97,7	96,6	17,2	11,2	102,6	8,5	111,1	19 32	5 57	18 38	13 20
:: 28	95,5	94,5	17,4	11,3	100,6	8,5	109,1	19 16	5 41	23 23	18 5
July 5	95,6	94,6	17,7	11,4	100,9	8,5	109,4	18 54	5 19	3 0 2	24 44
: 19	95,5	94,5	18,2	11,2	101,5	8,5	110,0	18 9	4 34	13 23	4 8 5
20	93,0	92,0	18,2	11,1	99,1	8,6	107,7	18 6	4 31	14 21	9 3
22	94,8	93,8	18,3	11,0	101,1	8,6	109,7	18 0	4 25	16 14	10 56
m. 27	93,0	92,0	18,5	10,5	100,0	8,6	108,6	17 44	4 9	21 1	15 43

α Pers.	Measured Dist.	Reduced Dist.	Precession.	Aberration.	Corrected Dist.	Nutation.	Mean Dist.	Argument of Nutation.	Moon's ascend. Node.	Argument of Aberration.	Sun's Longitude.
			+	−		+					
	″	″	″	″	″	″	″	S. ° ′	S. ° ′	S. ° ′	S. ° ′
1728, July 29	93,0	92,0	18,6	10,5	100,1	8,6	108,7	9 17 38	11 4 3	3 22 56	4 17 38
m. Aug. :: 3	91,0	90,0	18,8	10,3	98,5	8,6	107,1	17 22	3 47	27 45	22 27
8	91,0	90,0	19,0	9,7	99,3	8,6	107,9	17 16	3 31	4 2 33	27 15
13	88,5	87,5	19,2	9,3	97,4	8,6	106,0	17 10	3 15	7 21	5 2 3
m. 19	87,0	86,1	19,4	8,4	97,1	8,6	105,7	16 31	2 56	13 9	7 51
m. Sept. :: 9	85,5	84,6	20,2	− 5,3	99,5	8,7	108,2	15 24	1 49	5 3 34	28 16
Oct. 18	76,0	75,2	21,7	+ 2,4	99,3	8,8	108,1	13 20	10 29 45	6 12 8	7 6 50
31	73,0	72,2	22,1	4,9	99,2	8,8	108,0	12 39	29 4	25 10	19 52
Nov. : 19	71,0	70,2	22,9	8,0	101,1	8,8	109,9	11 38	28 3	7 14 21	8 9 3
22	69,5	68,7	23,0	8,4	100,1	8,8	108,9	11 29	27 54	17 23	12 5
23	70,5	69,7	23,0	8,5	101,2	8,8	110,0	11 26	27 51	18 24	13 6
Dec. 1	68,7	68,0	23,3	9,5	100,8	8,9	109,7	11 0	27 25	26 31	21 13
9	67,0	66,3	23,6	10,3	100,2	8,9	109,1	10 35	27 0	8 4 39	29 21
11	66,6	65,9	23,7	10,5	100,1	8,9	109,0	10 29	26 54	6 41	9 1 23
13	66,4	65,7	23,8	10,6	100,1	8,9	109,0	10 23	26 48	8 42	3 24
21	64,5	63,8	24,1	11,1	99,0	8,9	107,9	9 57	26 22	16 50	11 32
:: 23	67,0	66,3	24,1	11,2	101,6	8,9	110,5	9 51	26 16	18 52	13 34
1729, Jan. :: 3	64,0	63,3	24,5	11,4	99,2	8,9	108,1	9 16	25 41	9 0 3	24 45
14	64,4	63,7	25,0	11,2	99,9	8,9	108,8	8 41	25 6	11 12	10 5 54
Feb. 1	64,2	63,5	25,7	10,0	99,2	8,9	108,1	7 43	24 8	29 23	24 5
25	68,0	67,3	26,6	6,8	100,7	8,9	109,6	6 27	22 52	10 23 24	11 18 6
26	68,0	67,3	26,6	+ 6,7	100,6	9,0	109,6	6 24	22 49	24 24	19 6
June 23	82,2	81,3	31,0	−11,1	101,2	9,0	110,2	0 12	16 37	2 18 24	3 13 6
24	81,5	80,6	31,0	11,1	100,5	9,0	109,5	0 9	16 34	19 21	14 3
27	81,7	80,8	31,2	11,2	100,8	9,0	109,8	0 0	16 25	22 13	16 55
29	83,0	82,1	31,2	11,3	102,0	9,0	111,0	8 29 53	16 18	24 6	18 48
30	81,7	80,8	31,3	11,3	100,8	9,0	109,8	29 50	16 15	25 3	19 45
July 1	82,0	81,1	31,3	11,4	101,0	9,0	110,0	29 47	16 12	26 0	20 42
12	81,2	80,3	31,8	11,3	100,8	9,0	109,8	29 12	15 37	3 6 28	4 1 10
Aug. 7	76,0	75,2	32,8	9,9	98,1	9,0	107,1	27 49	14 14	4 1 21	26 3
m. 15	76,8	76,0	33,0	9,0	100,0	9,0	109,0	27 24	13 49	9 3	5 3 45
:: 20	73,0	72,2	33,2	8,4	97,0	9,0	106,0	27 8	13 33	13 53	8 35
: 21	76,0	75,2	33,2	− 8,3	100,1	9,0	109,1	27 5	13 30	14 51	9 33
Nov. 28	56,0	55,4	37,0	+ 9,1	101,4	8,9	110,3	21 50	8 15	7 23 13	8 17 55
Dec. 2	55,0	54,4	37,2	9,6	101,2	8,9	110,1	21 38	8 3	27 17	21 59
3	54,0	53,4	37,2	9,7	100,3	8,9	109,2	21 35	8 0	28 18	23 0
10	52,7	52,1	37,5	10,4	100,0	8,9	108,9	21 12	7 37	8 5 25	9 0 7
1730, Jan. 31	51,5	50,9	39,5	+10,1	100,5	8,8	109,3	18 27	4 52	9 28 8	10 22 50
July 15	68,5	67,8	45,8	−11,3	102,3	8,4	110,7	9 43	9 26 8	3 8 46	4 3 28
Dec. 24	40,0	39,6	51,9	+11,2	102,7	7,9	110,6	1 8	17 33	8 19 23	9 14 5
1731, Jan. 3	39,0	38,6	52,3	11,4	102,3	7,8	110,1	0 36	17 1	29 34	24 15
18	39,5	39,1	52,8	11,1	103,0	7,8	110,8	7 29 49	16 14	9 14 45	10 9 27
Dec. 26	28,0	27,7	65,8	11,3	104,8	6,0	110,8	11 42	8 28 7	8 21 13	9 15 53
1732, Jan. 8	26,8	26,5	66,3	+11,4	104,2	+5,9	110,1	11 1	27 26	9 4 22	29 4
1734, June 12	22,0	21,8	99,9	−10,5	111,2	−0,9	110,3	5 24 6	7 10 31	2 7 44	3 2 26
July 11	21,0	20,8	101,0	−11,4	110,4	1,2	109,2	22 33	8 58	3 5 21	4 0 3
1738, Dec. 23	56,5	55,9	162,6	+11,2	117,9	9,0	108,9	2 26 27	4 12 52	8 18 11	9 12 53

α Pers.	Measured Dist.	Reduced Dist.	Precession.	Aberration.	Corrected Dist.	Nutation.	Mean Dist.	Argument of Nutation.	Moon's ascend. Node.	Argument of Aberration.	Sun's Longitude.
			+	+		−					
	"	"	"	"	"	"	"	S. ° ′	S. ° ′	S. ° ′	S. ° ′
1738, Dec. 24	57,3	56,7	162,6	11,2	117,1	9,0	108,1	2 26 24	4 12 49	8 19 13	9 13 54
1740, Jan. 20	73,0	72,2	177,4	11,0	116,2	8,2	108,0	5 39	3 22 4	9 16 36	10 11 18
22	72,3	71,5	177,5	10,8	116,8	−8,2	108,6	5 32	21 57	18 38	13 20
1747, Feb. 27	184,5	182,5	275,4	+ 6,5	99,4	+8,5	108,0	9 18 13	11 4 38	10 25 1	11 19 43

τ Persei south of 38° 20′, to Jan. 31, 1730, inclusive, and afterwards north.

+15″,625 annual precession.

12″,87 maximum of aberration.

10ˢ 21° 4′=12ˢ−right ascension of the star.

0ˢ 18° 35′=longitude of the sun's place when the aberration in declination is 0.

τ Pers.	Measured Dist.	Reduced Dist.	Precession.	Aberration.	Corrected Dist.	Nutation.	Mean Dist.	Argument of Nutation.	Moon's ascend. Node.	Argument of Aberration.	Sun's Longitude.
			+	−		+					
	"	"	"	"	"	"	"	S. ° ′	S. ° ′	S. ° ′	S. ° ′
1727, Aug. 22	66,0	65,3	6,3	8,0	63,6	6,6	70,2	10 13 0	11 21 56	4 21 26	5 10 1
Sept. : 11	62,8	62,1	7,2	4,2	65,1	6,7	71,8	12 10	21 6	5 10 53	29 28
16	60,8	60,1	7,4	− 3,2	64,3	6,7	71,0	11 54	20 50	15 47	6 4 22
Oct. 13	53,9	53,3	8,5	+ 2,8	64,6	6,8	71,4	10 28	19 24	6 12 30	7 1 5
20	53,6	53,0	8,9	4,3	66,2	6,9	73,1	10 6	19 2	19 29	8 4
Nov. 14	45,8	45,3	9,9	9,0	64,2	7,0	71,2	8 46	17 42	7 14 36	8 3 11
: : 22	44,0	43,5	10,2	10,2	63,9	7,1	71,0	8 21	17 17	22 42	11 17
Dec. 12	42,0	41,5	11,1	12,3	64,9	7,2	72,1	7 17	16 13	8 13 11	9 1 46
21	41,4	41,0	11,5	12,6	65,1	7,2	72,6	6 49	15 45	22 10	10 45
29	40,1	39,7	11,8	12,9	64,4	7,2	71,6	6 23	15 19	9 0 18	18 53
1728, Jan. : 3	41,6	41,2	12,1	12,8	66,1	7,3	73,4	6 7	15 3	5 22	23 57
16	40,2	39,8	12,6	12,2	64,6	7,3	71,9	5 26	14 22	18 33	10 7 8
: : 20	39,2	38,8	12,8	11,9	63,5	7,3	70,8	5 13	14 9	22 37	11 12
Feb. 2	41,0	40,6	13,4	10,5	64,5	7,4	71,9	4 32	13 28	10 5 45	24 20
3	42,0	41,5	13,4	10,3	65,2	7,4	72,6	4 29	13 25	6 46	25 21
: 4	39,8	39,4	13,4	10,2	63,0	7,4	70,4	4 26	13 22	7 46	26 21
: 5	42,4	42,0	13,5	+10,0	65,5	7,4	72,9	4 22	13 18	8 46	27 21
manè. July 29 :	55,7	55,1	20,9	−11,4	64,6	8,1	72,7	9 25 6	4 2	3 29 2	4 17 37
m. Aug. 3 :	52,0	51,4	21,1	10,8	61,7	8,2	69,9	24 50	3 46	4 3 51	22 26
: 7	54,5	53,9	21,3	10,3	64,9	8,2	73,1	24 38	3 34	7 41	26 16
: 8	54,5	53,9	21,4	10,2	65,1	8,2	73,3	24 35	3 31	8 38	27 13
13	52,7	52,1	21,6	9,5	64,2	8,2	72,4	24 19	3 15	13 27	5 2 2
m. : 19	52,0	51,4	21,8	8,6	64,6	8,2	72,8	24 0	2 56	19 15	7 50
m. Sept. : : 9	46,5	46,0	22,7	− 4,7	64,0	8,3	72,3	22 53	1 49	5 9 40	28 15
Oct. 18	35,0	34,6	24,4	+ 4,0	63,0	8,4	71,4	20 46	10 29 42	6 18 14	7 6 49
31	32,5	32,1	24,9	6,7	63,7	8,4	72,1	20 8	29 4	7 1 16	19 51
Nov. 19	28,0	27,7	25,8	9,9	63,4	8,5	71,9	19 7	28 3	20 26	8 9 1
22	27,5	27,2	25,9	10,4	63,5	8,5	72,0	18 58	27 54	23 29	12 4

τ Pers.	Measured Dist.	Reduced Dist.	Precession.	Aberration.	Corrected Dist.	Nutation.	Mean Dist.	Argument of Nutation.	Moon's ascend. Node.	Argument of Aberration.	Sun's Longitude.
			+	+		+					
	″	″	″	″	″	″	″	S. ° ′	S. ° ′	S. ° ′	S. ° ′
1728, Nov. 23	27,5	27,2	25,9	10,5	63,6	8,5	72,1	9 18 55	10 27 51	7 24 30	8 13 5
Dec. 1	26,5	26,2	26,2	11,4	63,8	8,5	72,3	18 29	27 25	8 2 37	21 12
::: 11	23,5	23,2	26,7	12,3	62,2	8,6	70,8	17 58	26 54	12 47	9 1 22
21	22,5	22,3	27,1	12,8	62,2	8,6	70,8	17 26	26 22	22 57	11 32
1729, Jan. : 3	21,6	21,4	27,7	12,8	61,9	8,6	70,5	16 45	25 41	9 6 8	24 43
: 14	22,5	22,3	28,1	12,2	62,6	8,6	71,2	16 10	25 6	17 18	10 5 53
25	22,5	22,3	28,6	11,3	62,2	8,7	70,9	15 35	24 31	28 25	17 0
Feb. 1	23,8	23,5	28,9	+10,5	62,9	8,7	71,6	15 12	24 8	10 5 31	24 6
m. Aug. 15	35,5	35,1	37,3	− 9,2	63,2	9,0	72,2	4 53	13 49	4 15 9	5 3 44
::: 19	36,0	35,6	37,5	8,6	64,5	9,0	73,5	4 40	13 36	19 1	7 36
20	34,0	33,6	37,5	8,4	62,7	9,0	71,7	4 37	13 33	19 59	8 34
m. 21	36,0	35,6	37,5	− 8,3	64,8	9,0	73,8	4 34	13 30	20 57	9 32
Nov. 28	10,0	10,0	41,8	+11,1	62,9	9,0	71,9	8 29 19	8 15	7 29 20	8 17 55
Dec. 2	9,2	9,2	41,9	11,5	62,6	9,0	71,6	29 7	8 3	8 3 22	21 57
: 3	8,7	8,7	41,9	11,6	62,2	9,0	71.2	29 4	8 0	4 23	22 58
10	9,0	9,0	42,3	12,2	63,5	9,0	72,5	28 41	7 37	11 30	9 0 5
1730, Jan. 31	8,0	+8,0	44,5	10,6	63,1	9,0	72,1	25 56	4 52	10 4 15	10 22 50
Dec. 24	7,0	−7,0	58,5	12,8	64,3	8,4	72,7	8 37	9 17 33	8 25 29	9 14 4
1731, Jan. 3	8,2	8,2	59,0	12,8	63,6	8,4	72,0	8 8	17 4	9 5 37	24 12
18	8,2	8,2	59,6	12,1	63,5	8,3	71,8	7 17	16 13	20 50	10 9 25
Dec. 26	22,5	22,3	74,2	12,9	64,8	6,8	71,6	7 19 11	8 28 7	8 26 17	9 15 52
1732, Jan. 8	22,3	22,0	74,7	12,7	65,4	6,7	72,1	18 30	27 26	9 10 28	29 3
9	21,5	21,3	74,8	12,6	66,1	6,7	72,8	18 27	27 23	11 29	10 0 4
1733, Jan. 21	35,0	34,6	90,9	11,7	68,0	+4,3	72,3	6 28 25	7 21	24 24	12 59
1738, Dec. 22	119,7	118,4	183,3	12,7	77,6	−9,0	68,6	3 3 59	4 12 55	8 23 31	9 12 6
23	118,5	117,2	183,3	12,8	78,9	9,0	69,9	3 56	12 52	24 32	13 7
24	118,0	116,7	183,4	12,9	79,6	9,0	70,6	3 53	12 49	25 33	14 8
28	117,2	115,9	183,6	12,9	80,6	9,0	71,6	3 40	12 36	29 37	18 12
1740, Jan. 22	134,0	132,5	200,2	11,7	79,4	8,6	70,8	2 13 1	3 21 57	9 24 43	10 13 18

35 Camelopard. south of 38° 25′.

+ 1″,476 annual precession.

9″,44 maximum of aberration.

9^s 4° 5′ = 12^s − right ascension of the star.

2^s 23° 12′ = longitude of the sun's place when the aberration in declination is 0.

35 Camelop.	Measured Dist.	Reduced Dist.	Precession.	Aberration.	Corrected Dist.	Nutation.	Mean Dist.	Argument of Nutation.	Moon's ascend. Node.	Argument of Aberration.	Sun's Longitude.
			+	−		+					
	″	″	″	″	″	″	″	S. ° ′	S. ° ′	S. ° ′	S. ° ′
m. 1727, Sept. 14	76,3	75,5	0,7	9,3	66,9	9,0	75,0	8 25 1	11 20 56	3 9 20	6 2 32
n. :: 23	75,3	74,5	0,8	9,0	66,3	8,9	75,2	24 33	20 28	18 11	11 23
:: 27	77,4	76,6	0,8	8,7	68,7	8,9	77,6	24 20	20 15	22 7	15 19
Oct. 13	74,3	73,5	0,8	7,4	66,9	8,9	75,8	23 29	19 24	4 8 1	7 1 13

35 Camelop.	Measured Dist.	Reduced Dist.	Precession.	Aberration.	Corrected Dist.	Nutation.	Mean Dist.	Argument of Nutation.	Moon's ascend. Node.	Argument of Aberration.	Sun's Longitude.
			+	−		+					
	"	"	"	"	"	"	"	S. ° ′	S. ° ′	S. ° ′	S. ° ′
1727, Oct. 20	74,4	73,6	0,8	6,7	67,7	8,9	76,6	8 23 7	11 19 2	4 15 0	7 8 12
Nov. 22	69,8	69,0	1,0	− 1,9	68,1	8,9	77,0	21 22	17 17	5 18 14	8 11 26
Dec. 12	67,2	66,5	1,1	+ 1,4	69,0	8,9	77,9	20 19	16 14	6 8 31	9 1 43
21	65,1	64,4	1,1	2,9	68,4	8,9	77,3	19 50	15 45	17 41	10 53
1728, Jan. 12	61,5	60,8	1,2	6,1	68,1	8,8	76,9	18 40	14 35	7 10 1	10 3 13
16	60,5	59,8	1,2	6,6	67,6	8,8	76,4	18 27	14 22	14 4	7 16
: 24	58,0	57,4	1,2	7,5	66,1	8,8	74,9	18 2	13 57	22 9	15 21
27	58,7	58,1	1,3	7,8	67,2	8,8	76,0	17 52	13 47	25 11	18 23
Feb. 2	59,3	58,7	1,3	8,3	68,3	8,8	77,1	17 33	13 28	8 1 14	24 26
3	58,7	58,1	1,3	8,4	67,8	8,8	76,6	17 30	13 25	2 15	25 27
4	57,0	56,4	1,3	8,4	66,1	8,8	74,9	17 27	13 22	3 15	26 27
5	59,8	59,2	1,3	8,5	69,0	8,8	77,8	17 24	13 19	4 15	27 27
18	57,2	56,6	1,4	9,2	67,2	8,8	76,0	16 42	12 37	17 17	11 10 29
21	57,2	56,6	1,4	9,3	67,3	8,8	76,1	16 33	12 28	20 17	13 29
March 1	58,4	57,8	1,4	9,4	68,6	8,7	77,3	16 4	11 59	29 14	22 26
2	58,5	57,9	1,4	+ 9,4	68,7	8,7	77,4	16 1	11 56	9 0 13	23 25
m. Sept. 13	75,8	75,0	2,2	− 9,4	67,8	8,2	76,0	5 41	1 36	3 9 6	6 2 18
: : 18	76,0	75,2	2,2	9,2	68,2	8,2	76,4	5 26	1 21	14 1	7 13
26	76,0	75,2	2,3	8,7	68,8	8,2	77,0	5 0	0 55	21 53	15 5
Oct. 1	74,5	73,7	2,3	− 8,4	67,6	8,1	75,7	4 44	0 39	26 50	20 2
Dec. 24	63,0	62,3	2,6	+ 3,5	68,4	7,8	76,2	0 17	10 26 12	6 21 30	9 14 42
1729, Feb. 25	56,5	55,9	2,9	9,4	68,2	7,5	75,7	7 26 57	22 52	8 25 1	11 18 13
26	57,0	56,4	2,9	9,4	68,7	7,5	76,2	26 54	22 49	26 1	19 13
: 28	56,8	56,2	2,9	+ 9,4	68,5	7,5	76,0	26 48	22 43	28 0	21 12
Sept. : 19	77,5	76,7	3,7	− 9,2	71,2	6,5	77,7	16 3	11 58	3 14 45	6 7 57
20	76,0	75,2	3,7	9,1	69,8	6,5	76,3	15 59	11 54	15 44	8 56
24	75,3	74,5	3,7	9,0	69,2	6,5	75,7	15 47	11 42	19 41	12 53
29	76,0	75,2	3,8	8,6	70,4	6,4	76,4	15 31	11 26	24 37	17 49
Oct. 8	74,5	73,7	3,8	− 7,8	69,7	6,4	76,1	14 59	10 54	4 3 32	26 44
Dec. 10	66,2	65,5	4,1	+ 1,2	70,8	6,1	76,9	11 42	7 37	6 7 1	9 0 13
1730, March 3	58,4	57,8	4,4	9,4	71,6	5,5	77,1	7 19	3 14	9 0 44	11 23 56
1731, Feb. 5	59,7	59,1	5,8	8,5	73,4	+3,0	76,4	6 19 22	9 15 17	8 4 32	10 27 44
1733, Jan. 31	64,8	64,1	8,7	8,2	81,0	−2,9	78,1	5 10 55	8 6 50	0 0	23 12
1738, Dec. 30	62,5	61,8	17,2	4,3	83,3	6,5	76,8	1 16 36	4 12 31	6 27 8	9 20 20
1739, Jan. 14	60,2	59,5	17,2	6,4	83,1	6,4	76,7	15 48	11 43	7 12 23	10 5 35
Feb. : : 2	58,0	57,4	17,3	8,3	83,0	6,3	76,7	14 47	10 42	8 1 34	24 46
4	57,5	56,9	17,3	8,5	82,7	6,3	76,4	14 41	10 36	3 35	26 47
6	57,2	56,6	17,3	8,5	82,4	6,3	76,1	14 34	10 29	5 36	28 48
1740, Jan. 20	56,6	56,0	18,6	7,1	81,7	−3,9	77,8	0 26 8	3 22 3	7 18 4	11 16
1747, Feb. 27	32,7	32,3	28,5	+ 9,4	70,2	+8,4	78,6	8 8 43	11 4 38	8 26 38	11 19 50

γ Ursæ Majoris south of 34° 45′.

−19″,98 annual precession.

16″,67 maximum of aberration.

6ˢ 5° 1′=12ˢ−right ascension of the star.

6ˢ 11° 11′=longitude of sun's place when the aberration in declination is 0.

γ Urs. Maj.	Measured Dist.	Reduced Dist.	Precession.	Aberration.	Corrected Dist.	Nutation.	Mean Dist.	Argument of Nutation.	Moon's ascend. Node.	Argument of Aberration.	Sun's Longitude.
			−	+		−					
	″	″	″	″	″	″	″	S. ° ′	S. ° ′	S. ° ′	S. ° ′
1727, Sept. 15	48,7	48,2	9,4	2,4	41,2	0,6	40,6	5 25 54	11 20 53	11 22 35	6 3 46
22	: 50,0	49,5	9,8	0,5	40,2	0,7	39,5	25 32	20 31	29 27	10 38
24	51,7	51,1	9,9	+ 0,1	41,3	0,7	40,6	25 16	20 15	0 1 27	12 38
Oct. 16	58,3	57,7	11,0	− 6,3	40,4	0,9	39,5	24 15	19 14	23 16	7 4 27
: 22	60,5	59,8	11,4	7,9	40,5	0,9	39,6	23 56	18 55	29 15	7 10 26
manè. 27	61,5	60,8	11,6	8,9	40,3	1,0	39,3	23 40	18 39	1 4 16	15 27
m. Nov. 17	68,7	67,9	12,8	13,4	41,7	1,2	40,5	22 34	17 33	25 26	8 6 37
m. Dec. 6	: 72,5	71,7	13,8	16,0	41,9	1,3	40,6	21 33	16 32	2 14 42	25 53
manè. 13	72,0	71,2	14,2	16,5	40,5	1,4	39,1	21 11	16 10	21 50	9 3 1
m. 14	73,0	72,2	14,2	16,5	41,5	1,4	40,1	21 8	16 7	22 51	4 2
m. 29	75,0	74,2	15,1	16,5	42,6	1,5	41,1	20 20	15 19	3 8 4	19 15
m. 1728, Ján. 7	74,7	73,9	15,5	16,0	42,4	1,6	40,8	19 52	14 51	17 13	28 24
n. : 16	76,0	75,2	16,1	14,9	44,2	1,7	42,5	19 23	14 22	26 20	10 7 31
: : 27	73,0	72,2	16,6	13,2	42,4	1,8	40,6	18 48	13 47	4 7 27	18 38
Feb. 17	70,6	69,8	17,8	8,7	43,3	1,9	41,4	17 41	12 40	28 33	11 9 44
March 17	62,7	62,0	19,4	− 0,8	41,8	2,2	39,6	16 9	11 8	5 27 18	0 8 29
22	59,2	58,6	19,6	+ 0,7	39,7	2,2	37,5	15 53	10 52	6 2 12	13 23
24	59,3	58,7	19,7	1,3	40,3	2,2	38,1	15 45	10 44	4 10	15 21
April : 2	56,6	56,0	20,3	3,7	39,4	2,3	37,1	15 15	10 14	12 57	24 8
16	55,6	55,0	21,1	7,4	41,3	2,4	38,9	14 34	9 33	26 34	1 7 45
: : 17	56,0	55,4	21,1	7,7	42,0	2,4	39,6	14 31	9 30	27 32	8 43
26	52,5	51,9	21,6	9,8	40,1	2,5	37,6	14 2	9 1	7 6 11	17 22
May 7	54,2	53,6	22,2	12,1	43,5	2,5	41,0	13 27	8 26	16 45	27 56
June 5	51,0	50,4	23,8	16,0	42,6	2,8	39,8	11 55	6 54	8 14 25	2 25 36
: : 8	49,5	49,0	24,0	16,2	41,2	2,8	38,4	11 45	6 44	17 16	28 27
15	51,7	51,1	24,4	16,5	43,2	2,9	40,3	11 23	6 22	23 55	3 5 6
: : 22	50,3	49,8	24,7	16,7	41,8	2,9	38,9	11 1	6 0	9 0 35	11 46
: 24	51,0	50,4	24,9	16,6	42,1	2,9	39,2	10 55	5 54	2 29	13 40
July : : 3	52,0	51,4	25,3	16,3	42,4	3,0	39,4	10 26	5 25	11 3	22 14
Aug. 24	65,0	64,3	28,2	8,1	44,2	3,4	40,8	7 41	2 40	11 0 52	5 12 3
Sept. 11	69,0	68,2	29,2	3,3	42,3	3,5	38,8	6 44	1 43	19 24	6 0 35
13	71,0	70,2	29,3	2,8	43,7	3,6	40,1	6 37	1 36	21 22	2 33
20	72,5	71,7	29,7	0,8	42,8	3,6	39,2	6 15	1 14	28 14	9 25
21	73,0	72,2	29,7	+ 0,5	43,0	3,6	39,4	6 12	1 11	29 13	10 24
Oct. 11	80,0	79,1	30,8	− 5,2	43,1	3,8	39,3	5 8	0 7	0 19 2	7 0 13
: 22	82,0	81,1	31,4	8,1	41,6	3,9	37,7	4 33	10 29 32	1 0 0	11 11

γ Urs. Maj.	Measured Dist.	Reduced Dist.	Precession.	Aberration.	Corrected Dist.	Nutation.	Mean Dist.	Argument of Nutation.	Moon's ascend. Node.	Argument of Aberration.	Sun's Longitude.
			—	—		—					
	"	"	"	"	"	"	"	S. ° ′	S. ° ′	S. ° ′	S. ° ′
m. 1728, Nov. 23	92,5	91,5	33,2	14,6	43,7	4,1	39,6	5 2 52	10 27 51	2 2 17	8 13 28
m. 24	92,0	91,0	33,2	14,7	43,1	4,1	39,0	2 49	27 48	3 18	14 29
m. Dec. 7	93,0	92,0	34,0	16,1	41,9	4,2	37,7	2 7	27 6	15 28	26 39
m. 16	95,0	94,0	34,5	16,6	42,9	4,3	38,6	1 39	26 38	25 38	9 6 49
m. 1729, Jan. 4	96,0	95,0	35,5	16,2	43,3	4,4	38,9	0 38	25 37	3 14 56	26 7
March 9	85,3	84,4	39,0	− 3,1	42,3	4,9	37,4	4 27 15	22 14	5 19 2	0 0 13
April 16	78,3	77,5	41,1	+ 7,4	43,8	5,1	38,7	25 14	20 13	6 26 19	1 7 30
May 11	73,0	72,2	42,4	12,8	42,6	5,3	37,3	23 55	18 54	7 20 21	2 1 32
13	72,8	72,0	42,6	13,4	42,8	5,3	37,5	23 45	18 44	22 15	3 26
30	72,5	71,7	43,5	15,5	43,7	5,4	38,3	22 55	17 54	8 8 29	19 40
June 4	73,5	72,7	43,7	15,9	44,9	5,4	39,5	22 39	17 38	13 14	24 25
6	73,5	72,7	43,8	16,1	45,0	5,5	39,5	22 33	17 32	15 9	26 20
: 7	73,0	72,2	43,9	16,2	44,5	5,5	39,0	22 30	17 29	16 6	27 17
: 10	73,3	72,5	44,1	16,3	44,7	5,5	39,2	22 20	17 19	18 57	3 0 8
: : 23	74,5	73,7	44,8	16,7	45,6	5,6	40,0	21 38	16 37	9 1 18	12 29
: 30	74,7	73,9	45,2	16,5	45,2	5,6	39,6	21 16	16 15	7 57	19 8
1730, June 21	95,5	94,5	64,7	16,7	46,5	7,6	38,9	2 25	9 27 24	8 29 10	10 21
1732, Sept. : : 3	153,5	151,8	108,6	5,5	48,7	8,9	39,8	2 19 47	8 14 46	11 10 38	5 21 49
1735, Sept. 13	210,0	207,7	169,1	3,0	41,6	−3,3	38,3	0 21 16	6 16 15	20 40	6 1 51
1739, April 19	271,0	268,0	241,0	+ 8,0	35,0	+6,7	41,7	10 11 41	4 6 40	6 28 48	1 9 59
: : 24	268,5	265,6	241,3	9,2	33,5	6,7	40,2	11 26	6 25	7 3 38	14 49
: 26	267,0	264,1	241,4	9,4	32,1	6,8	38,9	11 19	6 18	4 33	16 44
1740, June 2	286,5	283,4	263,4	15,8	35,8	8,5	44,3	9 19 59	3 14 58	8 11 40	2 22 51
3	287,5	284,4	263,4	15,9	36,9	8,4	45,3	19 56	14 55	12 37	23 48
6	287,2	284,1	263,6	16,1	36,6	8,4	45,0	19 49	14 48	15 28	26 39
1741, Aug. 29	318,0	314,5	288,2	6,9	33,2	9,0	42,2	8 26 0	2 20 59	11 5 35	5 16 46
31	320,2	316,7	288,3	6,4	34,8	9,0	43,8	25 53	20 52	7 22	18 33
1742, Sept. : : 6	340,7	337,0	308,6	4,8	33,2	+8,2	41,4	6 14	1 13	13 9	24 20
1746, Sept. : : 20	435,8	431,1	389,3	+ 1,0	42,8	−1,8	41,0	5 18 8	11 13 7	27 52	6 9 3
: 23	436,7	432,0	389,4	0,0	42,6	1,9	40,7	17 58	12 57	0 0 49	12 0
1747, Feb. 27	452,7	447,8	398,1	− 6,0	43,7	−3,1	40,6	9 39	4 38	5 8 54	11 20 5

ε Ursæ Majoris south of 82° 30′.

−19″,712 annual precession.

18″,03 maximum of aberration.

$5^{s}\ 19^{\circ}\ 22' = 12^{s} -$ right ascension of the star.

$6^{s}\ 23^{\circ}\ 19' =$ longitude of sun's place when the aberration in declination is 0.

ε Urs. Maj.	Measured Dist.	Reduced Dist.	Precession.	Aberration.	Corrected Dist.	Nutation.	Mean Dist.	Argument of Nutation.	Moon's ascend. Node.	Argument of Aberration.	Sun's Longitude.
			—	+		—					
	"	"	"	"	"	"	"	S. ° ′	S. ° ′	S. ° ′	S. ° ′
1727, Sept. 15	95,0	94,0	9,3	6,3	91,0	3,0	88,0	5 10 15	11 20 53	11 9 30	6 2 49
24	99,8	98,7	9,8	+ 3,6	92,5	3,1	89,4	9 46	20 24	18 21	11 40
Oct. 16	105,2	104,1	11,0	− 3,2	89,9	3,3	86,6	8 36	19 14	0 11 11	7 4 30
17	106,8	105,6	11,0	3,4	91,2	3,3	87,9	8 33	19 11	12 10	5 29

ε Urs. Maj.	Measured Dist.	Reduced Dist.	Precession.	Aberration.	Corrected Dist.	Nutation.	Mean Dist.	Argument of Nutation.	Moon's ascend. Node.	Argument of Aberration.	Sun's Longitude.
	″	″	— ″	— ″	″	— ″	″	S. ° ′	S. ° ′	S. ° ′	S. ° ′
1727, Oct. : 20	108,0	106,8	11,2	4,4	91,2	3,3	87,9	5 8 24	11 19 2	0 15 10	7 8 29
: 22	107,3	106,1	11,3	5,0	89,8	3,3	86,5	8 18	18 56	17 11	10 30
27	109,5	108,3	11,6	6,5	90,2	3,4	86,8	8 1	18 39	22 11	15 30
Nov. 13	116,8	115,5	12,5	11,2	91,8	3,5	88,3	7 7	17 45	1 9 19	8 2 38
14	115,0	113,7	12,5	11,4	89,8	3,5	86,3	7 4	17 42	10 19	3 38
15	115,0	113,7	12,6	11,6	89,5	3,5	86,0	7 1	17 39	11 19	4 38
17	116,7	115,4	12,7	12,0	90,7	3,5	87,2	6 55	17 33	12 21	6 40
Dec. : : 6	126,0	124,6	13,7	15.9	94,0	3,7	90,3	5 54	16 32	2 2 37	25 56
m. 13	122,5	121,2	14,1	16,8	90,3	3,7	86,8	5 32	16 10	9 44	9 3 3
: 14	124,3	123,0	14,2	16,9	91,9	3,7	88,2	5 29	16 7	10 45	4 4
29	128,0	126,6	15,0	17,9	93,7	3,8	89,9	4 41	15 19	25 59	19 18
1728, Jan. 7	128,0	126,6	15,5	17,9	93,2	3,9	89,3	4 13	14 51	3 5 8	28 27
n. 24	127,0	125,6	16,4	16,7	92,5	4,0	88,2	3 19	13 57	22 20	10 15 39
25	126,5	125,1	16,4	16,5	92,2	4,0	88,2	3 16	13 54	23 21	16 40
Feb. 17	124,8	123,4	17,7	12,4	93,3	4,2	89,1	2 2	12 40	4 16 28	11 9 47
March 21	114,5	113,3	19,4	– 3,4	90,5	4,5	86,0	0 18	10 56	5 19 8	0 12 27
April 6	110,0	108,8	20,2	+ 1,5	90,1	4,6	85,5	4 29 27	10 5	6 4 46	28 5
: 7	111,7	110,5	20,3	1,8	92,0	4,6	87,4	29 24	10 2	5 44	29 3
16	108,3	107,1	20,8	4,5	90,8	4,6	86,2	28 55	9 33	14 28	1 7 47
: : 17	111,8	110,6	20,8	4,8	94,6	4,7	89,9	28 52	9 30	15 25	8 44
: 18	107,8	106,6	20,9	5,1	90,8	4,7	86,1	28 49	9 27	16 23	9 42
: 24	107,8	106,6	21,2	6.8	92,2	4,7	87,5	28 30	9 8	22 10	15 29
May : 5	105,2	104,1	21,8	9,7	92,0	4,8	87,2	27 55	8 33	7 2 45	26 4
11	105,0	103,9	22,1	11,2	93,0	4,8	88,2	27 35	8 13	8 30	2 1 49
June 5	101,0	99,9	23,5	16,0	92,4	5,0	87,4	26 16	6 54	8 2 20	25 39
8	101,0	99,9	23,7	16,4	92,6	5,0	87,6	26 7	6 45	5 11	28 30
9	100,5	99,4	23,7	16,5	92,2	5,0	87,2	26 4	6 42	6 7	29 27
15	100,8	99,7	24,0	17,0	92,7	5,1	87,6	25 44	6 22	11 50	3 5 9
22	99,0	97,9	24,4	17,7	91,2	5,1	86,1	25 22	6 0	18 29	11 48
24	100,5	99,4	24,5	17,8	92,7	5,1	87,6	25 16	5 54	20 23	13 42
Ld. C. 25	101,0	99,9	24,6	17,8	93,1	5,1	88,0	25 13	5 51	21 20	14 39
July 5	101,5	100,4	25,1	18,0	93,3	5,2	88,1	24 41	5 19	9 0 51	24 10
6	102,5	101,4	25,1	18,0	94,3	5,2	89,1	24 38	5 16	1 48	25 7
Aug. : 2	107,0	105,8	26,6	16,0	95,2	5,4	89,8	23 12	3 50	27 36	4 20 55
5	108,0	106,8	26,8	15,5	95,5	5,4	90,1	23 2	3 40	10 0 29	23 48
24	110,7	109,5	27,8	11,9	93,6	5,5	88,1	22 2	2 40	18 47	5 12 6
Sept. 6	115,0	113,7	28,5	8,6	93,8	5,6	88,2	21 21	1 59	11 1 27	24 46
: : 11	115,5	114,2	28,8	7,2	92,6	5,6	87,0	21 5	1 43	6 20	29 39
13	117,0	115,7	28,9	+ 6,7	93,5	5,7	87,8	20 59	1 37	8 18	6 1 37
Oct. 9	125,8	124,4	30,3	– 1,2	92,9	5,8	87,1	19 35	0 13	0 4 57	28 16
11	126,5	125,1	30,4	1,9	93,8	5,9	87,9	19 29	0 7	6 56	7 0 15
29	132,7	131,2	31,4	7,3	92,5	6,0	86,5	18 32	10 29 10	24 57	18 16
Nov. 17	139,0	137,5	32,4	12,3	92,8	6,1	86,7	17 32	28 10	1 14 7	8 7 26
m. : 23	142,0	140,5	32,8	13,6	94,1	6,1	88,0	17 13	27 51	20 11	13 30
24	140,5	139,0	32,8	13,9	92,3	6,1	86,2	17 10	27 48	21 12	14 31
Dec. : : 7	143,5	141,9	33,5	16,1	92,3	6,2	86,1	16 28	27 6	2 4 24	27 43
11	145,3	143,7	33,7	16,6	93,4	6,2	87,2	16 16	26 54	8 28	9 1 47
16	145,5	143,9	33,9	17,2	92,8	6,2	86,6	16 0	26 38	13 33	6 52

ε Urs. Maj.	Measured Dist.	Reduced Dist.	Precession.	Aberration.	Corrected Dist.	Nutation.	Mean Dist.	Argument of Nutation.	Moon's ascend. Node.	Argument of Aberration.	Sun's Longitude.
			—	—	—	—					
	″	″	″	″	″	″	″	S. ° ′	S. ° ′	S. ° ′	S. ° ′
1729, Jan. 16	148,0	146,4	35,7	17,5	93,2	6,4	86,8	4 14 21	10 24 59	3 15 2	10 8 21
20	149,0	147,4	35,9	17,2	94,3	6,4	87,9	14 9	24 47	19 5	12 24
21	147,5	145,9	35,9	17,0	93,0	6,5	86,5	14 6	24 44	20 5	13 24
22	148,0	146,4	36,0	16,9	93,5	6,5	87,0	14 2	24 40	21 5	14 24
25	149,5	147,9	36,1	16,5	95,3	6,5	88,8	13 53	24 31	24 6	17 25
m. 27	148,5	146,9	36,2	16,3	94,4	6,5	87,9	13 47	24 25	26 7	19 26
March : : 9	140,7	139,2	38,4	− 7,0	93,8	6,7	87,1	11 36	22 14	5 7 5	0 0 24
April 4	134,5	133,0	39,8	+ 0,8	94,0	6,9	87,1	10 13	20 51	6 2 35	25 54
5	135,0	133,5	39,9	1,1	94,7	6,9	87,8	10 10	20 48	3 33	26 52
: 16	131,8	130,4	40,5	4,5	94,4	6,9	87,5	9 33	20 13	14 13	1 7 32
May 11	124,5	123,1	41,8	11,2	92,5	7,1	85,4	8 16	18 54	7 8 16	2 1 35
30	123,2	121,9	42,9	15,0	94,0	7,2	86,8	7 16	17 54	26 24	19 43
June 4	123,2	121,9	43,1	15,8	94,6	7,2	87,4	7 0	17 38	8 1 9	24 28
7	123,7	122,3	43,3	16,2	95,2	7,2	88,0	6 50	17 28	4 1	27 20
18	121,8	120,5	43,9	17,3	93,9	7,3	86,6	6 15	16 53	14 27	3 7 46
: 23	122,0	120,7	44,2	17,7	94,2	7,3	86,9	5 59	16 37	19 13	12 32
: 28	123,5	122,2	44,4	17,9	95,7	7,3	88,4	5 47	16 25	23 58	17 17
30	121,5	120,2	44,5	17,9	93,6	7,3	86,3	5 37	16 15	25 52	19 11
July 4	121,3	120,0	44,7	18,0	93,3	7,3	86,0	5 24	16 2	29 41	23 0
5	122,0	120,7	44,8	18,0	93,9	7,3	86,6	5 21	15 59	9 0 37	23 56
: 18	124,0	122,6	45,5	17,6	94,7	7,4	87,3	4 40	15 18	13 1	4 6 20
21	125,0	123,6	45,7	17,3	95,2	7,4	87,8	4 30	15 8	15 52	9 11
Aug. : 21	130,0	128,6	47,4	+12,5	93,7	7,6	86,1	2 52	13 30	10 15 39	5 8 58
Oct. 13	147,0	145,4	50,2	− 2,4	92,8	7,7	85,1	1 39	12 17	0 8 42	7 2 1
14	147,8	146,2	50,2	2,7	93,3	7,7	85,6	1 36	12 14	9 42	3 1
24	152,0	150,3	50,8	− 5,8	93,7	7,7	86,0	1 6	11 42	19 41	13 0
1730, June : 17	143,0	141,4	63,5	+17,2	95,1	8,6	86,5	3 16 59	9 27 37	8 13 17	3 6 36
July 24	147,0	145,4	65,6	17,0	96,8	8,7	88,1	15 1	25 39	9 18 30	4 11 49
Aug. : 18	152,0	150,3	66,9	13,3	96,7	8,7	88,0	13 42	24 20	10 12 33	5 5 52
23	152,0	150,3	67,1	12,2	95,4	8,8	86,6	13 26	24 4	17 21	10 40
26	154,5	152,8	67,3	+11,5	97,0	8,8	88,2	13 16	23 54	20 16	13 35
Dec. 28	189,0	186,9	74,0	−17,9	95,0	8,9	86,1	6 32	17 10	2 25 15	9 18 34
1731, Jan. : 3	189,0	186,9	74,3	−18,0	94,6	9,0	85,6	6 23	17 1	3 1 21	24 40
Aug. 4	167,5	165,7	85,8	+15,8	95,7	9,0	86,7	2 25 6	5 44	9 28 48	4 22 7
: 20	172,0	170,1	86,7	12,9	96,3	8,9	87,4	24 16	4 54	10 14 13	5 7 32
Sept. : 10	177,0	175,1	87,9	7,7	94,9	8,9	86,0	23 9	3 47	11 4 38	27 57
13	177,0	175,1	88,0	6,9	94,0	8,9	85,1	22 59	3 37	7 34	6 0 53
15	178,5	176,6	88,1	6,3	94,8	8,9	85,9	22 53	3 31	9 32	2 51
Oct. 1	185,0	183,0	88,9	+ 1,5	95,6	8,9	86,7	22 2	2 40	26 17	19 36
m. 1732, Jan. 1	209,0	206,7	93,9	−18,0	94,8	8,8	86,0	17 10	8 27 48	2 29 4	9 22 23
m. 9	208,4	206,1	94,4	17,8	93,9	8,8	85,1	16 44	27 22	3 7 11	10 0 30
q. m. 11	206,3	204,0	94,5	−17,7	91,8	8,8	83,0	16 38	27 16	9 13	2 32
Sept. 3	196,0	193,9	107,2	+ 9,4	96,1	8,1	88,0	4 8	14 46	10 28 32	5 21 51
4	197,0	194,8	107,2	9,2	96,8	8,1	88,7	4 5	14 43	29 31	22 50
6	195,0	192,9	107,3	8,7	94,3	8,1	86,2	4 1	14 37	11 1 28	24 47
1734, June 11	219,5	217,1	142,1	16,7	91,7	4,5	87,2	0 29 56	7 10 34	8 7 36	3 0 55
12	220,5	218,1	142,1	16,8	92,8	4,5	88,3	29 53	10 31	8 33	1 52
: 16	220,5	218,1	142,4	17,2	92,9	4,5	88,4	29 40	10 18	12 22	5 41

ε Urs. Maj.	Measured Dist.	Reduced Dist.	Precession.	Aberration.	Corrected Dist.	Nutation.	Mean Dist.	Argument of Nutation.	Moon's ascend. Node.	Argument of Aberration.	Sun's Longitude.
			−	+		−					
	″	″	″	″	″	″	″	S. ° ′	S. ° ′	S. ° ′	S. ° ′
1734, July 9	221,0	218,6	143,6	18,0	93,0	4,3	88,7	0 28 27	7 9 5	9 4 13	3 27 32
10	220,5	218,1	143,6	18,0	92,5	4,3	88,2	28 24	9 2	5 10	28 29
17	221,0	218,6	144,0	17,6	92,2	4,2	88,0	28 1	8 39	11 51	4 5 10
: : 31	222,0	219,6	144,8	16,3	91,1	4,1	87,0	27 17	7 55	25 16	18 35
1735, Sept. : : 13	254,5	251,7	166,9	6,9	91,7	−0,9	90,8	5 37	6 16 15	11 7 37	6 0 56
1736, Sept. : 10	268,0	265,1	186,4	7,5	86,2	+2,1	88,3	11 16 24	5 27 2	5 24	5 28 43
: 12	267,8	264,9	186,5	6,9	85,3	2,3	87,6	15 18	26 56	7 22	6 0 41
1737, Sept. 6	285,3	282,2	205,9	8,7	85,0	4,9	89,9	10 27 17	7 55	1 16	5 24 35
1739, April 19	317,0	313,6	237,8	5,2	81,0	8,0	89,0	9 27 2	4 6 40	6 16 43	1 10 2
: : 25	312,5	309,1	238,1	6,9	77,9	8,1	86,0	25 43	6 21	22 30	15 49
26	315,2	311,8	238,1	7,2	80,9	8,1	88,0	25 40	6 18	23 28	16 47
1740, May 30	332,2	328,6	259,7	15,1	84,0	9,0	93,0	4 29	3 15 7	7 26 43	2 20 2
: : 31	333,7	330,1	259,7	15,2	85,6	9,0	94,6	4 26	15 4	27 40	20 59
June 1	332,7	329,1	259,8	15,4	84,7	9,0	93,7	4 23	15 1	28 37	21 56
3	332,7	329,1	259,9	15,7	84,9	9,0	93,9	4 17	14 55	8 0 31	23 50
4	331,0	327,4	260,0	15,8	83,2	9,0	92,2	4 14	14 52	1 28	24 47
6	331,7	328,1	260,1	16,1	84,1	9,0	93,1	4 8	14 46	3 22	26 41
10	331,0	327,4	260,3	16,7	83,8	9,0	92,8	3 54	14 32	7 10	3 0 29
1741, Aug. 31	363,0	359,0	284,5	10,2	84,7	8,5	93,2	8 10 14	2 20 52	10 25 27	5 18 46
Sept. 1	361,9	358,0	284,6	10,0	83,4	8,5	91,9	10 11	20 49	26 26	19 45
3	362,3	358,4	284,7	9,7	83,4	8,5	91,9	10 5	20 43	27 23	21 42
: 21	368,7	364,7	285,6	5,3	84,4	8,4	92,8	9 7	19 45	11 12 59	6 6 18
23	370,2	366,2	285,7	3,8	84,3	8,4	92,7	9 1	19 39	17 58	11 17
1742, Sept. 6	382,2	378,0	304,5	8,7	82,2	+7,0	89,2	7 20 35	1 13	1 4	5 24 23
1745, Sept. 5	449,9	445,0	363,6	8,8	90,2	−1,2	89,0	5 22 36	0 3 14	0 22	23 41
1746, Sept. 19	475,9	470,7	384,0	5,0	91,7	4,2	87,5	2 32	11 13 10	13 48	6 7 7
23	477,2	472,0	384,2	+ 3,8	91,6	4,2	87,4	2 19	12 57	17 45	11 4
1747, Feb. 27	499,2	493,8	392,7	− 9,9	91,2	5,3	85,9	4 24 0	4 38	4 26 47	11 20 6
Sept. 3	497,0	491,6	402,8	+ 9,6	98,4	6,5	91,9	14 6	10 24 44	10 27 54	5 21 15

η Ursæ Majoris south of 39° 15′.

−18″,279 annual precession.

17″,81 maximum of aberration.

$5^s\ 5°\ 42' = 12^s -$ right ascension of the star.

$7^s\ 7°\ 41' =$ longitude of sun's place when the aberration in declination is 0.

η Urs. Maj.	Measured Dist.	Reduced Dist.	Precession.	Aberration.	Corrected Dist.	Nutation.	Mean Dist.	Argument of Nutation.	Moon's ascend. Node.	Argument of Aberration.	Sun's Longitude.
			−	+		−					
	″	″	″	″	″	″	″	S. ° ′	S. ° ′	S. ° ′	S. ° ′
1727, Sept. 5	141,3	139,8	8,1	12,5	144,2	4,9	139,3	4 27 7	11 21 25	10 15 24	5 23 5
14	142,0	140,5	8,6	10,3	142,2	5,0	137,2	26 38	20 56	24 11	6 1 52
: : 15	144,0	142,4	8,6	10,1	143,9	5,0	138,9	26 35	20 53	25 10	2 51
24	147,4	145,8	9,1	7,8	144,5	5,0	139,5	26 6	20 24	11 4 1	11 42

η Urs. Maj.	Measured Dist.	Reduced Dist.	Precession.	Aberration.	Corrected Dist.	Nutation.	Mean Dist.	Argument of Nutation.	Moon's ascend. Node.	Argument of Aberration.	Sun's Longitude.
			−	+		−					
	″	″	″	″	″	″	″	s. ° ′	s. ° ′	s. ° ′	s. ° ′
1727, Sept. : 25	146,0	144,4	9,1	7,5	142,8	5,0	137,8	4 26 3	11 20 21	11 5 0	6 12 41
manè. Oct. 13	152,0	150,3	10,0	2,2	142,5	5,1	137,4	25 6	19 24	23 51	7 1 32
16	153,7	152,0	10,2	1,3	143,1	5,2	137,9	24 56	19 14	26 51	4 32
17	155,0	153,3	10,2	1,0	144,1	5,2	138,9	24 53	19 11	27 50	5 31
20	155,8	154,1	10,4	+ 0,1	143,8	5,2	138,6	24 44	19 2	0 0 50	8 31
26	156,7	155,0	10,6	− 1,9	142,5	5,2	137,3	24 25	18 43	6 51	14 32
manè. 27	158,0	156,3	10,6	2,3	143,3	5,2	138,1	24 22	18 40	7 51	15 32
Nov. 13	162,5	160,7	11,6	7,2	141,9	5,4	136,5	23 27	17 45	24 59	8 2 40
14	163,5	161,7	11,6	7,5	142,6	5,4	137,2	23 24	17 42	25 59	3 40
15	163,7	161,9	11,7	7,8	142,4	5,4	137,0	23 21	17 39	26 59	4 40
17	164,3	162,5	11,8	8,3	142,4	5,4	137,0	23 15	17 33	29 1	6 42
manè. Dec. f. : : 6	169,8	168,0	12,7	13,1	142,2	5,5	136,7	22 14	16 32	1 18 17	25 58
13	173,0	171,1	13,1	14,4	143,6	5,6	138,0	21 38	15 56	25 24	9 3 5
f. : 14	172,5	170,6	13,1	14,6	142,9	5,6	137,3	21 35	15 53	26 25	4 6
29	177,2	175,3	13,9	16,8	144,6	5,7	138,9	21 1	15 19	2 11 40	19 21
1728, Jan. f. 23	178,0	176,1	15,1	17,7	143,3	5,8	137,5	19 42	14 0	3 7 0	10 14 41
24	178,3	176,4	15,1	17,6	143,7	5,8	137,9	19 39	13 57	8 1	15 42
25	180,0	178,0	15,2	17,5	145,3	5,8	139,5	19 36	13 54	9 1	16 42
f. : 26	179,0	177,0	15,3	17,5	144,2	5,9	138,3	19 33	13 51	10 2	17 43
Feb. 17	178,3	176,4	16,4	15,1	144,9	6,0	138,9	18 22	12 40	4 2 8	11 9 49
March 21	170,5	168,6	18,0	7,6	143,0	6,2	136,8	16 38	10 56	5 4 48	0 12 29
April f. : : 7	164,5	162,7	18,8	2,7	141,2	6,3	134,9	15 44	10 2	21 24	29 5
10	165,5	163,7	19,0	1,8	142,9	6,3	136,6	15 31	9 49	24 19	1 2 0
15	164,0	162,2	19,2	− 0,3	142,7	6,3	136,4	15 18	9 36	29 10	6 51
16	164,8	163,0	19,3	0,0	143,7	6,3	137,4	15 15	9 33	6 0 8	7 49
f. : : 17	164,8	163,0	19,3	+ 0,3	144,0	6,4	137,6	15 12	9 30	1 6	8 47
f. : 18	163,7	161,9	19,4	0,6	143,1	6,4	136,7	15 9	9 27	2 4	9 45
May 5	159,7	158,0	20,2	5,6	143,4	6,4	137,0	14 15	8 33	18 25	26 6
f. : 11	157,0	155,3	20,5	7,3	142,1	6,5	135,6	13 55	8 13	24 10	2 1 51
June 5	153,8	152,1	21,8	13,2	143,5	6,6	136,9	12 36	6 54	7 18 0	25 41
8	153,5	151,8	21,9	13,7	143,6	6,6	137,0	12 27	6 45	20 51	28 32
15	153,5	151,8	22,3	15,0	144,5	6,7	137,8	12 4	6 22	27 31	3 5 12
21	153,0	151,3	22,6	16,0	144,7	6,7	138,0	11 45	6 3	8 3 13	10 54
22	152,4	150,7	22,6	16,1	144,2	6,7	137,5	11 42	6 0	4 10	11 51
24	151,5	149,9	22,7	16,3	143,5	6,7	136,8	11 36	5 54	6 4	13 45
Ld. C. Cav. 25	152,0	150,3	22,8	16,4	143,9	6,7	137,2	11 33	5 51	7 1	14 42
28	151,5	149,9	22,9	16,7	143,7	6,8	136,9	11 23	5 41	9 52	17 33
29	152,0	150,3	23,0	16,8	144,1	6,8	137,3	11 20	5 38	10 49	18 30
July 2	151,8	150,1	23,1	17,1	144,1	6,8	137,3	11 10	5 28	13 40	21 21
17	152,5	150,8	23,9	17,8	144,7	6,9	137,8	10 23	4 41	27 57	4 5 38
Aug. 2	154,0	152,3	24,7	17,3	144,9	6,9	138,0	9 32	3 50	9 13 15	20 56
: : 3	154,0	152,3	24,7	17,3	144,9	6,9	138,0	9 29	3 47	14 13	21 54
5	155,0	153,3	24,9	17,1	145,5	7,0	138,5	9 23	3 41	16 8	23 49
12	156,0	154,3	25,2	16,4	145,5	7,0	138,5	9 3	3 21	22 52	5 0 33
24	158,3	156,6	25,8	14,7	145,5	7,1	138,4	8 22	2 40	10 4 28	12 9
Sept. 8	162,2	160,4	26,5	11,6	145,5	7,1	138,4	7 34	1 52	19 4	26 45
20	165,5	163,7	27,1	8,7	145,3	7,2	138,1	6 56	1 14	11 0 49	6 8 30
21	166,0	164,2	27,2	8,4	145,4	7,2	138,2	6 53	1 11	1 48	9 29
28	167,7	165,9	27,6	6,4	144,7	7,2	137,5	6 31	0 49	8 43	16 24

η Urs. Maj.	Measured Dist.	Reduced Dist.	Precession.	Aberration.	Corrected Dist.	Nutation.	Mean Dist.	Argument of Nutation.	Moon's ascend. Node.	Argument of Aberration.	Sun's Longitude.
			−	+		−					
	″	″	″	″	″	″	″	S. ° ′	S. ° ′	S. ° ′	S. ° ′
1728, Oct. : 9	172,5	170,6	28,1	3,2	145,7	7,3	138,4	4 5 55	11 0 13	11 20 37	6 28 18
11	172,5	170,6	28,2	+ 2,6	145,0	7,3	137,7	5 49	0 7	22 37	7 0 18
: : 29	178,5	176,6	29,1	− 3,0	144,5	7,4	137,1	4 52	10 29 10	0 10 37	18 18
Nov. 17	184,7	182,7	30,1	8,6	144,0	7,5	137,5	3 52	28 10	29 47	8 7 28
19	185,5	183,5	30,1	9,2	144,2	7,5	136,7	3 46	28 4	1 1 48	9 29
manè. f. : 23	187,0	185,0	30,3	10,1	144,6	7,5	137,1	3 33	27 51	5 51	13 32
24	186,5	184,5	30,4	10,4	143,7	7,5	136,2	3 30	27 48	6 52	14 33
Dec. 11	193,0	190,9	31,3	14,3	145,3	7,6	137,7	2 36	26 54	24 8	9 1 49
16	192,6	190,5	31,5	15,1	143,9	7,6	136,3	2 30	26 38	29 12	6 53
1729, Jan. 16	198,8	196,6	33,1	17,8	145,7	7,7	138,0	0 41	24 59	3 0 41	10 8 22
20	199,5	197,3	33,3	17,8	146,2	7,7	138,5	0 29	24 47	4 44	12 25
21	198,7	196,5	33,3	17,7	145,5	7,8	137,7	0 26	24 44	5 45	13 26
22	199,0	196,8	33,4	17,7	145,8	7,8	138,0	0 23	24 41	6 46	14 27
25	199,7	197,5	33,5	17,6	146,4	7,8	138,6	0 13	24 31	9 47	17 28
27	199,0	196,8	33,6	17,5	145,7	7,8	137,9	0 7	24 25	11 48	19 29
April : 4	188,0	185,9	37,0	3,6	145,3	8,0	137,3	3 26 33	20 51	5 18 15	0 25 56
5	187,0	185,0	37,0	3,3	144,7	8,1	136,6	26 30	20 48	19 13	26 54
: : 8	187,0	185,0	37,2	2,4	145,4	8,1	137,3	26 21	20 39	21 58	29 39
12	184,8	182,8	37,4	1,2	144,2	8,1	136,1	26 18	20 26	26 1	1 3 42
13	185,2	183,2	37,4	− 0,9	144,9	8,1	136,8	26 15	20 23	26 59	4 40
16	183,5	181,5	37,6	0,0	144,0	8,1	135,9	26 5	20 13	29 53	7 34
May 11	178,8	176,8	38,8	+ 7,2	145,2	8,2	137,0	24 36	18 54	6 23 56	2 1 37
30	173,5	171,6	39,8	11,9	143,7	8,2	135,5	23 36	17 54	7 12 4	19 45
31	173,5	171,6	39,8	12,1	143,9	8,2	135,7	23 33	17 51	13 1	20 42
June 4	173,4	171,5	40,1	13,0	144,5	8,3	136,2	23 20	17 38	16 49	24 30
6	173,8	171,9	40,2	13,4	145,1	8,3	136,8	23 14	17 32	18 44	26 25
7	173,3	171,4	40,2	13,6	144,8	8,3	136,5	23 11	17 29	19 40	27 21
23	171,5	169,6	41,0	16,1	144,7	8,3	136,4	22 19	16 37	8 4 53	3 12 34
25	172,5	170,6	41,1	16,3	145,8	8,3	137,5	22 13	16 31	6 47	14 28
h : 29	170,5	168,6	41,3	16,8	144,1	8,3	135,8	22 0	16 18	10 35	18 16
30	171,7	169,8	41,3	16,9	145,4	8,4	137,0	21 57	16 15	11 32	19 13
July 5	172,0	170,1	41,6	17,3	145,8	8,4	137,4	21 41	15 59	16 17	23 58
21	172,3	170,4	42,4	17,8	145,8	8,4	137,4	20 50	15 8	9 1 32	4 9 13
Sept. 10	181,0	179,0	44,9	11,2	145,3	8,5	136,8	18 8	12 26	10 20 47	5 28 28
: 22	185,3	183,3	45,5	8,2	146,0	8,5	137,5	17 30	11 48	11 2 33	6 10 14
Oct. 7	190,2	188,1	46,3	3,9	145,7	8,6	137,1	16 42	11 0	17 23	25 4
13	191,3	189,2	46,6	2,1	144,7	8,6	136,1	16 23	10 41	24 22	7 2 3
14	192,5	190,4	46,6	+ 1,8	145,6	8,6	137,0	16 20	10 38	25 22	3 3
: 24	195,5	193,4	47,1	− 1,7	144,6	8,7	135,9	15 48	10 6	0 5 21	13 2
1730, July 17	191,7	189,6	60,4	+17,8	147,0	9,0	138,0	1 42	9 26 0	8 27 29	4 5 10
19	191,7	189,6	60,5	17,8	146,9	9,0	137,9	1 37	25 55	29 24	7 5
24	191,0	188,9	60,8	17,7	145,8	9,0	136,8	1 21	25 39	9 4 11	11 52
Aug. 5	193,2	191,1	61,4	17,2	146,9	9,0	137,9	0 43	25 1	15 40	23 21
12	195,0	192,9	61,7	16,5	147,7	9,0	138,7	0 21	24 39	22 24	5 0 5
f. : 18	196,0	193,9	62,0	15,6	147,5	9,0	138,5	0 2	24 20	28 11	5 52
26	197,2	195,0	62,4	14,4	147,0	9,0	138,0	2 29 36	23 54	10 5 56	13 37
Sept. 18	201,5	199,3	63,6	+ 9,3	145,0	9,0	136,0	28 33	22 51	28 23	6 6 4
Dec. 28	235,0	232,4	68,6	−16,7	147,1	8,9	138,2	23 2	17 20	2 10 55	9 18 36

η Urs. Maj.	Measured Dist.	Reduced Dist.	Precession.	Aberration.	Corrected Dist.	Nutation.	Mean Dist.	Argument of Nutation.	Moon's ascend. Node.	Argument of Aberration.	Sun's Longitude.
			—	—		—					
	″	″	″	″	″	″	″	S. ° ′	S. ° ′	S. ° ′	S. ° ′
1731, Jan. 3	236,7	234,1	69,0	−17,4	147,7	8,9	138,8	2 22 43	9 17 1	2 17 1	9 24 42
Aug. 4	210,2	207,9	79,6	+17,2	145,5	8,5	137,0	11 26	5 44	9 14 29	4 22 10
7	210,5	208,2	79,8	17,0	145,4	8,5	136,9	11 17	5 35	17 22	25 3
: 17	211,8	209,5	80,3	15,9	145,1	8,5	136,6	10 45	5 3	26 59	5 4 40
20	212,5	210,2	80,4	15,5	145,3	8,5	136,8	10 36	4 54	29 53	7 34
Sept. : : 10	218,5	216,1	81,4	11,3	146,0	8,4	137,6	9 29	3 47	10 20 18	27 59
: 11	218,3	215,9	81,5	11,1	145,5	8,4	137,1	9 26	3 44	21 16	28 57
: : 12	219,5	217,1	81,5	10,9	146,5	8,4	138,1	9 23	3 41	22 15	29 56
13	219,7	217,3	81,6	10,6	146,3	8,4	137,9	9 20	3 38	23 14	6 0 55
18	220,5	218,1	81,8	9,4	145,7	8,4	137,3	9 3	3 21	28 8	5 49
h. : : 22	221,5	219,1	82,1	8,3	145,3	8,4	136,9	8 51	3 9	11 2 4	9 45
Oct. 1	224,5	222,1	82,5	5,8	145,4	8,4	137,0	8 22	2 40	10 57	18 38
4	225,0	222,5	82,7	+ 4,9	144,7	8,4	136,3	8 13	2 31	13 56	21 37
m. 1732, Jan. 1	251,0	248,3	87,2	−17,1	144,0	8,1	135,9	3 30	8 27 48	2 14 45	9 22 26
9	253,5	250,7	87,5	17,5	145,7	8,0	137,7	3 5	27 23	22 51	10 0 32
11	253,5	250,7	87,6	17,6	145,5	8,0	137,5	2 58	27 16	24 53	2 34
April 13	241,5	238,9	92,2	− 1,0	145,7	7,6	138,1	1 28 3	22 21	5 27 15	1 4 56
: 17	240,0	237,4	92,4	+ 0,3	145,3	7,6	137,7	27 47	22 5	6 1 8	8 49
1734, June 11	259,5	256,7	131,8	14,3	139,2	2,5	136,7	0 16 16	7 10 34	7 23 17	3 0 58
12	260,5	257,6	131,8	14,5	140,3	2,5	137,8	16 13	10 31	24 13	1 54
16	260,0	257,2	132,0	15,1	140,3	2,5	137,8	16 0	10 18	28 1	5 42
18	259,0	256,2	132,1	15,4	139,5	2,5	137,0	15 54	10 12	29 55	7 36
July 9	258,5	255,7	133,2	17,5	140,0	2,3	137,7	14 47	9 5	8 19 54	27 35
: 10	258,5	255,7	133,2	17,5	140,0	2,3	137,7	14 44	9 2	20 51	28 32
11	258,5	255,7	133,3	17,6	140,0	2,3	137,7	14 40	8 58	21 48	29 29
16	258,0	255,2	133,5	17,7	139,4	2,2	137,2	14 25	8 43	26 34	4 4 15
17	257,0	254,2	133,5	17,7	138,4	2,2	136,2	14 22	8 40	27 31	5 12
Aug. 3	261,5	258,6	134,4	17,3	141,5	2,1	139,4	13 27	7 45	9 13 47	21 28
7	260,3	257,5	134,6	17,0	139,9	−2,1	137,8	13 15	7 33	17 38	25 19
1735, Sept. 9	284,0	280,9	154,5	11,6	138,0	+1,2	139,2	11 22 10	6 16 28	10 19 21	5 27 2
10	283,7	280,6	154,6	11,4	137,4	1,2	138,6	22 7	16 25	20 30	28 1
1736, Sept. 8	298,0	294,7	172,8	11,6	133,5	4,1	137,6	2 50	5 27 8	19 7	26 48
12	299,0	295,7	173,0	10,7	133,4	4,1	137,5	2 38	26 56	23 2	6 0 43
1737, June 22	305,8	302,5	187,2	16,0	131,3	6,1	137,4	10 17 39	11 57	8 3 59	3 11 40
July 1	307,0	303,7	187,6	16,9	133,0	6,1	139,1	17 10	11 28	12 33	20 14
3	306,5	303,2	187,7	17,1	132,6	6,1	138,7	17 4	11 22	14 26	22 7
6	306,3	303,0	187,8	17,3	132,5	6,1	138,6	16 54	11 12	17 18	24 59
Sept. 6	315,5	312,1	190,9	12,2	133,4	6,5	139,9	10 13 37	7 55	10 16 56	5 24 37
1738, June 18	324,5	321,0	205,2	15,4	131,2	7,8	139,0	9 28 31	4 22 49	7 29 59	3 7 40
27	321,2	317,7	205,7	16,6	128,6	7,8	136,4	28 3	22 21	8 8 30	16 11
29	322,5	319,0	205,8	16,8	130,0	7,9	137,9	27 57	22 15	10 24	18 5
July 2	322,0	318,5	205,9	17,1	129,7	8,0	137,7	27 47	22 5	13 16	20 57
: : 15	322,2	318,7	206,6	17,7	129,8	8,0	137,8	27 6	21 24	25 39	4 3 20
Sept. : : 23	337,0	333,3	210,1	8,0	131,2	8,3	139,5	23 23	17 41	11 3 22	6 11 3
1739, April 19	354,5	350,6	220,5	0,7	130,8	8,8	139,6	12 22	6 40	6 2 24	1 10 5
25	351,5	347,7	220,8	2,5	129,4	8,8	138,2	12 3	6 21	8 10	15 51
26	350,9	347,1	220,8	2,8	129,1	8,8	137,9	12 0	6 18	9 8	16 49
Aug. : : : 31	349,7	345,9	227,2	13,5	132,2	8,9	141,1	5 17	3 29 35	10 10 37	5 18 18
Sept. : 3	351,4	347,6	227,4	12,9	133,1	9,0	142,1	5 7	29 25	13 32	21 13

η Urs. Maj.	Measured Dist.	Reduced Dist.	Precession.	Aberration.	Corrected Dist.	Nutation.	Mean Dist.	Argument of Nutation.	Moon's ascend. Node.	Argument of Aberration.	Sun's Longitude.
			−	+		+					
	″	″	″	″	″	″	″	S. ° ′	S. ° ′	S. ° ′	S. ° ′
1740, May 30	364,8	360,8	240,8	12,0	132,0	8,9	140,9	8 20 49	3 15 7	7 12 23	2 20 4
31	365,0	361,0	240,9	12,2	132,3	8,9	141,2	20 46	15 4	13 20	21 1
June 1	365,0	361,0	240,9	12,5	132,6	8,9	141,5	20 43	15 1	14 17	21 58
2	365,8	361,8	241,0	12,7	133,5	8,9	142,4	20 40	14 58	15 15	22 56
3	364,0	360,0	241,0	12,9	131,9	8,9	140,8	20 37	14 55	16 12	23 53
4	364,0	360,0	241,1	13,1	132,0	8,9	140,9	20 34	14 52	17 8	24 49
6	365,0	361,0	241,2	13,5	133,3	8,9	142,2	20 27	14 45	19 2	26 43
10	362,4	358,5	241,4	14,2	131,3	8,9	140,2	20 14	14 32	22 51	3 0 32
1741, Sept. 4	389,0	384,8	264,0	12,6	133,4	7,5	140,9	7 26 21	2 20 39	10 15 2	5 22 43
21	395,0	390,7	264,8	8,4	134,3	7,4	141,7	25 27	19 45	11 1 39	6 9 20
23	395,2	390,9	264,9	7,9	133,9	+7,4	141,3	25 21	19 39	3 38	11 19
1745, Sept. 5	471,8	466,7	337,1	12,3	141,9	−3,2	138,7	5 8 56	0 3 14	10 16 0	5 23 41
1746, Sept. 19	497,6	492,2	356,1	9,0	145,1	5,9	139,2	4 18 52	11 13 10	29 29	6 7 10
20	496,8	491,4	356,2	8,8	144,0	5,9	138,1	18 49	13 7	11 0 28	8 9
1747, Sept. 2	513,8	508,2	373,5	13,1	147,8	7,8	140,0	0 29	10 24 47	10 12 34	5 20 15
3	512,3	506,7	373,5	12,9	146,1	7,8	138,3	0 26	24 44	13 34	21 15

N. B. The pen had been drawn through the following computations, which were therefore omitted.

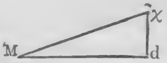
β Draconis	1728, May 7.
α Cass.	1730, Dec. 24.
Capella	1727, Aug. 31.
	1728, Dec. 23.
	1729, Aug. 19.

The blanks were left in the computations for γ Draconis at the end of 1739, and for η Urs. M. at the end for 1730, merely to preserve the symmetry of the pages.

MISCELLANEOUS

ASTRONOMICAL OBSERVATIONS[a].

1715, Oct. 21. 6h. 55′ 20″ Mχ in the short glass$=$31,20 rev. $=$36′ 45″.



χd$=$difference of declination between M and χ in the same glass was 19,60 rev.$=$23′ 23″

χ is supposed to be χ Sagittarii in Bayer. Mars was more northward, and also more westward than χ Sagittarii.

Oct. 29. 6h. Jupiter's diameter$=$1,45 rev.$=$46″.

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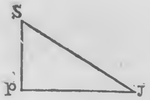
[a] Subsequent to the time of Bradley's becoming Astronomer Royal, the entries made by his assistants have been considered as belonging to him, but with this exception, all the following observations are printed from originals in his own handwriting. They were made previous to May 1732, at Wansted; from that time to the middle of 1742, at Oxford; and afterwards at Greenwich.

The earliest up to March 1718 inclusive were found on a loose half-sheet of paper. After this time till about 1732, the observations were entered in books, made by sewing together some sheets of foolscap; there is likewise a quarto paper book, in which observations at Oxford were entered, between 1734 and 1741: from these, and a large mass of loose papers, the selection was made of what is now laid before the public. In making it, care was taken to omit nothing, excepting in the following particulars. The distances of the planets were sometimes taken from stars which were familiar at the time to the Observer's recollection, but of which (possibly from that reason) he has omitted to specify any description by which they can now be recognised. These it seemed useless to print, unless when the measures from the same star were repeated, and something could therefore be deduced from them with respect to the motion of the planet in the interval. Transits likewise of the sun and some of the fixed stars, which were evidently made merely for the sake of finding the errors of the clock, and which could answer no other purpose, have been likewise omitted in the collection, and it was not thought worth while to have copies made from some rough drawing of configurations.

Some difficulty occurred from Pound's observations having been entered previous to 1725, indiscriminately in the same book with Bradley's. They were constantly working together, and it is not improbable that either of them might have entered what was really the result of their joint labours; but there is now no means of ascertaining in what cases this was or was not so, and it seemed therefore to be most safe to confine the publication to what Bradley had written himself. Fortunately the two handwritings are so different that the separation could be made without much danger of mistake.



1715, Oct. 29. 6h. 45′ 0″ sp. = 30,13 rev. in the long glass, pJ. being the parallel of Jupiter's nearest limb to the star.

7h. 5′ 30″ pJ. = 43,20 rev. being the diff. right asc. between the star and Jupiter's nearest limb.

11′ 15′ sJ. = 53,55 rev. being the dist. of the star from Jupiter's nearest limb.

19′ 0″ sp. = 30,00.

Hence at 7h. 11′ 15″ by the clock, the star was as follows: clock 1′ 40″ too slow for the mean time.

° ′ ″
0 27 14 the distance of the star from Jupiter's centre.
0 15 40 the difference of declination between the star and Jupiter's centre.
0 22 10 the difference of right asc. between the same, measured by the micrometer.

N. B. The star was westward of Jupiter, and nearer the equator than Jupiter.

Nov. 3. 5h. by the clock, the star from the nearest limb of Jupiter was as follows: clock too slow 1′ 30″.

5h. 22′ 0″ the distance of the star and nearest limb of Jupiter = 34,75 rev.
31′ 0″ the difference of declination between the star and nearest limb .... = 10,58 rev.
40′ 0″ the diff. of right asc. between the same.............. = 32,90 rev.

N. B. The star was eastward of Jupiter, and had less declination from the equator.

30. 18h. 6′ by the clock, the difference of declination between Saturn and γ Virginis in the long glass was 49,76 rev. Saturn being more southward than γ.

18h. 11′ Saturn from γ 68,58 rev. their difference of right asc. in time 1′ 33″, Saturn was more westward than the star.

Dec. 4. 16h. 52′ Saturn from γ was in the long glass 61,94 rev.
17h. 2′ their diff. right asc. 21,10 rev. in the same.
25′ their diff. declin. 58,00 rev. γ being more northerly than Saturn.
38′ their diff. right asc. in time 44″, Saturn being more west than γ.
44′ their distance 61,85 rev.
55′ Saturn from γ in the 11 foot glass 45,95 rev.
58′ the same repeated.................... 45,85 rev.

1716, Aug. 12. 14h. 27′ Jupiter was distant from a star visible without a telescope 37,10 rev. in the long glass, the star was more northerly and more easterly than Jupiter. This star is called Propus.

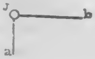
14. 15h. 0′ the diff. of right asc. between Jupiter and the same star was 4″ in time. The diff. of declin. 28,35 rev. long glass: the positions as before.

1716, Aug. 19. 13h. 2′ Jupiter's centre had exactly the same declination with a[b] star whose right asc. in Cat. Britan. in 1690 was 87° 55′ 30″; its declination 28° 28′ 25″; the diff. of right asc. between Jupiter and the star was 3′ 20″ in time. [4][c]

Jupiter was westward of the star.

24. Jupiter's diameter 1,20 rev.

12h. 19′ Jupiter was near the small stars *a* and *b*.



Jupiter's nearest limb and *a* were distant 13,43 rev. its nearest limb and *b* 10,70 rev. Jupiter was eastward and southward of *a*, but westward and northward of *b*. The distance therefore between Jupiter's centre and *a* was 7′ 17″, between *b* and the same was 5′ 54″. The distance between *a* and *b* is 24,50 rev. = 12′ 31″.

26. 12h. 59′ *b* from Jupiter's centre was 9′ 26″, *a* from the same 23′ 29″, Jupiter was eastward of both, and northward from *b*, and southward from *a*.

Venus in 28° of ♉ between 5 and 6 degrees south latitude. Rev. ′ ″
Sept. 10. 16h. 54′ Venus's nearest limb from a small star............42,72 = 21 33
diff. decl. from the same ....41,80 = 21 5
Venus's diameter .... 1,80 = 57
Venus westward and southward from the star.
12. 16h. 41′ The farther limb of Venus from the same star...39,90 = 18 40
50′½ the diff. of decl. between the southern limb of Venus and the star......................................11,22 rev. = 5′ 51″
57′½ the farther limb of Venus from the same star 37,00 = 18′ 42″
17h. 4′ the diff. of right asc. between the centre of Venus and the star was 1′ 10″½ in time.
the diff. of declin. betwixt the southern limb and the star was 24″ in time.

Venus was more southerly and more easterly than the star.

33′ the diameter of Venus was........................ 1,60 rev. = 51″

Nov. 29. 6h. 5′ Jupiter from Propus 41,25 rev.

30. 5h. 41′ Jupiter from Propus 27,20 rev.; diff. declin. 14,70; diff. right asc. 50″½ decl. 33″ in time. By this observation the diff. of right asc. between Jupiter and Propus 12′ 36″, diff. decl. 7′ 36″ of a degree.

Dec. 1. 5h. 53′ the diff. right asc. between Jupiter and Propus was 15″½ in time, Jupiter from Propus 16,25. By this observation the diff. right asc. between Jupiter and Propus was 3′ 50″; diff. decl. 7′ 39″. In all these observ. Jupiter was eastward and southward of Propus.

4. 7h. 0′ diff. right asc. between Jupiter and Propus 22′ 55″; diff. decl. 7′ 47″.

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[b] There is some difficulty here; no star appears in the British Catalogue with exactly this declination. 4 Geminorum is the only one which occurs with a right asc. of 87° 55′ 30″, and its polar distance is 66° 59′ 55″.

[c] See page 208, note [b].

1716, Dec. 5. 6h. 24′ diff. right asc. between the same and Jupiter 31′ 40″; diff. decl. 7′ 50″. In these two last observ. Jupiter was more westward than Propus.

6. 4h. 46′ diff. right asc. between Jupiter and Propus 40′ 10″; diff. decl. 7′ 52″.

7. 4h. 41′ diff. right asc. between Jupiter and Propus 48′ 55″; diff. decl. 7′ 54″.

18. 0h. The sun's vertical diameter was 65,25 rev. in the long glass, the difference of refractions between the upper and lower limb was 7″, which, added to the other, will give the sun's diameter 32′ 46″½ by my Table, by the other 32′ 39″½, at which time the earth was in its perihelion; so that this is his greatest diameter, which Mr. Flamsteed makes 32′ 46″.

1718, March 12. A line passing through α and δ of Virgo was parallel to another passing through the two stars of γ[d].

April 10. 8h. 15′ mean time, Mars was distant from 1κ of Taurus 20,90 rev. in the 10 feet glass, or (allowing for refraction) 15′ 40″, and 25,30 rev. or 18′ 50″ from 1υ Tauri, being to the east of a line passing through them 6,00 rev. or 4′ 51″.

At 8h. 50′ Mars was nearly in a line joining 2κ and 2υ. Hence the longitude of Mars was 2^s 4° 27′ 20″ with north lat. 0° 45′ 55″. 1κ à 1υ=45,80 rev.= 33′ 25″; the micrometer seems to make the distance too small between the stars, so that the other angles ought to be increased in the same proportion.

15, 8h. 40′ mean time, Mars was 47,75 rev.=24′ 0″ in 15 feet glass from τ of Taurus.

30, 8h. 33′ 5″ mean time, the 1st satellite of Jupiter emerged.
May 12, 9h. 11′ 0″ ——— the 3d satellite of Jupiter immerged.
23, 8h. 46′ 0″ ——— the 1st satellite of Jupiter emerged.

An eclipse of the sun observed Feb. 18, 1718.

18h. 54′ 7″	ap. t. the chord between the horns was	18′ 30″
19h. 17′ 0″	—— the chord between the horns was	10′ 18″
19′ 30″	—— the same	8′ 5″
23′ 20″	the end of the eclipse.	

Sept. 4. An application of the moon's north horn to the Bull's eye[e].

19h. 59′ 0″	mean time, Aldebaran from the moon's nearest limb	5′ 38″
20h. 3′ 38″	———————— from the same	4′ 15″

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[d] A loose paper was found with an entry in Pound's handwriting of the distance of Saturn from κ Virginis on the 11th of March 1718; and he has added to it, "The same night the "direction of the double star (viz. γ) of Virgo was parallel to a line through α and δ Virginis." This seems to be the original memorandum from which he copied the same into one of the observation books, and Bradley's observation on the following night is a very valuable addition to it.

[e] This is the observation of which Halley speaks (Ph. T. vol. XXX. p. 858.) Denique coronidis loco observationem adjiciamus, eximiam quidem, suique generis, quod scimus, ab invento telescopio primam; quamque indefessæ D. Jac. Bradley debemus diligentiæ. Quinto enim Septembris mane, Sole jam fere 30 gr. alto, vidit apud Wansted arctissimum Lunæ infra Palilicium transitum.

1718, Sept. 4. 17′ 5″ mean time, again .... 1′ 25″
23′ 9″ ——— rep. .... 0′ 38″
31′ 7″ ——— rep. .... 0′ 18″
33′ 15″ Aldebaran in the line of the horns, being then 13″ distant from the moon's limb.
41′ 0″ Aldebaran from the north horn .... 3′ 42″
42′ 47″ ——— the same .... 4′ 31″
45′ 37″ ——— repeated .... 5′ 36″
58′ 0″ the moon's apparent diameter .... 31′ 7″

The conjunction of Venus and Jupiter.

5. 22h. 57′ 30″ m. t. Venus and Jupiter in the 7 feet glass, Venus being westerly .... 1° 3′ 28″
7. 17h. 21′ 0″ — Venus from Jupiter in the 10 feet glass ... 0° 43′ 18″
34′ 0″ The difference of declination between Venus and Jupiter. Venus more south and east than Jupiter ....[5] 14′ 23″
39′ 0″ Venus from Jupiter .... 44′ 4″

Hence the conjunction was 7th, 0h. 9′; at which time the distance between their centres was 1′ 42″, Venus being more south: than Jupiter.

17. 16h. 51′ 0″ mean time, Jupiter from Cor Leonis .... 24′ 22″
17h. 6′ 20″ difference of declination, Jupiter more north: than Regulus ....[5] 12′ 43″
54′ 0″ Jupiter from Regulus .... 24′ 44″
18h. 7′ 0″ diff. decl. Jupiter more north: and east: ...... 12′ 35″

Hence long. ♌ 26° 11′ 7″; lat. bor. 45′ 39″.

Oct. 17. 17h. 12′ 20″ m. t. the 2d sat. of Jupiter immerged. [5]

18h. 14′ 0″ the distance between Venus and θ of Virgo $17\frac{6}{25}$=12′ 8″, the diff. decl. $11\frac{16}{25}$=8′ 11″; Venus was south: and west: from the star.

Dec. 11. 12h. 12′ 0″ m. t. the shade of the 4th sat. of Jupiter appeared in the middle of Jupiter more northerly than its centre; the centre of the shade moving a little to the north of the northermost belt. The air at the time of the observation was very unconstant, Jupiter appearing sometimes very distinct, and in a minute's time very confused.

13h. 12′ the 1st and 2d sat. were equally distant from Jupiter, being distant from each other about the diameter of either.

14h. 8′ the first edge of the shade of the 4th sat. touched Jupiter's west: limb.

18′ Jupiter's limb appeared free from the shadow. At this time, Jupiter appeared very distinct and clear, and the shade of the satellite was exceeding black and distinct, and seemed, all the time I observed it, just to touch the northermost belt with its southern limb. The 4th satellite appeared very small, the others appearing almost twice as big.

18h. 30′ m. t. Saturn was distant from μ of Libra $61\frac{20}{25}$=28′ 32″; the diff. of declination $9\frac{20}{25}$=4′ 31″, Saturn more northerly than the star, and more east. Hence the difference of longitude 25′⅓, of lat. 13′,0, Saturn being in ♏ 10° 41′ 10″ with 2° 16′ 55″ north latitude.

1718, Dec. 19. 18h. 59′ 50″ mean time, the 4th sat. of Jupiter immerged. [5]

1719, Jan. 2. 11h. 10′ m. t. Jupiter followed ρ Leonis 9′ 35″=2° 24′ 5″, being more north by 49″=12′ 5″. Hence the diff. long. 2° 7′ 36″, and diff. lat. 1° 2′ 8″, Jupiter being in ♍ 4° 35′ 36″ with 1° 9′ 53″ north lat.

16. 18h. 40′ m. t. Mars was distant from ω Scorp. 23′ 10″, diff. decl. 7′ 2″, Mars being more south and east. Hence the diff. long. 22′ 58″, of lat. 3′ 2″, Mars being ♐ 6° 7′ 3″ with 25′ 38″ north lat.

19h. 0′ the diff. of right asc. between Mars and ω Scorpii was 1′ 36″=24′ 4″, the diff. decl. 30″½=7′ 7″. [4] Hence Mars was in ♐ 6° 7′ 26″ with 25′ 36″ north lat.

21. 15h. 42′ 15″ m. t. the 2d sat. immerged. [4]

Feb. 8. 6h. 42′ 45″ the 1st and 4th sat. were in conjunction.

By the observation of Jan. 28, at 10h. the right asc. of Jupiter was 154° 22′ 15″ with south decl. 11° 59′ 10″, whence his place was ♍ 1° 54′ 15″ with 1° 15′ 15″ north lat.

16. 9h. 39′½ the shade of the 4th sat. appeared within, but in contact with Jupiter's eastern limb.

9h. 45′ the 4th sat. appeared in the middle of Jupiter; seeming almost as black as its shade.

11h. 45′ the shade in the middle.

50′ the satellite touched Jupiter's limb inwardly.

12h. 5′ it seemed to touch outwardly.

6′½ it was certainly separated from Jupiter's western limb.

13h. 56′ the shade touched inwardly.

14h. 5′½ the limb of Jupiter appeared free from the shade.

March 5. 7h. 51′ 0″ the shade of the 4th sat. touched the west: limb of Jupiter.

8h. 1′ 30″ limb free from shade.

9h. 52′ 5″ the 2d sat. began to emerge in the long glass, the air being a little hazy, within a minute after I saw it with the 15 feet glass.

6. 8h. 15′ a black spot a little more north: than the north belt appeared in the middle of Jupiter, and very near to it there was another spot in the said belt.

9. 7h. 26′ 0″ a bright spot in the northern belt appeared in the middle of Jupiter, it appeared a little black on the north side of it.

11h. 28′ 45″ mean time, the 3d sat. of Jupiter emerged. [6]

34′ 50″ the 1st sat. touched Jupiter's western limb. Apog.

38′ 20″ the 1st sat. disappeared.

14h. 34′ 10″ the 1st sat. began to emerge out of Jupiter's shade. [5]

10. 8h. 44′ 8″ the 1st sat. touched Jupiter's eastern limb, perig.

49′ 50″ it appeared all within.

9h. 27′ 50″ the shade of the 1st seen entering on Jupiter.

31′ 40″ all within.

1719, March 10. 10h. 58′ 38″ the 1st sat. touched Jupiter's west: limb.
11h. 4′ 18″ separated from the limb.
38′ 40″ the shade of the 1st touched Jupiter's west: limb.
44′ 0″ the limb free from the shade.
13h. 4′ 0″ the bright spot on the northern belt appeared again in mid.
56′ 20″ the 2d sat. touched Jupiter's east: limb.
14h. 2′ 50″ it appeared all within.

Memorand. In these observations allow for the versed sine of the parallax of the earth's orb.

March 9. 11h. 34′ 50″ the 1st sat. touched Jupiter's western limb, being in apogee.
10. 8h. 44′ 8″ it touched Jupiter's eastern limb being in per. so that
in 21h. 9′ 18″ it moved 5^s 29° 57′, its mean motion in the same time being but 5^s 29° 21′, the difference being + 36′.
March 9. 14h. 34′ 10″ the 1st sat. began to emerge out of Jupiter's shade.
and on 10. 11h. 38′ 40″ the first edge of its shade touched the eastern edge of Jupiter.
whence in 21h. 4′ 30″ it moved 6^s 0° 6′ 15″, its mean motion for that time being 1° 20′ 40″ less, whence it may be concluded, that if these inequalities proceed from an excentricity in the satellite's orbit, the perigæon part must be nearer the middle between the two last observations than the two first, the motion of the satellite being swiftest between the last observations. If the place of the apogee be supposed in the beginning of Leo, and the greatest equation one degree, 'twill very well account for these observations.

11. 9h. 2′ 47″ the 1st sat. began to emerge in the long glass. [6]

12. 7h. 10′ 0″ a large black spot in the middle belt appeared near the middle of Jupiter, having past the centre two or three minutes before 'twas observed.

8h. 7′ 45″ the 2d sat. touched Jupiter's west: limb, being in apog.
12h. 26′ 14″ the 2d sat. emerged, a very exact observation. [8]
13. 12h. 50′ 4″ the 4th sat. immerged, [5] a good observation.
16. 15h. 28′ 10″ the 3d sat. emerged. [5]

17. 8h. 45′ the 4th sat. from the east: or nearest limb of Jupiter 8′ 57″ in 15 feet tube, being within two degrees of its utmost elongation.

10h. 32′ 30″ the 1st sat. touched Jupiter's eastern limb. Perig.
36′ 45″ it appeared all within.
18. 10h. 57′ 40″ the 1st sat. began to emerge [7] a very good observation.
27. 7h. 21′ 20″ the 1st sat. emerged.
9h. 5′ 50″ the shade of the 3d touched Jupiter's west: limb.
14′ 0″ the limb free from the shade.

30. 11h. 4′ 0″ the 4th sat. began to emerge out of Jupiter's shade, by the long glass. (123 f.) At the time of its first appearance it was almost in contact with the 2d, being more southerly, and a little nearer Jupiter than the 2d. The distance of their

nearest limbs did not exceed the diameter of either of them. It was seen in the 15 foot glass within less than a minute after the time mentioned before, and 'tis probable the difference would not have been so great, if it had not appeared so exceeding near to the 2d when it began to emerge. The air was very still and clear, and the observation very good. [7]

1719, March 30, the direction of the double star α of Gemini was so nearly parallel to a line through κ and σ of ♊, that after many trials we could scarce determine on which side of σ the line from κ parallel to the line of their direction tended; if on either, 'twas towards β. This observation was made when the air was still, and with the $4\frac{1}{2}$ inch eye-glass, which made the stars appear a good distance from each other[f].

April 6. 9h. 29′ 35″ the 2d sat. began to emerge, by the 15 foot glass, the air being a little hazy.

7. the 4th sat. was seen on Jupiter's disc, seeming very black, as on the 16th of Feb. before. It continued to appear a dark spot quite to Jupiter's limb, which it seemed to touch at 9h. 13′, but the sat. did not appear on the limb till 9h. 21′, and was certainly separated from Jupiter's limb at 9h. 28′.

From this observation, it appears that the part of the sat. that seemed so black whilst on Jupiter's disc was great part of the eastern limb, which reflected the light so faintly, that it could not be seen till the sat. was half out, otherwise it must have made a protuberance on Jupiter's limb soon after the dark spot touched it, but the sat. not beginning to appear till 8′ after, and being separated from the limb in 15′, it follows that the quantity of light reflected from the foremost half of the sat. disc was not sufficient to make it visible, which will easily account for its appearing so black when on Jupiter, and also why it seemed smaller than its shade, as was observed on Feb. 16, when the sat. and shade were seen on Jupiter at the same time.

About 12h. the distance of the 4th sat. of Saturn from its nearest limb 15 rev. $\frac{20}{25}$= 54″,3 the satellite being then in conjunction.

The diameter of Saturn being $5\frac{13}{25}$=19″.

The diameter of the ring $12\frac{23}{25}$=44″,4.

9. 10h. 25′ 15″ the 1st sat. touched Jupiter's east: limb, being in Perig.
31′ 0″ it appeared all within, and continued to be seen all the time till it came beyond Jupiter's centre, and at
11h. 34′ 30″ the shade was seen coming on.
37′ 10″ 'twas all within.
12h. 40′ 20″ the satellite touched Jupiter's west: limb internally.
46′ 0″ it touched externally.

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[f] The following entry has been found in Pound's handwriting: "1718, March 25, the direction of the double star (Castor or) α of Gemini was parallel to a line through Pollux (or β) which left κ to the westward, as also *g* tending to near the middle between *g* and *l* of Gemini."

Another observation of Castor by Bradley was made 1722, Oct. 1.

The greatest diameter of Jupiter was 12 rev. $\frac{8}{25}$=41″,4.

The least 11 rev. $\frac{12}{25}$=38″,5.

The diameter of Saturn 5 rev. $\frac{13}{25}$=19″.

The diameter of the ring 12 rev. $\frac{23}{25}$=44″,4.

1719, April 10. 7h. 41′ 50″ the 1st sat. touched Jupiter's west: limb, being in apog.
46′ 40″ it disappeared.
8h. 57′ 0″ the 3d sat. touched Jupiter's east: limb, being in perigee.
9h. 7′ 0″ it appeared within the disc as a bright spot, but afterwards it appeared dark like the 4th; so that when 'twas about the middle it appeared like the shade of a satellite, but not quite so black. The sat. continued to be seen as a black spot till 'twas near the west: limb, and not bright as when it came on, so that when it was very near the limb, it could not be seen.
11h. 11′ 4″ the 1st sat. seen emerging by the long glass.
11′ 24″ 'twas seen in the 15 f. glass, [7] a very exact observation.
12h. 32′ 0″ the 3d sat. seen coming off Jupiter's disc.
42′ 20″ separated from Jupiter's west: limb.
13h. 41′ 0″ the shade of the 3d sat. seen coming on Jupiter.
48′ 0″ the shade appeared all within; at this time Jupiter was low and did not appear so distinct as before, though better than it often does at the same altitude.

11. 10½h. the diameter of Saturn 5 rev. $\frac{12}{25}$; of the ring 13 $\frac{2}{25}$=45″.
11h. 5′ the 4th sat. of Jupiter from Jupiter's centre 18 rev. $\frac{23}{25}$=8′ 45″, the satellite being in its greatest elongation.

14. 7h. 23′ 30″ the 3d sat. of Jupiter emerged. [5]

21. 7h. 57′ 20″ the 3d sat. immerged, both very exact observations. [9]
11h. 21′ 35″ the 3d emerged.

22. 9h. 41′ 0″ the shade of the 2d seemed to touch Jupiter's west: limb within, the centre of the shade moving along on the north: edge of the northermost belt: about this time a spot in the same belt appeared in the middle.
9h. 45′ 0′ the shade disappeared, Jupiter at this time did not appear well.
10h. 45′ 0″ μ of Libra preceded Saturn 4″½ in time. Saturn was more north: and distant from μ 76 rev. $\frac{14}{25}$=35′ 25″. The diff. of right asc. was taken several times by different threads, and all the observations agreed to 1″. The distance ought to be corrected on account of refraction.

23. the diameter of Saturn 5$\frac{12}{25}$=19″, of the ring 12$\frac{10}{25}$=44″

27. 11h. the 4th sat. of Saturn from Saturn's centre 54$\frac{20}{25}$=3′ 8″,5.

May 6. 11h. the 4th sat. of Jupiter being in its greatest elongation, was distant from Jupiter's centre 17$\frac{12}{25}$=8′ 5″.

7. 9h. 30′ the 3d sat. of Jupiter from the nearest limb of Jupiter 74½ rev. =4′ 16″,3, from the farthest 85$\frac{5}{25}$ rev.=4′ 53″,1.

1719, May 7. the greatest diameter of Jupiter $10\frac{17}{25}$ rev. = 36″,7.
lesser.......................... 10 = 34″,4.

At 9h. 30′ the 3d sat. of Jupiter was 4° 10′ from its greatest elongation, and the distance of it from Jupiter's centre being by those observations 79 rev. $\frac{20}{25}$ = 4′ 34″,51; it follows, that when Jupiter is in its mean distance from the earth, the 3d sat. in its greatest elongation will be 4′ 41″,83 from Jupiter's centre, and the 4th will be 8′ 15″,71.

From the transit of the 3d over Jupiter on the 10th of April, it follows, that the longest semidiameter of Jupiter at the 3d sat. is 3° 53′; at the 4th, 2° 12′ 20″; the 3d being 14,766, and the 4th 25,97 semidiameters of Jupiter from his centre. Hence the equatorial diameter of Jupiter at its mean distance from the earth is 38″,174.

the diameter of Saturn's ring... $12\frac{18}{25}$ rev. = 43″,7.
the inner diameter of the ring ... $8\frac{1}{2}$ = 29″,2.
the diameter of Saturn............ $5\frac{1}{2}$ = 19″.

11h. 40′ the 2d sat. of Saturn was to the north of the line of the ansæ, and a perpendicular to that line from the sat. fell upon the western ansa about $\frac{1}{3}$ of the breadth of the ring from the outward edge. At this time all the satellites were visible, the 1st coming towards its greatest elongation to the west; the 3d to the east; the 4th past his to the west, and the 5th not far from its conjunction with Saturn.

8. 9h. 10′ 0″ the 2d sat. of Jupiter emerged, [8] a very good observation.

16. 9h. 15′ Regulus preceded Jupiter 6′ 17″ in time, Jupiter being more north: than Regulus 41″$\frac{1}{2}$ in time. [6]

18. 8h. 50′ 40″ the 1st sat. touched Jupiter's eastern limb.
55′ 0″ it appeared all within Jupiter's disc.

27. 9h. 52′ the 4th sat. of Jupiter touched Jupiter's east: limb.
10h. 3′ it appeared within.

By the mean of the two observations of the transit of the 1st satellite over Jupiter on the 10th of March and the 9th of April, the 4th sat. is 26,8 semidiameters of Jupiter from Jupiter's centre, and the longest diameter of Jupiter at his mean distance from the earth 37″.

28. 10h. 45′ the 4th sat. of Saturn from the nearest ansa 46 rev. = $\overset{\prime}{2}$ $\overset{\prime\prime}{38}$,2
from Saturn's nearest limb $49\frac{1}{4}$ = 2 49,4
from the farthest ansa......$58\frac{1}{2}$ = 3 21,2
$10\frac{1}{2}$h. the diameter of the ring ....$12\frac{8}{25}$ = 42,4
the inner diameter of the ring .... $8\frac{20}{25}$ = 30,3
the diameter of Saturn .... $5\frac{7}{25}$ = 18,2

11h. 0′ the 3d sat. of Saturn appeared exactly in conjunction with Saturn, being in the superior part of its orb. [5]

29. 10h. the 4th sat. of Saturn from the nearest ansa ......48 rev. = 2 45,1
from the farthest ansa...$60\frac{1}{2}$ = 3 28,1

At the time of this observation, the sat. was a little to the southward of the line of the ansæ, being somewhat past its greatest elongation: on the 28th day it appeared much more to the northward of the line of the ansæ than southward at this time, it then wanting much more of being in its greatest elongation, than 'twas past it now.

1719, June 5. 10h. 52′ the 4th sat. of Jupiter was seen emerging. Jupiter at that time being very near the horizon, and the satellites all appearing dull by reason of the vapours near the horizon.

Rev. ′ ″
6. 10h. 30′ the 4th sat. of Saturn from the farthest ansa... $58\frac{3}{4}$ = 3 22
from the nearest .... $46\frac{1}{4}$ = 2 39
the diameter of the ring ....$12\frac{10}{25}$ = $42\frac{1}{2}$

By the observations made 1719, the greatest elongation of the 4th satellite of Jupiter seen at the sun (Jupiter being at its mean distance therefrom) is as follows:

March 17.	8h. 45′	elong. max.	8′ 15″,0
April 11.	11h. 5′	———	8′ 16″,6
May 6.	11h. 0′	———	8′ 16″,4
7.	9h. 30′	———	8′ 16″,5

The mean of all is 8′ 16″,1. Hence supposing the distance of the 4th sat. from Jupiter's centre to be 26,38 semidiameters of Jupiter, as in my Tables, the greatest diameter of Jupiter at its mean distance from the sun will be 37″,6. But from the several observations of Jupiter's diameter taken with the micrometer in the long glass, the greatest diameter when Jupiter is at its mean distance is 39″,0.

By the transit of the 1st sat. over Jupiter's disc, March 10, 1719, the distance of the 4th sat. from Jupiter is 26,8 semidiameters of Jupiter; and by the transit observed of April 9, 1719, the distance of the 4th is 26,6; and from the mean of these, and the greatest elongation of the 4th before mentioned, the longest diameter of Jupiter at its mean distance from the sun is 37″,1; so that the diameter of Jupiter observed by the micrometer seems too big about 1″½ or 2″.

By the transit of the 3d satellite on April 10, 1719, the distance of the 4th ought to be but 25,7 semidiameters from Jupiter, and the greatest diameter of Jupiter at its mean distance from the sun 38″,6.

From the mean of all the observations, the greatest diameter of Jupiter is to the least (both taken with the micrometer) as 27 to 25, or as 39″ to 36″,1; but if two seconds be subtracted from each upon the account of the light, then the diameters will be as 37″ to 34″,1.

The diameter of Saturn's ring at its mean distance from the sun is, by the observations made in the year 1719, as follows:

		″			″
April 7.	diam. ring	41,3	May 2.	diam. ring	41,5
9.	———	41,3	7.	———	40,9
11.	———	41,8	28.	———	40,4
23.	———	41,0	June 6.	———	41,1

The mean of all is 41″$\frac{1}{4}$. The diameter of the ring is to the diameter of Saturn (both observed by the micrometer) as 7 to 3, therefore the diameter of Saturn, at its mean distance from the sun, is by the micrometer 17″$\frac{3}{4}$.

The greatest elongation of the 4th sat. of Saturn when Saturn is at its mean distance, from the observations made 1719, is as follows:

	′ ″		′ ″
April 27. elong. 4th sat. Saturn	2 55,5	May 29. elong. 4th sat. Saturn	2 58,6
May 28. ————	2 57,4	June 6. ————	2 55,4

The mean of all is 2′ 56″$\frac{3}{4}$, and hence the greatest elongation of the 5th or outermost satellite is 8′ 34″, when Saturn is at its mean distance from the sun.

1721, Jan. 12. About this time the new contrived micrometer was finished.

Mars observed from a small star by the micrometer, in the 15 feet tube, which was moved by a machine that made it keep pace with the stars.

1721, October 14.

Time of the observation by the parlour clock, not correct. H.	′	″	Rev.	Angles ′	″	
7	5	40	$53\frac{16}{40}$	24	42,1	the dist. of Mars from the star.
	17	40	$53\frac{4}{40}$	24	33,8	the same
	36	20	$52\frac{14}{40}$*	24	13	*error forsan $\frac{24}{40}$
	48	0	$52\frac{7}{40}$	24	8,2	
	59	25	$51\frac{29}{40}$	23	55,6	
8	27	0	$50\frac{29}{40}$	23	27,8	
	33	20	$50\frac{25}{40}$	23	25	
	43	0	$50\frac{14}{40}$	23	17,4	
	45	30	$50\frac{9}{40}$	23	13,9	
	55	30	$50\frac{1}{40}$	23	8,4	

9 5 0 $2\frac{17}{40}$ 1 7,3 the diff. of declination, Mars being more south: than star.

9 46 46 9h. 15′ 0″ by the chamber clock, which was too slow 12′ 35″ for the mean time.

16 28 0 $4\frac{7}{40}$ rev. 1′ 56″ diff. of decl. Mars more southerly than the star.

right asc. of this star 43° 42′$\frac{1}{2}$
s. declination ....15° 36′$\frac{1}{2}$

H.	′	″	Rev.	′	″	
16	44	10	$35\frac{5}{40}$	16	15	distance
	58	0	$34\frac{31}{40}$	16	5,2	
17	9	35	$34\frac{13}{40}$	15	52,7	
	19	20	$34\frac{7}{40}$	15	48,6	
	24	30	34	15	43,7	
	29	0	$33\frac{38}{40}$	15	42,3	
	33	30	$33\frac{25}{40}$	15	33,2	
	41	0	$33\frac{21}{40}$	15	30,5	
	45	40	$33\frac{14}{40}$	15	25,6	
	49	55	$33\frac{9}{40}$	15	22,1	
	52	35	$33\frac{5}{40}$	15	19,4	

18 2 0 $4\frac{25}{49}$ 2 8,3 diff. of declin.

21 5 18 20h. 33′ by the chamber clock, which was then too slow, 12′ 34″ for the mean time, and therefore the parlour clock was 19′ 44″ too fast for the mean time.

Mars observed from the same star as on the 14th.

Time by the parlour clock not correct. H.	′	″	Rev.	′	″	
15. 7	2	30	$7\frac{20}{40}$	3	28	the diff. of decl.
	25	0	$8\frac{26}{40}$	4	0	the diff. of

right asc. in the parallel taken by the microm.

H. ′ ″	Rev.	′ ″	
7 34 15	$8\frac{23}{40}$	3 58	the same repeated.
39 54	$8\frac{13}{40}$	3 51	
45 37	$8\frac{4}{40}$	3 44,8	
54 17	$7\frac{35}{40}$	3 38,6	
8 1 30	$7\frac{27}{40}$	3 33	
3 30	$7\frac{22}{40}$	3 30	
16 30	$7\frac{7}{40}$	3 19,2	
26 10	$6\frac{32}{40}$	3 8,7	
32 17	$6\frac{26}{40}$	3 4,5	
41 0	$6\frac{11}{40}$	2 54,1	
47 10	$6\frac{7}{40}$	2 51,4	
57 0	$8\frac{2}{40}$	3 43,4	the diff. of decl.

9 40 55 9h. 8′ by the chamber clock, at that time slower than the mean time 12′ 32″.

Mars observed from another small star. The diff. of right asc. were taken by a chronometer that beat ¼ seconds, and which went exactly sidereal time.

1721, Oct. 20.

The right asc. of star 41° 36′.

Time by the parlour clock, not correct. H. ′ ″	″	
8 29 0	$81\frac{1}{4}$	in time, the diff. of right asc. between Mars and the star.
34 30	$81\frac{1}{8}$	
40 0	$81\frac{1}{8}$	
46 0	$80\frac{3}{4}$	
50 45	$80\frac{1}{2}$	
56 45	$80\frac{3}{8}$	
9 0 45	$79\frac{5}{8}$	
5 45	$79\frac{1}{2}$	
9 10 30	$79\frac{1}{4}$	
18 30	$78\frac{5}{8}$	
22 30	$78\frac{3}{4}$	
27 0	$78\frac{1}{8}$	
34 30	$77\frac{3}{8}$	
42 0	77	
45 0	$76\frac{1}{2}$	
50 0	$76\frac{1}{2}$	
17 15 15	$47\frac{1}{2}$	
20 0	$47\frac{3}{8}$	
23 50	$46\frac{3}{4}$	
31 20	$46\frac{1}{8}$	
34 20	$46\frac{1}{8}$	
38 40	$45\frac{3}{4}$	
41 50	$45\frac{3}{4}$	
48 0	$45\frac{1}{4}$	
50 15	$45\frac{1}{4}$	
18 1 15	$44\frac{3}{8}$	
3 45	$44\frac{1}{4}$	
9 30	44	

21 8 17 20h. 29′ by the chamber clock, which was too slow for the m. t. 12′ 20″.

The parlour clock gained 1″¼ per diem.

Mars observed from another small star.

Oct. 25.

Right asc. of the star……39° 58′
South declination ………15° 9′

Time by the parlour clock, not correct. H. ′ ″	″	
5 54 30	$35\frac{3}{4}$	diff. right asc. in seconds.
56 0	$35\frac{3}{8}$	
57 49	$35\frac{1}{2}$	
6 0 0	$35\frac{1}{4}$	
3 30	$34\frac{7}{8}$	
6 15	$34\frac{3}{4}$	
7 42	$34\frac{5}{8}$	

H.	′	″	″
6	9	27	34¾
	10	56	34⅝
	12	13	34½
	14	17	34¼
	15	34	34¼
	17	30	34⅛
	18	38	34
	20	14	34
	23	8	33½
	25	8	33½
	26	35	33⅜
	28	18	33⅜
	30	38	32¾
	33	0	32½
	35	6	32½
7	7	30	the star had the same decl. with the south limb.
	12	0	30¾
	13	13	30¾
	14	50	30¾
	16	0	30⅜
	17	20	30⅜
	18	36	30
	21	16	29¾
	23	0	30
8	8	8	7h. 40′ by the chamber clock.
14	42	0	2
	48	40	1⅞
	49	40	1⅝
	50	15	1¼
	51	30	1⅜
	53	0	1¼
	55	27	1¼
15	0	0	the west limb of Mars had the same right asc.
	9	50	Mars and the star the same right asc.
	16	0	the diff. of decl. 2,0 rev. = 55″½ Mars south.
15	24	0	the east limb the same right asc. with the star.
16	55	0	6″½ the star eastward.
	57	10	6¾
	59	0	6¾
17	1	40	6⅞
	3	50	7
	6	40	7⅜
	9	7	7⅝
	10	14	7¼
	11	10	7¼
	15	9	7¾
	16	9	8
	17	16	7⅞
	18	8	8⅛
	20	15	8
	21	10	8¼
	22	24	8
	23	37	8¼
	25		8¼
	25	53	8½
	27	12	8¼
	28	0	8¾
	28	50	8½
	30	30	8½
	31	20	9
	32	20	9¼
	34	0	9¼
	34	56	9⅜
	36	9	9
	39	40	9
	41	0	9½
	42	0	9½
	42	52	9½
	43	55	9½
	44	50	10⅛
	45	46	9¾
	46	43	10
	47	42	9¾
	48	38	10

H.	′	″	″
17	49	36	10⅛
	50	38	10
	51	24	10¼
	52	10	10⅛
19	42	21	19h. 14′ by the chamber clock,

which was 12′ 8″ too slow for the mean time.

Parlour clock gains 29″ per diem of the mean time[g].

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[g] There are some similar observations of Mars made in Aug. 1719, which are mentioned by Halley. Ph. Tr. vol. XXX. p. 1111. They have not been inserted in the text, because they are all entered in Pound's handwriting; but they are here subjoined, as it is certain that Bradley had some share in making them. See Ph. Tr. vol. XXXI. p. 114.

1719, Aug. 14.

	H.	′	″	H.	′	″
M. T.	9	54	13¾	14	44	28⅜
		55½	13⅞		48	28⅝
		57	13¾		49½	28½
	10	1	14		51½	28⅝
		4	14⅜		54½	29
		7½	14⅞		56	29¼
		9½	14¾		57½	29
		13	14⅞	15	5	29
		15	15		9	29¼
		16½	15⅛		10½	29¾
					11½	29⅜

The second and fourth columns are the differences of the right ascension in time, between Mars and a small telescopic star whose declination was about 10′¼ greater than that of Mars at 10½h.

The star being very small it could not be observed very exactly.

The time-keeper, by which these observations were made, lost about one second in a quarter of an hour m. time.

Aug. 17.

H.	′	′	″	H.	′	′	″
8	56	2	50⅝	10	11	2	47
9	4		50⅜		17		46⅞
	12		50½	14	9		35¼
	19		49⅞		37		33⅞
	25		49⅝		48		33½
	31		49¼	15	1		33
	40		48½		7		32½
	46		48½		12		32½
	51		48¼		24		32
	56		48¼		31		31¼
10	4		47⅝				

This night Mars was observed from the southernmost of two small stars.

At 10h. 27′½ the diff. of declination between Mars and that star was 32″¾ by the clock; Mars being more southward and more eastward.

The diff. of right asc. between the two stars 4″⅝ by the clock.

Their diff. of decl. 15″⅞ by the clock.

The southermost the more easterly.

To Aug. 14, Bradley has annexed the following memorandum:

"By taking the mean of these observations, as they are here connected, and supposing the "motion of Mars in right asc. to be 15′ 52″,7 per diem, the parallax of the sun comes out "11″,52." He calculated also the later observations, and the results which he brought out from them, are thus summed up at the end of the paper.

	″
"1721, Oct. 14 and 15, sun's parallax	9,3
20. ———	11,5
25. ———	10,2
	31,0
Mean of all	10,⅓"

1722, June 11. 12h. 22′ 45″ the 2d sat. of Jupiter began to emerge in 123 f. glass; a very exact obs.

23′ 15″ the 2d sat. seen emerging in the 15 f. tube.

The air was serene and quiet, and Jupiter appeared very distinct and well.

14. 9h. 45′ 0″ the shade of the 3d sat. of Jupiter all within the limb coming on.

50′ 0″ the 4th sat. of Jupiter in conjunction with Jupiter (in apog.)

10h. 45′ 0″ the 1st sat. began to emerge in the Hugenian glass, and was seen at the same time by Mr. Hadley's reflecting telescope, [7] a very exact observation: the air being serene and quiet.

11h. 46′ 0″ the shade of the 3d sat. in contact with Jupiter's limb going off: the air was now very unconstant, and Jupiter's limb did not appear well defined by reason of the undulation of the vapours, so that this last observation is not much to be depended on.

19. the greatest diameter of Saturn's ring $12\frac{1}{2}$ rev.; the least $5\frac{20}{25}$ rev.

July 7. 10h. 59′ 28″ the 1st sat. began to emerge in the reflecting telescope, and 18″ after it was seen in the 15 foot telescope. At the time of the emersion Jupiter was low, and did not appear well by reason of the vapours, but the observation is sufficiently exact. [8]

12. 9h. 30′ 0″ the difference of right asc. between Jupiter and κ of Libra, observed by the chronometer, was 10′ 35″$\frac{1}{2}$ sidereal time=2° 38′ 52″ right asc.; the difference of declination by the micrometer $18\frac{20}{40}$=8′ 34″, Jupiter being more northerly and east: than the star.

23. 9h. 19′ 10″ the first sat. of Jupiter began to emerge in the $15\frac{1}{4}$ tube, and was seen emerging at the same time in the reflector. A very exact observation, the air being serene and quiet.

Aug. 21. 7h. 30′ the diff. of right asc. between Jupiter and β Scorpii 3′ 26″ time=53′ 30″; the diff. of declin. by the micrometer, corrected by refract. 33 rev.$\frac{34}{40}$= 15′ 40″. [7] Jupiter was more south: and west: from the star, which is the same he was observed from May 27. Hence the right asc. of Jupiter is 236° 28′ 5″; declin. s. 19° 16′ 40″.

Oct. 1. A line through the double star α of Gemini was parallel to another drawn through β and κ. The southermost star is brightest.

7. 5h. 48′ difference of declin. between Jupiter and ω Scorpii 3 rev. $\frac{23}{40}$ =1′ 39″, Jupiter more southerly than the star.

Distance between Jupiter and ω 32 rev. $\frac{4}{40}$=14′ 51″, Jupiter more easterly. [6]

Dec. 27. 5h. the distance between the horns of Venus was observed 29 rev.= 57,8 in a telescope of $212\frac{1}{4}$ f.—m. diam. 19″,04.

1723, April 16. 14h. 28′$\frac{1}{2}$ the 2d sat. of Jupiter immerged by the reflector. The air was hazy, and Jupiter low, but the observation, I believe, may be depended upon within the minute.

1723, April 14. 15h. 0′ Jupiter was distant from the westermost of two stars in the eye of Sagittarius 47 rev. 0=21′ 43″, and more north 8 rev. $\frac{30}{40}$=4′ 3″; alt. of Jupiter 13°; par. ∠ 12°; at the same time Jupiter was about the breadth of the thread, or 4″ to the southward of the following of the two stars.

April 23. 14h. 54′ 5″ the 1st sat. of Jupiter immerged by the reflector; the air was pretty clear: the sat. appeared much smaller than the 2d which was on the same side of Jupiter, about a diameter of Jupiter from the 1st.

June 1. 11h. the 2d, 3d, and 4th sat. of Saturn were seen through Mr. Hadley's reflector. In appearance the 2d was eastward of Saturn, and almost in a line with the ansæ; the 3d to the westward of Saturn, and below the line of the ansæ; the innermost was not seen, but the 2d and 3d might be seen even when Saturn was in the glass at the same time. The moon shined very bright, being about 9 days old.

10h. 30′ 40″ the 4th sat. of Jupiter immerged by the 15 f. glass, the air serene and good. [6]

13h. 17′ 55″ the 1st sat. immerged [6] in the 15 f. glass. Jupiter appeared exceeding distinct and well.

July 7. 11h. 9′ 15″ the 2d sat. of Jupiter emerged [7] in the 15 f. glass.

Sept. 2. 7h. 58′ 0″ the 2d sat. emerged in the 15 f. tube.

Oct. 21. 5h. 52′ 45″ the 1st sat. emerged in the 15 f. tube.

29. 2h. 25′ 35″ mean time, about half of Mercury was entered on the sun's disc, seen in the 15 f. tube.

26′ 45″ m. t. or at 2h. 42′ 39″ ap. t. was the total immersion of Mercury seen in the Hugenian telescope.

The diameter of Mercury observed in that telescope while he was on the sun's disc, was by the micrometer 3 rev. $\frac{3}{40}$=10″$\frac{3}{4}$.

33′ 4″ the diff. of declin. between the south limb of Mercury
and the south limb of the sun..........................33 rev. $\frac{6}{40}$=15′ 19″
48′ 38″ the same repeated 34 $\frac{11}{40}$=15′ 50″
3h. 4′ 12″ .... 35 $\frac{28}{40}$=16′ 30″[h]

30. 5h. 4′ 30″ the 4th sat. immerged in 15 f. glass. Twilight strong.

1724, March 19. 16h. 25′ Jupiter had exactly the same declination with the 19th star of Capricorn in the Brit. Cat. and preceded it in right asc. 1′ 6″$\frac{1}{2}$ by the clock=16′ 39″. [8]

June 25. 11h. 0′ m. t. Saturn was distant from a small star in the 15 f.
tube ....6 rev. $\frac{24}{40}$=3′ 3″
Saturn more northerly in decl. ....0 $\frac{16}{40}$= 11″
26h. 10′ 0″ Saturn was distant from the same star ....2 $\frac{21}{40}$=1′ 10″
Saturn more south ....0 $\frac{12}{40}$= 8″
11′ 5″ Saturn was distant from the star in the 123 f. telescope 23 rev.

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[h] The three first entries on the 29th of Oct. are written by Pound, the three last by Bradley; this is the observation referred to by Halley, Ph. Tr. vol. XXXIII. p. 229.

$\frac{5}{25}=1' 20''$, and had then exactly the same altitude with the star, for I waited till I judged that the line passing through the star and Saturn was parallel to the horizon, so that this observation may be more depended on than those made with the shorter tube. The motion of Saturn at this time was retrogade, and therefore at the time of the first observation he was to the eastward, and the night following to the west of the star, and was in conjunction with it about 26 4h. 20′, when Saturn's centre passed about 2″ more southerly than the star; so that had the occultation happened in the night, it might have been a proper observation for determining the exact diameter of Saturn, &c.

1724, July 24. 11h. 31′ 20″ m. t. the 4th sat. began to emerge in the reflector. [9]

Sept. 7. 8h. 31′ 10″ the 1st sat. began to emerge in the reflector. [8]

27. 7h. 5′ 15″ the 2d began to emerge in the reflect. [6]

1725, Observations made at my aunt Pound's house in Wansted town, which is
about 1′ 45″=7″ time more east than the observatory at Greenwich, and
5′ 53″ more northerly than the observatory, as I found by
measuring the distance, &c.[i]

July 24. 14h. 49′ 43″ m. t. the 1st sat. of Jupiter immerged in the reflect. (Mr. Had.)

Aug. 18. 9h. 27′ 25″ the 1st sat. of Jupiter immerged by an 8 f. reflect. at Kew, diff. mer. 1′ 15″.

27. 9h. 15′ 45″ the 2d sat. of Jupiter was seen emerging very near the limb of Jupiter in the reflector; the satellite came out nearer to Jupiter than I expected, so that 'tis probable I might have seen it sooner if I had known exactly where to have looked.

Sept. 16. 6h. 55′ 30″ m. t. the 4th sat. of Jupiter immerged in Mr. Hadley's reflector. [7]

10h. 32′ 40″ the 4th sat. of Jupiter emerged in the reflect. [7]

The satellite appeared very small both before it immerged and after the emersion; not half so big as either of the others, which probably is the reason why the duration was so long, it being 18′ longer than it ought by the Tables. The middle of the eclipse agrees within 2′½ with the Tables. There was a little haziness about Jupiter at the time of the emersion, but not so much as to occasion much difference in the time of seeing the satellite, so that the observation may be depended upon within the minute, it being made with all the care and caution I could, because I thought that I had mistaken 20′ in the calculation.

Sept. 17. 13h. 48′ 40″ m. t. the 1st sat. of Jupiter emerged in Mr. Hadley's reflector. [6]

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[i] In the library of the observatory at Greenwich there is a paper book, in which Bradley has entered these quantities, about 1750, a little more minutely; he there makes the difference of time 6″,85, and the difference of latitude 5′ 51″¾.

1725, Oct. 3. 12h. 7′ 20″ the 1st sat. of Jupiter emerged in Mr. Hadley's reflector. [6]

5. 6h. 35′ 48″ the 1st sat. of Jupiter emerged in Mr. Hadley's reflector. [6]

11h. 44′ 50″ the 2d sat. of Jupiter emerged in Mr. Hadley's reflector. [8]

10. Moon eclipsed total. Clouds hindered me from seeing the moon till after her emersion out of total darkness, but

7h. 38′ 20″ m. t. I observed the enlightened part in 15f. glass 15 rev. $\frac{20}{40}$=7′ 10″.

41′ 20″ I observed the same again 17 rev. $\frac{20}{40}$=8′ 5″.

By the first of these observations the emersion was at 7h. 25′ 40″
by the second......7h. 26′ 50″

eq. t.+15′ 20″

23. 6h. 21′ 10″ the 2d sat. of Jupiter seen emerging in the 15 f. tube through a thin cloud, so that probably it might have been seen $\frac{1}{2}$ minute sooner if the air had been clear.

Nov. 27. 9h. 2′ 45″ the 1st sat. of Jupiter emerged in 15 f. glass [6] air a little hazy.

1726, Jan. 5. 6h. 38′ 30″ mean time, the 3d sat. of Jupiter began to emerge in 15 f. tube. [9]

7h. 38′ 55″ the 1st sat. of Jupiter began to emerge in 15f. tube. [7]

An occultation of Mars by the moon observed.

7. 7h. 20′ 17″ m. t. the first contact of the limb of the moon and Mars.
20′ 42″ Mars wholly hid.

8h. 14′ 15″ Mars was emerged from behind the moon, and its nearest limb was about one of his diameters from the moon's limb; so that it was wholly separated from the moon at about 8h 13′ 50″. But the exact time of its emerging I did not observe by reason of clouds.

8h. 23′ 25″ diff. declin. of Mars and moon's northern limb 15 rev. $\frac{34}{40}$=7′ 20″
31′ 45″ the same repeated..................................13 $\frac{19}{40}$=6′ 14″
35′ 10″ the same ....12 $\frac{14}{40}$=5′ 43″
38′ 35″ the same, Mars more southerly than moon's limb ....11 $\frac{15}{40}$=5′ 15″

9h. 25′ 0″ the moon's diameter 73,0 rev.=33′ 44″; alt. 39° ∠ 37°.

15. 8h. 22′ 0″ Mars preceded ξ Cancri 14′ 21″$\frac{2}{3}$ in m. t.=3° 36′ 0′ right asc. and was more southerly than ξ 0 rev. $\frac{33}{40}$ in 7 f. tube=48″ [7] Mars was this day in opposition to the sun.

Mem. the diff. of decl. was taken with the same thread.

1726, Aug. 5. 14h. 49′ 50″ m. t. the 1st sat. of Jupiter immerged in Mr. Hadley's reflec.: the air was very serene, and Jupiter appeared very distinct, and the observation was very good. [8]

14. 11h. 12′ 5″ the 1st sat. of Jupiter immerged in Mr. Hadley's reflec.: [7] air very good.

15. 10h. 56′ 35″ I saw the 3rd sat. emerging in a 14f. tube at Greenwich, air very good, and observation good. [7]

22. 12h. 33′ 5″ m. t. the 3d sat. of Jupiter immerged in Mr. Hadley's reflec.

14h. 55′ 20″ the 3d sat. of Jupiter began to emerge in the reflector: the air very good, and the observation exact; the satellite being seen, when it appeared, exceeding small, and Jupiter appearing very distinct and well defined. [7]

28. 8h. 53′ 20″ the 2d sat. of Jupiter immerged in the refl. Jupiter being low. [5]

15h. 0′ 5″ the 1st sat. of Jupiter immerged in refl.: air very good. [5]

Aug. 29. 16h. 35′ 30″ the 3d sat. of Jupiter immerged in the ref.: air very good [7] a small twilight.

Sept. 2. 10h. 15′ 0″ mean time, Jupiter preceded o in lino boreo Piscium 50″$\frac{1}{2}$ in time=12′ 40″ in right ascension, and his centre was 1 rev. $\frac{32}{40}$=0′ 50″ more southerly than the star. This observation was made with the 15f. tube, with great care and exactness, and may be depended upon; the diff. of declination being taken by the same thread, and therefore not depending on the threads being truly adjusted to the beginning of the divisions.

11. 14h. 8′ 0″ the 2d sat. of Jupiter was immerged. I saw it 4′ before this, but the exact time of the immersion could not be observed for clouds.

12. 12h. 10′ 0″ Jupiter preceded a small telescopic star 1″$\frac{1}{4}$ in time, or 20″ in right asc., and his centre was more south: than the star 2 rev. $\frac{28}{40}$=1′ 15″. The next night at 10h. 40′ m. t. Jupiter preceded the star 26″$\frac{1}{4}$ in time, and was more southerly 8 $\frac{8}{40}$=3′ 47″; this diff. decl. was taken by the same thread.

[8] Hence Jupiter had exactly the same right ascension as the star, Sept. 12. 11h. 0′ m. t. being then 1′ 7″ more southerly than the star; I take this deduction to be very exact.

13. 13h. 17′ 10″ m. t. the 1st sat. of Jupiter immerged by the reflector, [6] a very good observation; Jupiter appearing distinct. At the time of the immersion, the 2d sat. appeared equally distant from Jupiter, but was really more southerly, near a semidiameter of Jupiter.

An eclipse of the sun observed at Wansted, Sept. 14, 1726, in a 15f. tube.

14. 3h. 47′ m. t. the sun's diameter between the threads lying parallel to the apparent motion of the sun 69 rev. $\frac{4}{40}$=31′ 56′. This observation corrected, by allowing for refraction, makes the sun's diameter 32′ 0″, that is, 7″ or 8″ less than Dr. H.'s Tables; by another observation taken about $\frac{1}{4}$ hour after this, the sun's diameter

was 10″ or 12″ less than the Tables. After this, I set the threads of the micrometer so as to make an angle with a verticle circle of 57°, by which means I knew exactly on which part of the sun's limb the eclipse would begin, and keeping my eye on that part, I perceived the first contact of the moon's and sun's limbs, or the beginning of the eclipse.

1726, Sept. 14. 4h. 12′ 27″ beginning. [8]
15′ 20″ the chord between the horns.........19 rev. $\frac{15}{40}$= 8′ 57″
16′ 30″ the same repeated ....22 $\frac{36}{40}$=10′ 35″
17′ 30″ the same.................................25 $\frac{14}{40}$=11′ 43″
19′ 10″ the moon's limb touched the middle of the first spot in the sun. [5]
20′ 29″ the moon's limb touched the middle of the second or biggest spot. [5]
21′ 56″ the chord between the horns ......33 rev. $\frac{38}{40}$=15′ 41″
22′ 57″ the moon's limb touched the middle of the third spot. [6]
23′ 6″ the moon's limb touched the middle of the fourth and last spot. [6]
24′ 40″ the chord between the horns.........37 rev. $\frac{36}{40}$=17′ 31″
26′ 17″ the same repeated ....39 $\frac{32}{40}$=18′ 24″
29′ 27″ the same.................................43 $\frac{27}{40}$=20′ 11″
31′ 42″ the same.................................45 $\frac{33}{40}$=21′ 11″
34′ 30″ the same repeated ....47 rev. $\frac{15}{40}$=21′ 54″
then cloudy weather.
48′ 0″ the enlightened part of the sun's diameter ....39 $\frac{31}{40}$=18′ 23″
49′ 45″ the same repeated ....38 $\frac{27}{40}$=17′ 25″
54′ 10″ the same ....36 $\frac{17}{40}$=16′ 50″
57′ 7″ the same repeated [5] ....35 $\frac{8}{40}$=16′ 16″

Then clouds and trees prevented me from making any more observations, the sun setting behind clouds.

15. 7h. 45′ 20″ the 1st sat. of Jupiter immerged by the refl. Jupiter was low and did not appear well, but I believe the observation is pretty good. [5]

30. Sun's alt. 31°$\frac{1}{2}$; sun's diam. equat. 69 rev. $\frac{6}{40}$, the outside of the threads.

By the mean of 23 observations of the sun's diameter, the biggest 2′ 10″$\frac{1}{8}$, least 2′ 9″$\frac{1}{2}$, the mean of all was 2′ 9″$\frac{37}{46}$; the timekeeper gains 2″$\frac{1}{4}$ in 17′ mean time of m. t.

By this day's observation, the diameter of the sun, corrected by refraction, was by the micrometer 32′ 8″.

By the timekeeper, the sun's diameter passed in 2′ 9″$\frac{37}{46}$, that is, in mean time 2′ 9″$\frac{12}{23}$=32′ 22″,8 in the parallel of declination. Hence the sun's true diameter was 32′ 8″.

Oct. 1. The mean of all taken this day with the micrometer is 69 rev. $\frac{18}{40}$=

32′ 6″, to which adding 2″ for refraction, then the true diameter is 32′ 8″, exactly agreeing with the mean of all taken by the chronometer[k].

1726, Oct. 8. 10h. 5′ 30″ m. t. the 1st sat. of Jupiter emerged in the refl.: this observation is not so exact as might be wished, but I believe may be safely depended on to ½ of a minute.

Nov. 23. 10h. 32′ 45″ the 1st sat. emerged in the 15f. tube; it was coming out before I came to the glass, which was 33′ 10″ mean time; but from its smallness, and the nearness of the moon, I judge the true emersion was 25″ or 30″ sooner than I saw it.

25. 5h. 1′ 10″ the 1st sat. of Jupiter began to emerge in the 15f. tube; [10] very good.

8h. 21′ 0″ the 2d sat. of Jupiter began to emerge in 15f. tube, [8] there were flying clouds, but at the time of the emersion Jupiter appeared very bright and well.

Dec. 2. 6h. 56′ 20″ the 1st sat. began to emerge in 15f. tube, being seen when it appeared exceeding small; a very exact observation. [10]

10h. 59′ 10″ the 2d sat. began to emerge in 15f. tube; a very exact observation, Jupiter appearing very distinct, &c. [8]

15. 7h. 11′ 10″ the 3d sat. of Jupiter began to emerge in the 15f. tube: the air was serene, and the observation very good. [7]

25. 7h. 11′ 40″ the 1st sat. of Jupiter began to emerge in the 15f. tube, being first seen when it appeared very small. Jupiter appeared distinct and well, and the observation is very exact. [8]

1727, Jan. 1. 9h. 7′ 20″ the 1st sat. of Jupiter began to emerge in the 15f. tube; air clear, and the observation very exact. [10]

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[k] There were 9 measures taken with the micrometer about noon.

The observations of the time which the sun took up in passing the wire of the micrometer were more numerous, and their general results as follow:

8h. 30′ to 10h. 44′ aperture 2 inches; 33 observations with 3 inch eyeglass: extremes 2′ 9″⅝ and 2′ 10″½.

10h. 52′ to 11h. 1′ —————— 4 obs. with 4½ inch eyeglass: extremes 2′ 9″¾ and 2 10″¼.

"The mean of all these is 2′ 10″$\frac{1}{16}$; hence the sun's true diameter 32′ 10″½."

0h. 41′ to 0h. 59″ 3 inch eyeglass 7 obs. with a small aperture: extremes 2′ 9″½ and 2′ 10″.

1h. 5′ to 1h. 20″ —————— 5 obs. with 1,4 inch aperture: extremes 2′ 9″⅝ and 2′ 10″.

"The mean of these is 2′ 9″¾; hence the sun's diameter is 32′ 6″."

1h. 27′ to 1h. 45″ —————— 7 obs. with 2 inch aperture: extremes 2′ 9″¾ and 2′ 10″¼.

"The mean 2′ 9″$\frac{10}{11}$; hence the sun's diameter 32′ 8″¼.

"Chronometer gains of mean time 2″$\frac{3}{16}$ in 17′."

1727, Jan. 3. 10h. 54′ 5″ the 2d sat. of Jupiter began to emerge in the 15f. tube; air clear, but Jupiter pretty low; observ. good. [7]

10. 5h. 31′ 35″ emersion of the 1st sat. of Jupiter; a small twilight, but I believe the observation is pretty good. [6]

Feb. 9. 7h. 43′ 10″ the 1st sat. began to emerge in the 15f. tube; air clear, and the observation very good. [7]

16. 10h.$\frac{1}{2}$ a nebulos: about 4° to the south of Sirius contiguous to a small star.

Another near the middle of an isosceles triangle formed by three stars, the brightest and northermost has two small ones contiguous, being in a line with it, the equal and longest sides of the triangle are about 5°$\frac{1}{2}$. The stars lie almost in the midway between Procyon and Cor Hydræ.

April 15. I took the sun's diameter with the micrometer in the 15f. tube, and found it 68rev. $\frac{29}{40}$=31′ 45″,7, to which add 0,8 for refraction, and then the true diameter was 31′ 46″,5: hence the mean diameter is 32′ 1″ exact.

July 2. 14h. 24′ 20″ the 1st sat. of Jupiter immerged in the 15f. tube, a small twilight and moon: observ. good. [5]

Aug. 4. 14h. 16′ 10″ the 2d sat. of Jupiter immerged in the 15f. tube: observation very good. [6]

10. 12h. 52′ 35″ the 1st sat. of Jupiter immerged in the 15f. tube: obser. very good. [5]

Sept. 4. manè. An eclipse of the sun observed in part.

3. 18h. 17′ 0″ mean time, the eclipse was begun, when I first looked at the sun.

20′ 40″	the chord between the horns	20rev.	$\frac{33}{40}$= 9′ 37″,5
21′ 55″	the same	23	$\frac{20}{40}$=10′ 51″,7
23′ 30″	the same	26	$\frac{26}{40}$=12′ 19″,0
24′ 20″	the same	28	$\frac{4}{40}$=12′ 59″,2
26′ 5″	the same	30	$\frac{16}{40}$=14′ 3″,0
	then clouds till		
33′ 30″	the same repeated	38	$\frac{17}{40}$=17′ 45″,5
36′ 0″	the same	40	0=18′ 29″,2
37′ 10″	the same	41	0=18′ 56″,9
About this time the line of the horns was vertical.			
40′ 30″	the chord between the horns	42	$\frac{30}{40}$=19′ 45″,5
42′ 30″	the same	43	$\frac{25}{40}$=20′ 9″,7
47′ 30″	the same	45	$\frac{10}{40}$=20′ 54″,7
48′ 50″	the same	45	$\frac{33}{40}$=21′ 10″,7
50′ 10″	the same	46	0=21′ 15″,6

eq. temp. + 4′ 46″

Then clouds wholly intercepted the sight of the sun till the eclipse was ended.

1727, Oct. 20. 7h. 52′ 15″ the 1st sat. of Jupiter immerged in the 15f. tube, [6] moon very bright and near Jupiter.

Dec. 5. 10h. 24′ 15″ the 1st sat. of Jupiter began to emerge in the refl. [10] good observ.

21. 7h. 40′ 50″ m. t. the 2d sat. emerg. in the refl. [7]
8h. 43′ 0″ the 1st sat. emerged in the refl. [9]

1728, Feb. 5. 9h. 11′ 55″ the 1st sat. of Jupiter emerged in the 15f. tube, [9] air very clear.

18. 8h. 16′ 0″ the 3d sat. of Jupiter emerged in the 15f. tube. [7]

March 22. 9h. 42′ 0″ the 1st sat. of Jupiter emerged in 15f. tube. [8]

Aug. 12. 16h. 14′ 30″ the 1st sat. of Jupiter immerged in 15f. tube, [7] a strong twilight and moon.

Oct. 15. 9h. 19′ 20″ the 1st sat. of Jupiter immerged in the 15f. tube, [9] a good observation.

1729, Jan. 24. 8h. 40′ 40″ the 1st sat. of Jupiter emer. in Mr. Hadley's refl. [11] v. g.

27. 8h. 49′ 20″ the 3d sat. of Jupiter emerg. in Mr. Hadley's refl. [8]

A total eclipse of the moon observed at Wansted, Feb. 2, 1729.

Feb. 2. 7h. 0′ 0″ m. t. the moon's diameter ......69 rev. $\frac{10}{40}$=32′ 0″ [8]
7′ 50″ the eclipse begun.
10′ 50″ the shade at the middle of Grimaldus.
12′ 50″ the chord between the horns ...31 rev. $\frac{39}{40}$=14′ 46″
14′ 25″ the same repeated ....34 $\frac{6}{40}$=15′ 47″
17′ 25″ the same ....43 $\frac{16}{40}$=20′ 3″
19′ 55″ the shade on Kepler.
36′ 53″ the first edge of Tycho in the shade.
37′ 43″ the middle of Tycho, and the first edge of Plato.
38′ 33″ Tycho all in, and the first edge of Plato.
39′ 13″ the middle of Plato.
39′ 53″ Plato all in.
44′ 23″ middle of Manilius.
47′ 23″ Menelaus.
48′ 23″ Dionysius.
51′ 3″ Plinius.
54′$\frac{1}{2}$ moon's diameter ....69 rev. $\frac{10}{40}$=32′ 0″
57′ 45″ the chord ....58 $\frac{36}{40}$=27′ 13″
8h. 0′ 45″ the same ....52 $\frac{11}{40}$=24′ 9″
2′ 23″ the same ....47 $\frac{22}{40}$=21′ 58″
4′ 3″ the same ....39 rev. $\frac{24}{40}$=18′ 18″

1729, Feb. 2. 8h. 7′ 23″ total immersion of the moon into the shadow.
36′ 33″ the northermost and brightest star immerged.
9h. 53′ 20″ the chord.......................44 $\frac{16}{40}$=20′ 31″ dub.
55′ 55″ the same ....54 $\frac{6}{40}$=25′ 1″
10h. 2′ 23″ the bright part of the moon's diameter ....15 $\frac{39}{40}$= 7′ 23″
6′ 53″ the same; cloudy.......................20 $\frac{26}{40}$= 9′ 32″
11′ 40″ the same ....26 $\frac{4}{40}$=12′ 4″
13′ 50″ the same ....28 $\frac{4}{40}$=12′ 59″
47 57′ 50″ the end to the naked eye, dubious.
The equation of time is 14′ 47″ to be subtracted from mean time.

A total eclipse of the moon observed at Wansted, July 28, 1729, in 15f. tube.

10h. 50′ 0″ mean time, the moon's diameter 69 rev. $\frac{14}{40}$=32′ 3″.
Moon alt.=20° ∠ 11°$\frac{1}{2}$ paral.
11h. 16′ 20″ the eclipse began.
21′ 0″ the chord between the horns ...35 rev. $\frac{35}{40}$=16′ 35″
23′ 30″ Galileus in the shade.
26′ 0″ the chord between the horns ...44 $\frac{20}{40}$=20′ 34″
27′ 0″ the same repeated.......................47 $\frac{28}{40}$=22′ 3″
28′ 0″ the same ....50 $\frac{35}{40}$=23′ 30″
56′ 35″ the bright part of the moon's diameter.......................20 0= 9′ 15″
12h. 1′ 0″ the same repeated ....15 0= 6′ 56″
3′ 30″ the same ....12 $\frac{20}{40}$= 5′ 47″
5′ 45″ the same ....10 0= 4′ 37″
8′ 45″ the same .... 7 $\frac{20}{40}$= 3′ 28″
10′ 45″ the same .... 5 0= 2′ 19″
13′ 15″ the same repeated .... 2 $\frac{20}{40}$= 1′ 9″
15′ 35″ the total immersion of the moon into the earth's shadow.
1h. 54′ 45″ the moon began to emerge.
57′ 45″ the bright part of the moon's diameter .... 2 $\frac{20}{40}$= 1′ 9″
59′ 30″ the same .... 5 0= 2′ 19″
2h. 1′ 35″ the same .... 7 $\frac{20}{40}$= 3′ 28″
3′ 30″ the same ....10 0= 4′ 37″
5′ 45″ the same ....12 $\frac{20}{40}$= 5′ 47″
7′ 55″ the same ....15 0= 6′ 56″
12′ 45″ the same, very faint, cloudy ...20 0= 9′ 15″
56′ 0″ circ. the eclipse ended, or sooner, end dubious.

By the observations of the enlightened part of the moon's diameter before and after the total immersion, the total immersion happened at 15′¼, and the emersion at 55′¼, the middle at 13h. 5′¼ mean time, or 13h. 0′ 15″ app. time; the equation of time being 5′ 0″ to be subtracted from mean time.

1729, Aug. 24. 15h. 46′ 5″ m. t. the 1st sat. of Jupiter immerged in 15f. tube.

30. 15h. 37′ 10″ the 2d sat. of Jupiter immerged in the 15f. tube.

Oct. 2. 14h. 12′ 20″ the 1st sat. of Jupiter immerged in 15f. tube. [8]

Dec. 8. 13h. 15′ 30″ the 3d sat. of Jupiter immerged in Mr. Hadley's reflector, it was observed till it appeared exceeding small; allowance must be therefore made for the telescope. Jupiter appeared distinct and well.

12. 9h. 9′ 0″ the 1st sat. of Jupiter immerged in the refl. air somewhat hazy and cloudy; but I believe the observation cannot err much.

1730, Feb. 21. 6h. 29′ 25″ the 1st sat. of Jupiter emerged by the refl. [9]

Aug. 25. 11h. I looked at Saturn through my reflector, and found that his ring was become visible again, it having disappeared when I last saw him, which was about a month ago. It now appears like a line, but somewhat brighter and broader towards the ends than near the body. I saw three satellites, two to the west, which by their size and distance I judge must be the 2d and 1st, and appeared nearly in the line of the ansæ; the Hugenian satellite was to the east, likewise in the line of the ansæ.

Dec. 9. 11h. 5′ 15″ m. t. the 4th sat. of Jupiter immerged by the 15f. tube. The satellite appeared very small when I first looked at it, which was before it could touch the shadow, and Jupiter was but about an hour high; so that 'tis probable the immersion would have been somewhat later if it had not been for those accidents.

15h. 31′ 15″ the 4th sat. was seen emerging in the 15f. tube, very small; Jupiter appearing very bright and distinct.

15. 12h. 7′ 35″ the 1st sat. of Jupiter immerged in 15f. tube. [10]

30. 12h. 2′ 30″ the 3d sat. of Jupiter was immerged in 15f. tube: I saw it diminishing a little before, but while I was getting ready the reflecting telescope, it immerged. The time here set down, cannot, I believe, differ a minute from the true immersion in the 15f. tube.

The 2d sat. was a little nearer Jupiter than the 3d, so that I could but just distinguish them in the 15f. tube, for which reason the immersion might happen somewhat sooner than otherwise it would.

12h. 43′ 0″ the 2d sat. of Jupiter immerged by 15f. tube, very exact [8]: this satellite appeared rather brighter than usual before the immersion, seeming as bright as the 1st, which was about the same distance from Jupiter, but on the other side.

1731, Jan. 5. 17h. 48′ 35″ the 1st sat. of Jupiter immerged in 15f. tube [10]: obs. v. good; Jupiter very bright.

Feb. 6. 14h. 21′ 25″ the 1st sat. of Jupiter immerged in the reflector [8]: the satellite was very near Jupiter's limb, for which reason I used the reflector.

1731, April 11. 9h. 48′ 25″ the 1st sat. of Jupiter emerged by the 15f. tube [9]: obs. good.

1732, Jan. 10. 15h. 9′ 50″ the 1st sat. of Jupiter immerged by 15f. tube. [7]

April 13. 12h. 31′ 50″ the 1st sat. of Jupiter emerged by 15f. tube. [9]

Observations made at Oxford.

June 13. 9h. 15′ mean time, η Virginis preceded Jupiter 9″ of time=$2'\frac{1}{4}$ in right ascension, and was 8 rev. $\frac{37}{40}$=4′ 7″ more northerly than Jupiter's centre.

14. 8h. 50″ m. t. η Virginis preceded Jupiter 27″ of time=6′ 46″ in right ascension, and was 13 rev. $\frac{25}{40}$=6′ 18″ more northerly than Jupiter's centre. Both these observations were made with the same 15f. tube and micrometer that were made use of in the forementioned observations, which were made at Wansted in Essex.

30. 9h. 32′ 0″ m. t. the 1st sat. of Jupiter began to emerge in the 15f. tube. [7]

Dec. 6. An eclipse of the sun observed at Oxford.

Clouds prevented my seeing the beginning.

At 9h. 6′ 0″ by my watch, the chord between the horns was 9 rev. $\frac{15}{40}$ in the 15f. glass=4′ 20″.

At 7′ 30″ chord 10 rev. $\frac{18}{40}$=4′ 50″
11′ 15″ ....11 $\frac{36}{40}$=5′ 30″
14′ 0″ ....12 $\frac{15}{40}$=5′ 43″ then clouds.
24′ 30″ .... 7 $\frac{31}{40}$=3′ 35″ dubious.
26′ 0″ .... 6 $\frac{23}{40}$=3′ 2″
27′ 15″ } the end.
27′ 30″ }

The watch was 4′ 50″ slower than the clock in the museum.

Nov. 30. 0h. 5′ 37″ sun's cent. trans. filum axis.
Dec. 1. 0h. 6′ 12″ sun's cent. trans.
2. 0h. 6′ 48″ sun's cent. trans.

If I suppose the clock to go regular, the sun's centre would pass the meridian axis on Dec. 6, at 9′ 13″; but the axis pointing $3'\frac{1}{2}$ to the east of the meridian, the sun would be on the meridian 13″ after it had passed the axis[k], therefore at noon, Dec. 6, the clock in the museum would be 9′ 26″ faster than apparent time; and at 9h. in the morning 'twas about 9′ 22″ too fast for apparent time, but my watch being 4′ 50″ slower than the clock, the watch must be 4′ 32″ too fast for app. time.

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[k] The following memorandum, which is inserted in the Oxford observation book, will account for this large deviation of 13″. "1741, Feb. 17. N. B. The mark by which the transit instru-"ment of the museum is adjusted, is the eastermost part of the lower ridge of the roof of the "New Church*, (in High-street,) and the left hand side of the middle thread being brought "just to appear to touch the said point, the axis has been found by several trials to direct the

* All Saints.

By the next day's observation it appeared that the watch was 4′ 34″½ too fast for app. time.

1732[1], May 2. Watch or clock too slow for ap. time 4′ 40″.

Eclipse of the sun.

′ ″	H. ′ ″		Rev.	′ ″
	3 35	diameter inside	68 $\frac{10}{40}$	
+4 43	5 35 0	in. dist.	30 $\frac{13}{40}$	=14 6
	37 0	cor. chord	18 $\frac{15}{40}$	= 8 30
	38 0		22 $\frac{31}{40}$	=10 32
	39 20	dig. 1	27 $\frac{5}{40}$	=12 32
	41 0		31 $\frac{2}{40}$	=14 21
	42 20		34 $\frac{18}{40}$	=15 55
	44 0		37 $\frac{38}{40}$	=17 32
	45 45		40 $\frac{35}{40}$	=18 54
	50 30	pars lucida 3 dig.	49 $\frac{23}{40}$	=23 2
	52 40		47 $\frac{30}{40}$	=22 12
	55 0	5 dig.		
	59 30	6 dig.	38 $\frac{34}{40}$	
	6 2 0		30 $\frac{29}{40}$	
	4 15		32 $\frac{29}{40}$	
+4 42	5 0	7 dig.		
	6 4		30 0	
	10 0	8 dig.		
	12 0		24 $\frac{10}{40}$	
	14 15		21 $\frac{19}{40}$	
	15 0	horns vertical. 9 dig.		
	16 0		20 $\frac{5}{40}$	
	18 30		17 $\frac{26}{40}$	
	21 10		15 $\frac{20}{40}$	
	25 0		13 $\frac{1}{40}$	
	28 0		12 $\frac{17}{40}$	= 5 52
	30 0		12 $\frac{12}{40}$	= 5 48
	38 30		17 $\frac{10}{40}$ cor. par. hor.	
	46 30		25 $\frac{22}{40}$	
	51 0		30 $\frac{34}{40}$	
	55 15		35 $\frac{29}{40}$	
	57 20		38 $\frac{34}{40}$	

" telescope 3′ 10″ in azimuth to the east of the meridian; so that an object in the equator will " pass the middle thread 10″ in time before it comes to the true meridian. Whence may be " found the allowance for an object whose declination is given."

[1] 1733.

′ ″	H. ′ ″		Rev.	′ ″
+4 30	7 5 0		47 $\frac{30}{40}$	
	9 10	chord	40 $\frac{35}{40}$	=18 54
	10 35		37 $\frac{38}{40}$	=17 32
	12 20		34 $\frac{18}{40}$	=15 55
	13 45		31 $\frac{2}{40}$	=14 21
	14 45		27 $\frac{5}{40}$	=12 32
	18 55	end at	23 $\frac{35}{40}$ a lim. bor.	
+4 40 to the	19 0			

watch gives app. time.

1733, Aug. 2. 8h. 41′ 15″ app. time, the 3d sat. of Jupiter immerged (at Oxford) in my 15f. tube: Jupiter was low, but appeared pretty well, and the observation, I think, pretty good, and may at least be depended upon within the minute.

1736, Sept. 23. 4h. 39′ 5″ app. time, I observed the beginning of the eclipse of the sun in a seven foot glass, and though I could not (by reason of clouds) make any farther observations, yet I take this observation to be pretty exact, so as to be depended upon within 5″.

Oct. 7. An occultation of Mars by the moon observed with a 15f. glass at Oxford.

14h. 11′ 34″ app. time, diff. of decl. between the centre of Mars and the south limb of the moon was 22 rev. $\frac{9}{40}$=10′ 16″.

14′ 34″ diff. decl. between Mars and south limb of moon 20 rev. $\frac{34}{40}$=9′ 38″.

17′ 33″ Mars touched the moon's eastern limb.

18′ 8″ the centre of Mars touched the moon's limb.

18′ 41″ Mars wholly hid by the moon; very exactly observed.

44′ 0″ diam. of moon inter cornua 64 rev. $\frac{32}{40}$=29′ 57″.

15h. 9′ 40″ Mars' limb appeared from behind the moon.

10′ 25″ Mars was wholly out.

At the time of the emersion 'twas hazy weather; at the time of the emersion, Mars was 2 rev. $\frac{38}{40}$=1′ 22″ more north than the moon's south limb; diff. declination was 1′ 22″ at 15h. 10′½.

1737, Feb. 18. Eclipse of the sun.

Subtract 15′ 47″ from the time of the watch at the beginning of the eclipse gives ap. time.

H. ′ ″		Rev.
10 10 0	sun's diam.	69 $\frac{25}{40}$ Ins.
2 34 30	beg.	
35 50	chord	14 $\frac{4}{40}$
37 40	ch.	18 $\frac{13}{40}$
42 40	ch.	28 $\frac{17}{40}$

	H.	′	″		Rev.
1737.	3	12	30	part. lucid.	$40\frac{30}{40}$
		20	15	lucid.	$34\frac{35}{40}$
		31	20	touch first spot large	
		40	0	touch second spot	
		49	0	lucid.	14 0
		50	0	horns perp.	
		53	0	lucid.	12 0
		55¼			$11\frac{10}{40}$
		57¾			$11\frac{3}{40}$
	4	3			12 0
		7¼			14 0
		11	20	horiz.	
		20	0	8 dig.	
		27	40		$29\frac{2}{40}\frac{1}{2}$
		28½		7 dig.	
		34	25	6 dig.	$34\frac{35}{40}$
		41	30	lucid.	$40\frac{30}{40}$
		42½		5 dig.	
		45		spot appeared largest	
		49½		4 dig.	
		56		3 dig.	
	5	2	45	2 dig.	
		8	30	chord	$24\frac{2}{40}$
		9	30	1 dig. ch.	$20\frac{38}{40}$
		10	45		$17\frac{25}{40}$
		12	30		$10\frac{11}{40}$
		12	15 or 17	end.	

The watch lost from the beginning to the end of the eclipse only nine seconds.
At the end subtract 15′ 38″ gives app. time.
The equation of time is at the eclipse 12′ 44″.

$$\left.\begin{array}{l}\text{Oct. 28. 7h. 14′ 40″ m. t. imm. 3d sat.}\\\text{10h. 14′ 50″ m. t. emer. 3d sat.}\end{array}\right\}\text{ in 15f. tub. obser. good.}$$

29. 5h. 27′ 45″ m. t. begin. em. 1st sat. 15f. g. v. good.

1738, Oct. 16. 10h. 34′ 40″ m. t. in 15f. t. 2d sat. emerged.
37′ 30″ m. t. emers. 1st in 15f. tube. [8]

Both the satellites were near Jupiter's limb, but Jupiter appeared very distinct. Moon at full and near Jupiter.

11h. 5′ m. t. Jupiter's following limb had the same right ascension with *o* in nodo lini Pisc., and Jupiter's centre was more northerly than the star 5′ 10″ in declination.

1738, Nov. 8. 10h. 50′ 25″ m. t. em. 1st. [8]
10. 5h. 20′ 0″ em. 1st. [7] a little hazy.
7h. 49′ 30″ em. 2d. [8] good.
} in the 15f. tube.

12. 6h. 57′½ the 3d sat. was seen, but it appeared small as if it had not quite recovered its full lustre, clouds prevented my seeing it before it immerged, as also just at the time of its beginning to emerge, though I believe it could not have been seen much sooner had not the clouds hindered.

Dec. 10. 7h. 28′ 40″ m. t. the 1st sat. began to emerge [7] 15f. tube; it was cloudy at intervals, but I judge the observation to be good; for one of the satellites being then very near Jupiter's body on the same side, and another not far from it, appeared very plain at the time I first saw this beginning to emerge.

Dec. 12. Shirburn at 5h. 0′ 33″ by the clock, imm.
6 3 23 Aldebaran emerged.
diff. decl. 28 rev. 10′=14′ 35″ im.
10 0′= 5′ 10″ em.
lat. of Shirburn 51° 39′ 0″

The clock was 22′ 31″ too slow for mean time about the time of the middle of the occultation ...... +12 equat. t.

22 43 clock too slow for ap. t.

		H.	′	″	
Hence the immersion of Aldebaran was	at	5	23	16	ap. t.
	or	5	23	4	m. t.
and the emersion	at	6	26	6	ap. t.
	or	6	25	54	m. t.
Set the immersion of Aldebaran	at	5	23	15	ap. t.
the emersion		6	26	5	ap. t.
	or	6	26	0	on account of its not being seen as soon as it emerged.

1739, July 24. sun eclipsed, 15f. tube.

H.	′	″		Rev.
3	10	58	beg. eclipse.	
	12	30	chord	$12\frac{30}{40}$
	14	50	....	$20\frac{3}{40}$
	16	30	chord	$23\frac{8}{40}$
	31	27	touched first spot	
	35	0	pars lucida.........................	$50\frac{6}{40}$
	38	10	p. luc...............................	$47\frac{24}{40}$
	45	30	....	$42\frac{12}{40}$
	52	15	....	$37\frac{35}{40}$
4	0	0	....	33 0
	0	50	second small spot	

	H.	′	″		Rev.
1739, July 24.	4	2	30	touch second great spot	
		9	0		$28\frac{20}{40}$
		16	30		26 0
		20	30		$25\frac{12}{40}$
		23	45		25 0
		27	30	int. corn.	62 0
		31	45		26 0
		38	30		$28\frac{20}{40}$
		40	0	first spot out.	
		47	0		33 0
		54	0		$37\frac{35}{40}$
	5	0	0		$42\frac{12}{40}$
		6	30		$47\frac{34}{40}$
		29	30	end	d. d. $32\frac{5}{40}$ from the north limb.
		35	0	sun's diameter eq.	$67\frac{31}{40}$

N. B. The watch was 2″ behind the study clock at 2h. 50′, and 1″ before it at 5h. 50′; having gained 3″ in 3h.

The study clock at noon was 36″ too slow for ap. time, and lost 18″ per diem of mean time, or 23″ of the ap. t.

41″ added to the time by the watch, gives ap. time during the whole eclipse.

Oct. 28. 8h. 21′ 50″ m. t. 8h. 37′ 50″ ap. t. em. 1st in 15f. tube. [8]
Nov. 9. 17h. 42′ 14″ 17h. 56′ 7″ em. 1st 15f. tube. [7]

1740, Nov. 5. 9h. 19′$\frac{1}{4}$ by the study clock, the 4th sat. of Jupiter immerged (by the 15f. glass); and at 11h. 58′ 45″ it began to emerge. Hence the immersion may be set at 8h. 59′ 0″ m. t. and the emersion 11h. 38′ 30″ m. t. [8] seen when very small.

1741, Feb. 6. 8h. 34′ 40″ by study clock, the 3d sat. was emerged; while I was moving the rest it came out, but I believe the time here set down must be within the minute or less [1].

Feb. 8. the 2d sat. of Jupiter emerged [6] 8h. 35′ 22″ m. t.; 8h. 21′ 5″ ap. t.
the 1st sat. of Jupiter em. very exact, [9] 11h. 5′ 38″ m. t.; 10h. 51′ 21″ ap. t.

13. the 3d sat. of Jupiter immerged, the moon being very near Jupiter 9h. 26′ 48″ ap. t.; 9h. 40′ 23″ m. t.

15. 2d sat. of Jupiter emerg. [7] 11h. 0′ 35″ ap. t.; 11h. 13′ 48″ m. t.

17. 1st sat. of Jupiter emerged [8] 7h. 29′ 26″ m. t.; 7h. 16′ 34″ ap. t.

24. 1st emerged in 15f. tube [8] 9h. 24′ 15″ m. t.

March 12. the 1st sat. of Jupiter began to emerge in 15f. tube [8] 7h. 36′ 42″ ap. time. 7h. 43′ 20″ m. t. moon was near Jupiter.

[1] At noon, clock too slow on Feb. 7. 8′ 26″$\frac{1}{2}$. Feb. 10. 7′ 48″.

I thought that I saw the 2d sat. of Jupiter emerging almost in contact with the 1st, but a little more southerly in appearance: as it grew brighter they both seemed to form an oblong star, and continued to appear so for 10′ or 15′ after; the 2d being rather farther off Jupiter than the first: Quære. Place the emersion at 8h. 8′¼ ap. t.; or 8h. 15′ m. t.

1741, March 26. I thought I saw the 1st sat. of Jupiter emerging exceeding small, (a strong Aurora Borealis): put the emersion 11h. 31′ 0″ ap. t.; 11h. 33′ 15″ m. t.

May 3. 8h. 47′ 5″ by study clock, 3d sat. emerg.[m]

The[n] comet from α Lyræ 16° 6′ and
from β Cygni 6° 0′ gives its right asc. ....284° 44′ 43″
dist. from pole........................66° 9′ 20″

1743, Oct. 21. Moon eclipsed in the 7f. tube; cloudy and hazy.

H.	′	″		Rev.		′	″
3	35	0	s. t. the moon's diameter	33	33=	32	47
	58	45	the chord between the horns	24	0=	23	12
4	4	28	partes lucidæ	23	0=	22	20
	6	25	partes lucidæ, very hazy	21	2=	20	27
	9	0	partes lucidæ	19	6=	18	37
	12	8	partes lucidæ	17	33=	17	20
	14	0	partes lucidæ	17	20=	17	2
	24	33	partes lucidæ	10	33=	10	35
	26	10	partes lucidæ	10	10=	10	2
	27	37	partes lucidæ, very hazy	8	24=	8	26
	31	38	partes lucidæ	6	8=	6	8
	35	25	between the horns	28	11=	27	20
	37	50	between the horns	24	26=	23	50
	45	0	the immersion very uncertain, because of clouds and haziness.				

14 24 30⅓ by transit, the sun passed the meridian.
23. 32 19½ sun's centre.
25. The sun's true diameter=32′ 20″.

Greenwich, Oct. 24. The transit of Mercury.

Clouds and a hazy air prevented me from observing the ingress with certainty; but at 20h. 30′ 23″ ap. time, Mercury appeared as a notch in the sun's limb, the central ingress being near this time, or perhaps somewhat later. Clouds immediately coming on hindered me from seeing it again till 20h. 33′ 47″, when it was wholly within the sun, and near its own diameter from the sun's limb.

From this time the sight of the sun and Mercury was totally obstructed by clouds and hazy air, till above two hours after the ingress, when the clouds becoming thinner I could distinguish Mercury well enough to make the following observations.

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[m] At noon, clock too fast on the 2d, 5′ 23″½. On the 16th, 6′ 7″½.

[n] This observation is not dated, but it appears from some computations on the same paper to have been made in the morning of the 24th of February 1742.

1743, Oct. 24. 22h. 31′ 46″ ap. t. Mercury's centre moving on the fixed wire preceded that part of the sun's limb which was in the same parallel of declination with it 1′ 11″ sidereal time, or 17′ 45″ in right asc. The diff. of declination between the centre of Mercury and the sun's south limb being at the same time observed 16 rev. $\frac{22}{40}$=7′ 39′.

	H. ′ ″		′ ″	″		Rev.		′ ″
app. t.	22 42 41	diff. right asc.	1 14	=18 30	diff. decl.=	16 $\frac{38}{40}$	=	7 50
	45 12	diff. right asc.	1 14½	=18 37½.	diff. decl.=	17 10	=	7 58,3
	48 36	diff. right asc.	1 16-	=18 56	diff. decl.=	17 22	=	8 6,7
	56 45	diff. right asc.	1 19½	=19 52½	diff. decl.=	18 12	=	8 27,4
	23 1 8	diff. right asc.	1 22	=20 30	diff. decl.=	18 24½	=	8 36
	3 29	diff. right asc.	1 22½	=20 37½	diff. decl.=	18 36	=	8 44
	5 49	diff. right asc.	1 23	=20 45	diff. decl.=	19 2½	=	8 48,6
	8 49	diff. right asc.	1 25½	=21 22½	diff. decl.=	19 17	=	8 58,7
	40 42	diff. right asc.	1 39½	=24 52½	diff. decl....	22 15½	=	10 20$\frac{8}{10}$
	58 0	diff. right asc.	1 47	=26 45	diff. decl....	23 37	=	11 3,4 q
25.	0 2 15	diff. right asc.	1 48½	=27 7½	diff. decl....	24 15	=	11 16
	7 2	diff. right asc.	1 50	=27 30	diff. decl....	24 36	=	11 30,½
	16 27	diff. right asc.	1 55−	=28 41	diff. decl....	25 31½	=	11 55

24 42 { diff. r. a. of Mercury and sun's cent.=0′ 53″=13′ 15″
diff. r. a. 2L. 1′ 58″=29′ 30″ diff. decl....26 21 =12 15,6

29 11 diff. right asc. of Mercury and 1L. 0′ 11″= 2′ 41″.

34 11 { diff. right asc. sun's cent. 57′½=14′ 22″½ Rev.
diff. right asc. 2L. 2′ 3″½ ::=30′ 52″½ diff. decl. 27 15 =12′ 39″.

46 58 { diff. r. a. sun's cent. 1′ 1″−=15′ 11″
diff. right asc. 2L......2′ 7″=31′ 41″ diff. decl. 28 20½=13′ 10″,6.

51 55 diff. right asc. of Mercury and 1L. 0′ 4″=1′ 0″
54 3 .... diff. decl. 29 7 =13′ 29″.
1 2 7 central egress.
1 0 42 interior contact. } By Mr. Graham and Mr. Short.
2 16 exterior contact. }
7 56 43 exterior cont. at Kingston in Jamaica°.

Dec. 26. 5h. 20′ 16″ s. t.=10h. 8′ 15″ m. t. in the 7f. tube, the comet preceded a small star 2′ 53′⅓=0° 43′ 20″ diff. right asc.; diff. decl. comet south......... 0° 32′ 30″

° ′ ″			° ′ ″
0 43 20	diff. right asc.;	diff. decl. comet south.........	32 30
4 48 15	right asc. star.	by the mean of two obs.	68 13 30
4 4 50	Comet's right asc.	Comet à pol.	68 46 0

° Observed by Alexander Macfarlane.

1743, Dec. 27. 0h. 14′ 45″ s. t. or 5h. 8′ 25″ m. t. the comet was on the merid.

° ′ ″
Comet observed z. dist. 30 20 48
32
38 31 15
Hence the comet's right asc. 3° 41′ 15″. Comet from the pole 68 52 35

Dec. 28. 0h. 12′ 45″ + s. t. or 5h. 2′ 30″ comet passed the merid. à v. 30 28 47
32
38 31 15
Hence the comet's right asc. 3° 11′ 20″. Comet à pol. 69 0 34

Dec. 30. 4h. 0′ 56″ s. t. or 8h. 42′ 12″ m. t. By the mean of two observations, the comet preceded the middle point of the double star Piscium marked 1 ψ, 43′ 26″=

° ′ ″ ° ′ ″
10 51 30 Comet n. diff. decl. 0 37 17
13 0 0 right asc. of ψ Pisc. ψ à pol. 69 54 37
2 8 30 Comet's right asc. Comet à pol. 69 17 20

Dec. 31. 0h. 7′ 0″ + s. t. or 4h. 44′ 58″ + m. t. Comet on the merid. 30 51 48
38 31 47
Hence the comet's right asc. 1° 45′ 5″ Comet à pol. 69 23 35

1744, Jan. 6. 0h. 5′ 3″ s. t. or 8h. 18′ 46″ m. t. In the 15f. tube, by the mean of 3 observations, the comet preceded 54 Pisc. 29′ 53″=7° 28′ 15″ in right asc. and was 2′ 30″ more northerly.

° ′ ″ ° ′ ″
Right asc. 6 31 0 Dist. pol. 70 8 5
7 28 15 2 30
Right asc. of Comet 359 2 45 Comet à pol. 70 5 35

Jan. 10. 3h. 35′ 20″ s. t. or 7h. 33′ 25″ m. t. the comet preceded the 54th Pisc. 36′ 22″ + or 9° 5′ 35″ and was more southerly .... 21′ 50″
6 31 0 70° 8 5
357 25 25 right asc. comet. Comet à pol. 70 29 55

Jan. 11. 6h. 25′ 7″ s. t. or 10h. 18′ 48″ m. t. the comet preceded χ Pegasi 13′ 27″ or 3° 21′ 45″ in right asc. and was more north .... 36′ 25″
0 20 30 χ Peg. right asc. 71° 13 10
356 58 45 right asc. of the comet. Comet à pol. 70 36 45

1744, Jan. 13. 2h. 29′ 27″ s. t. or 6h. 15′ 55″ m. t. by 6 observations the comet preceded χ Pegasi 16′ 19″$\frac{2}{3}$=4° 4′ 55″ right asc., and was north ………… 26′ 3″

χ Peg. right asc. 0 20 30	71° 13 10
356 15 35	Comet à pol. 70 47 7

Jan. 15. 4h. 1′ 44″ s. t. or 7h. 40′ 5″ m. t. the comet preceded χ Pegasi (by 5 obs.) 19′ 35″=4° 53′ 45″ in right asc., and was north ………………… 14′ 10″

0 20 30	71° 13 10
Comet 355 26 45	Comet à pol. 70 59 0

Jan. 16. 2h. 23′ 6″ s. t. or 5h. 57′ 49″ m. t. the comet preceded χ Peg. 21′ 3″$\frac{2}{3}$ in time, or 5° 15′ 55″ in right asc. and was more northerly……………… 8′ 43″

20 30 star	71° 13 10
355 4 35=Comet's right asc.	Comet à pol. 71 4 27

Jan. 21. 3h. 25′ 0″ s. t. or 6h. 39′ 51″ m. t. (by 3 obs.) comet preceded φ Pegasi 7′ 21″=1° 50′ 15″ in right asc., being more north ……………… 0° 41′ 38″

354 52 10 star's right asc.	star 72 18 5
353 1 55 Comet's right asc.	Comet à pol. 71 36 27

Jan. 23. 4h. 7′ 25″ s. t. or 7h. 14′ 17″ m. t. (by 9 obs.) the comet preceded φ Pegasi 10′ 51″$\frac{1}{3}$=2° 42′ 50″ in right asc., being north ………………… 26′ 35″

354 52 10 φ Peg.	72° 18 5
352 9 20 Comet's right asc.	Comet à pol. 71 51 30

N. B. In these and the following observations an allowance is made for refraction when necessary.

Jan. 29. 4h. 7′ 9″ s. t. or 6h. 50′ 26″ m. t. (by 7 obs.) the comet preceded S. Pegasi 8′ 4″$\frac{2}{3}$=2° 1′ 10″ in right asc., and was north ………………… 8′ 26″

351 15 20 S. Peg.	73° 0 56
349 14 10 Comet's right asc.	Comet à pol. 72 52 30

Feb. 1. 3h. 59′ 40″ s. t. or 6h. 31′ 11″ m. t. (by 5 obs.) the comet preceded S. Pegasi 15′ 2″$\frac{1}{3}$=3° 45′ 36″ in right asc., being south ………………… 38′ 27″

351 15 20 star	star 73° 0 56
347 29 45 Comet's right asc.	Comet â pol. 73 39 23

Feb. 6. 4h. 1′ 0″ s. t. or 6h. 12′ 52″ m. t. (by 4 obs.) the comet followed α Pegasi 3′ 41″$\frac{1}{2}$=0° 55′ 22″ in right asc., and was north………………… 23′ 12″

343 0 3 α Pegasi.	76° 10 2
: 343 55 25 Comet' right asc.	Comet à pol. : 75 46 50

This observation is a little dubious. Cloudy.

1744, Feb. 9. 4h. 34′ 34″ s. t. or 6h. 34′ 32″ m. t. by one obs. the comet preceded q Pegasi 31′ 18″½=7° 49′ 37″ in right asc., and was more northerly......... 42′ 13″
349 3 5 star right asc. 78° 38 53
dubious : 341 13 28 Comet's right asc. dub. : Comet à pol. 77 56 40

Feb. 9. 5h. 13′ 43″ s. t. or 7h. 13′ 34″ m. t. the comet preceded λ Tauri 5h. 1′ 46″⅓=75° 26′ 35″ in right asc., being north .... 16′ 18″
56 37 55 right asc. of λ. λ à pol. 78° 15 20
dubious 341 11 20 Comet's right asc. dub. Comet à pol. 77 59 2

Feb. 10. 4h. 9′ 9″ s. t. or 6h. 5′ 15″ m. t. the comet preceded the 18th Orionis 6h. 20′ 49″−=95° 12′ 10″ in right asc., and was north .... 4′ 25″
75 27 40 18. Orionis star à pol. 78° 58 25
by one obs. 340 15 30 Comet's right asc. Comet à pol. 78 54 0

Feb. 12. 4h. 31′ 8″ s. t. or 6h. 19′ 19″ m. t. the comet preceded σ Pegasi 6′ 56″=1° 44′ 0″ in right asc. and was north................................ + 17′ 36″
refr. —20 refract. + 0 24
339 51 15 star's right asc. star à pol. 81° 31 10
338 7 35 Comet's right asc. Comet à pol. 81 13 10

Feb. 13. 4h. 2′ 47″ s. t. or 5h. 47′ 6″ m. t. the comet preceded p Pegasi 30′ 36″=7° 39′ 0″ in right asc. and was north of it.......................... 1′ 10″
344 41 55 star's right asc. star à pol. 82° 40 0
337 2 55 Comet's right asc. Comet à pol. 82 38 50

Feb. 17. 22h. 10′ 13″ s. t. or 23h. 35′ 51″ m. t. the comet passed the meridian, its apparent zenith distance being then .... 53° 56′ 18″
ref. 1 15
Co. Lat. 38 3′ 15
Hence the comet's right asc. 332° 33′ 15″ Comet à polo 92 28 48

1748, April 21.[p] By the sector.

H. ′ ″		° ′ ″	turn. n.	′ ″	
13 32 50	a comet, Nonius at	3 33 21½			
14 40 6	λ Cassiop. Nonˢ. at	3 28 9	diff. 1 12½	= 5 45	the star south
45 9−,	ζ Cassiop. Nonˢ. at	2 51 11	diff. 11 10½	=42 5	
58 21	the comet, Nonˢ. at	3 1 18			
15 5 8+,	λ Cassiop. Nonˢ. at	2 43 7	4 11	=16 30	
15 10 10−,	ζ Cassiop. Nonˢ. at	2 6 2½	13 15½	=50 15	

[p] The transits of this comet are inserted in the 2d volume of the Greenwich Observations, p. 425, with a memorandum that no mention is made of it in the quadrant book, or in any loose paper, which the editor had then been able to find. This deficiency is now supplied.

H. ′ ″ 1748, April 21. In 7 foot tube.

16 25 1 the comet N.

17 31 26 λ Cassiop. diff. decl. 30 rev. 10=29′ 15″

32 43 star 6 mag. diff. decl. 7 7= 7 0

22. By the sector.

At 14 32 the comet preceded a 5 mag. star about 2″,

Comet, Non^s. 3° 39′ 8″

the Star north 4 14 0½ diff. 9 t. 14n.½=35′ 25″,

At 14 50 the star preceded the comet about 1″.

Comet, Non^s. 3° 41′ 18″½

Star, Non^s. 4 13 19 ½ diff. 8t. 1n.=29′ 30″

16 18 7 Star 3 Cass.[p] or 5 mag. Rev. ′ ″

18 35 Comet............diff. 22 5=21 29½

16 22 20 S.

22 50 C..................diff. 20 39=20 23

16 25 30 S.

26 4½C..................diff. 20 10=19 37

16 31 14 S.

31 53 C..................diff. 19 38=19 23

16 36 9 S.

36 47 C..................diff. 18 38=18 26

16 49 27 S.

50 12 C..................diff. 17 16=16 59

16 57 13 S.

57 56 C..................diff. 16 35=16 29

17 1 11 S.

1 56 C..................diff. 14 37=14 33

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[p] Each "turn" for the equatorial sector was equal to 3′ 40″, and each subdivision, or "n," was equal to 10″. These quantities will not answer for the reduction of the differences which is given for the observations of April 22, from 16h. 18′ 7″ to 17h. 20′ 57″; these observations were therefore most probably made with the 7f., although no mention is made of it. Bradley found that 80 revolutions of the screw of his micrometer were equal to 36′ 58″,4 for his 15f.; 56′ 2″,3 for his 10f.; and 77′ 11″,3 for his 7f.; the circumference of the head of his screw was divided into 40 equal parts, and it will be found that the values assigned to the revolutions and parts, in the observations which have just been mentioned, will answer very nearly to those which would be found in the use of the 7f. They are indeed a few seconds too large: but he was in the habit of examining the micrometer before an observation, to see if the wires exactly coincided when the index stood at 0; he also brought the wires sometimes to touch rather than to bisect the object, and in either case it was necessary for a correction to be applied.

From the same reason it appears that the 7f. was used on the 15th of May, although no notice is taken of it.

H. ′ ″
17 10 13 S.
Rev. ′ ″
11 4 C. diff. 14 3=13 43
17 20 6 S.
20 57 C. diff. 13 20=13 10

1748, April 23. By the sector.

° ′ ″
11 55 12 the Comet Non^{s}. at 3 34 3
turn. n. ° ′ ″
12 5 0 τ Cassiop. Non^{s}. at 11 38 11 diff. 31 14 =1 56 0 the S. south of the Comet.
12 15 55 Comet Non^{s}. at 3 37 1½
Cloudy. τ Cassiop. Non^{s}.......1 39 14 diff. 32 9½=1 58 55
45 17 β Cassiop. Non^{s}.......2 10 3 diff. 23 20½=1 27 45
13 5 52 the Comet Non^{s}. at...3 46 12
Cloudy. τ Cassiop. Non^{s}. at...1 42 12 diff. 32 22 =2 1 0

April 24. In the 7f. tube.
13 25 20 the Comet.
Rev. ′ ″
14 9 26— κ Cassiop. diff. declin. 27 35=27 2½ the S. south of the Comet.
14 23 28 the Comet.
15 7 10 κ Cassiop. d. d.........33 14=32 19
15 44 4 the Comet.
16 1 45 10 Cassiop. d. d.........34 35=33 47½ the S. north of the Comet.

April 27. By the sector.

° ′ ″
12 39 15 the Comet Non^{s}. at 3 23 21 -
turn. n. ′ ″
13 14 13 S. 6 mag. Non^{s}. at 2 34 16 diff. 13 5=48 30 S. south
27 19 31 Cassiop. Non^{s}. at 2 35 0 diff. 12 21=47 30 S. south
13 34 33 the Comet. Cloudy.

May 2. By the sector.
12 37 21 S. 6 mag. Non^{s}. at 3 37 1
50 0: the Comet Non^{s}. at 3 20 6 diff. 4 17=17 30 S. north.

May 12. By the sector.
13 39 15 the Comet Non^{s}. at 3 32 16
° ′ ″
51 25 S. 6 mag. Non^{s}. at 4 42 18 diff. 19 2= 1 10 0 S. north.
14 31 55 the Comet Non^{s}. ...3 30 5
43 5 S. 6 mag. —— ...4 41 13 diff. 19 7= 1 10 50
15 17 22 the Comet.
Cloudy. 6 mag. S. d. d. 29 rev. 35=28′ 58″ the S. north by the 7f. tube.

May 14. By the sector.
14 19 50 the Comet Non^{s}. 3 20 9
42 50 S. 5 mag. Non^{s}. 4 26 7 diff. 17 20= 1 5 40

H. ′ ″ ° ′ ″
14 52 8 the Comet Non^{s}. 3 22 21 turn. n. ° ′ ″
15 15 36 S. 5 mag. Non^{s}. 4 28 20 diff. 17 21=1 5 50
15 21 30 the Comet. Rev. ° ′ ″
43 59 S. 5 mag. d. d. 67 31=1 5 32 the S. north in the 7f. tube.
49 14 the 1st S. 6,7 mag. d. d. 19 15=0 18 50
52 6 the 2d S. 6,7 mag. d. d. 13 24=0 13 16

1748, May 15. By the sector.
13 44 0 the Comet Non^{s}. 3° 10′ 15″ turn. n. ° ′ ″
56 25 S. 5 mag. Non^{s}. 4 22 7 diff. 19 14=1 12 0
14 39 58 the Comet. Rev. ° ′ ″
S. n. 51 30 S. 5 mag. d. d. 74 10=1 11 47 the S. north.
56 52 : the 1st S. 6,7 mag. d. d. 25 30=0 24 59
59 29 the 2d S. 6,7 mag. d. d. 20 1=0 19 28
15 11 54 the Comet.
28 30 the 1st S. 6,7 mag. d. d. 25 35= 25 7
31 12 the 2d S. 6,7 mag. d. d. 20 3= 19 31
15 50 4 the Comet.
16 6 17 the 1st S. 6,7 mag. d. d. 26 5= 25 21
9 2 the 2d S. 6,7 mag. d. d. 20 13= 19 45

May 16. In the 7f. tube.
14 58 50 the Comet.
15 4 44 the 1st S. 6,7 mag. d. d. 35 22=34 27
7 36 the 2d S. 6,7 mag. d. d. 29 29=28 50
8 58 the 3d S. 6,7 mag. d. d. 8 36= 8 44
15 12 30 the Comet.
18 25 the 1st S. 6,7 mag. d. d. 35 38=34 52
21 10 the 2d S. 6,7 mag. d. d. 30 4=29 1
the 3d S. 6,7 mag. d. d. 9 1= 8 51½
15 38 48 the Comet.
44 20 : the 1st S. 6,7 mag. d. d. 35 24=34 30
47 12 the 2d S. 6,7 mag. d. d. 29 32=28 54
48 42 the 3d S. 6,7 mag. d. d. 9 5= 8 57
16 16 &c. the Comet.
1st S. 6,7 mag. ....d. d. 35 35=34 46
2d S. 6,7 mag. ....d. d. 30 3=29 10
3d S. 6,7 mag. ....d. d. 9 10= 9 4 stars north.

May 17. In the 7f. tube. Rev.
15 35 the Comet and 3d S. 6,7 mag. had nearly the same right asc. S. n. 19 13 =18′ 47″½.
15 50 the S. preceded the Comet about 10″. d. d. 19 rev. 20=18′ 57″½.

H. ′ Rev. ′ ″
16 0 Comet from the S. ......19 22=19 0½
16 3 ....19 21=18 59
16 5 ....19 24=19 3½
16 10 ....19 25=19 5
16 19 ....19 27=19 6½
16 29 30″ the S.
29 55 the Comet. d. d. 19 29=19 10½
16 37 6 the 3d S. 6,7 mag.
37 33 the Comet. d. d. 19 31=19 13½
17 16 15 the 3d S. 6,7 mag.
16 55 the Comet faint. d. d. 20 0=19 26

1748, May 18. In the 7f. tube.

16 20 25 the 3d S. 6,7 mag.
29 55 the Comet. d. d. 29 18=28 34
16 34 50 the S.
44 30 the Comet. d. d. 29 35=28 58
17 25 20 the S.
35 5 : the Comet. d. d. 30 10=29 19

Comet of 1759.

	Mean Time.	Right Asc.	Decl. S.		
May 1.	9 54 25[r]	159 50 52	25 39 59	by ω Hydræ	both near the parallel. obs. with the sector.
		50 45	40 18	6 Hydræ cont.	
2.	9 24 9	158 27 35	22 14 41	β Hyd. & Crat.	sector.
		27 52	14 41	γ Hyd. cont. nearest the paral.	
5.	10 44 30	155 58 25	15 25 26	2 φ Hyd. & Crat. by the mean of 4 obs. with the microm.	
	10 57 48	155 58 15	15 24 39	ν ditto settled by D. F. by 2 obs. with the microm.	
6.	9 59 16	155 29 42	13 58 31	μ Hyd. by 2 obs.	sector.
		29 30	58 18	ν Hyd. & Crat. by 2 obs. the S. ν settled by D. F.	
16.	10 21 20	153 42 11	6 42 25	by the mean of 6 obs. with the microm.	
	long.	5^s 8 8 50	16 24 5 lat.	from a star of the 5th mag. (not in	

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[r] There is a paper on which a reduction has been made of the observation of the 1st of May; from this it appears that the time was 9h. 34′ 25″, and not 9h. 54′ 25″. There seems likewise to be an error of transcription with respect to the first star which is mentioned for this same day: ω Hydræ lies very far from the parallel in which the Comet was moving; it is possible that the original was ϖ Hydræ Cont. It is hardly necessary to mention that the observation of the 25th of May must have been made at the earl of Macclesfield's.

the Catalogue) whose right asc. was afterwards found to be 153° 26′ 0″, and decl. 5° 51′ 30″ south.

	H. ′ ″	° ′ ″	° ′ ″	
18.	9 45 23	153 39 4	6 2 59	by 6 obs. with the microm. from the last
	long.	5^s 7 50 38	15 48 37 lat.	star.
19.	10 16 17	153 38 58	5 43 41	by 7 obs. with the microm. from ditto.
	long.	5^s 7 43 5	15 30 46 lat.	
20.	10 18 47	153 38 39	5 28 11	by 10 obs. with the microm. from ditto.
	long.	5^s 7 36 48	15 16 27 lat.	
25.	10 16 55	153 49 57 : :	4 29 29 : :	from Spica ♍ with the sector, very
	long.	5^s 7 25 10	14 17 46 lat.	faint and uncertain.
25.	10 33 0	153 48 0	4 26 0	by Shirburn observ.
	long.	5^s 7 22 0	14 15 15 lat.	
April 30.	10 13 0	161 41 0	30 0 20	
		5^s 26 29 10	34 32 50	at Gr.

The following observations, having been written on small separate pieces of loose paper, were overlooked at the time when they might have been inserted according to their respective dates.

1717, Jan. 17. 6h. 4′ 10″ im. 3d sat. (of Jupiter) by the clock.
9 0 0 it was as bright as any of the others.

1718, Jan. 15. 8 0 0 m. t. Jupiter had less right asc. than η of Cancer, the diff. in time 14′ 1″; the diff. of decl. was 1′ 1″ in time, Jupiter being more southerly than η.

An observation recorded by Halley, Phil. Trans. vol. XXX. p. 854, may likewise be added to this part of the collection.

1717, Dec. 5. Cum luna jam propemodum plena esset, stellam contulit cum insigni illa macula quam Ricciolus Tychonem Hevelius Sinam appellat, et ex pluribus equalibus distantiis micrometro ante et post captis, stellam dictæ maculæ centro proximam apparuisse conclusit ad 11h. 15′ 8″ T. eq. apud Wansted. Ad 11h. 15′ 42″ distabat Palilicium à limbo lunæ proximo et austrino 5′ 55″. Macula autem Tycho ab eodem limbo aberat 4′ 16″. Ad 11h. 18′ 42″ stella erat in linea recta cum maculis Tychonis et Copernici sive Sinæ et Etnæ, et 11h. 25′ 27″ T. eq. erat in rectâ cum Tychone et Keplero. Inter hæc observata est lunæ diameter 32′ 45.″

State of the Instruments at the Greenwich Observatory when Dr. Bradley became Astronomer Royal.

WHEN I came to reside at the Observatory, in June 1742, I made but few observations either with the mural quadrant or transit instrument, till I had made some alterations in both. For there being no good method of illuminating the wires of the telescopes either of the quadrant or transit instrument, I soon found it necessary to make proper apparatus's for that use. Dr. Halley seldom attempted to make observations when the wires required to be illuminated, but when he found that to be necessary, he usually (for the quadrant) placed a candle upon the south end of the wall on which it hangs, which shining upon the inside of the shutter, (which was painted white,) the light was from thence reflected through the object-glass upon the wires in the focus, and by raising the shutter (which slided up and down) till the upper edge of it was in part before the object-glass, more or less light was reflected into the telescope. But this way being inconvenient, especially in windy weather, I soon found it necessary to make a more useful apparatus, by fixing a lamp in a box near the object-glass, which shining on a pasteboard that was fixed on the telescope, and inclined in an angle of 45° to its axis, and through which an elliptical aperture was made to permit the light of the stars, &c. to pass to the object-glass; I could by this means illuminate the wires more or less as the case required, by pulling a packthread reaching to the eye end of the telescope, which would raise or fall a shutter that was before the box, and intercepted the light of the lamp; by this contrivance I could very readily and conveniently adapt the strength of illumination to the object I was observing.

The roof of the quadrant room was so near the top of the stone wall, that in some seasons the warping of the timber would cause the leaden weight which balances the telescope of the quadrant to rub against the boards, so that in some positions it required a pretty considerable degree of force to move the telescope; which may be supposed the reason why the too great stiffness with which it turned about the brass cylinder was not duly attended to, otherwise the accident which happened of breaking the screws with which that cylinder was fastened to the centre plate might have been prevented, by putting a little fresh oil to it. It was on the 5th of July 1742 that I first discovered those screws to be broken, and that the centre cylinder then adhered so firmly to the steel collar, that it required a considerable force to get it out after I had taken the collar off with the cylinder and carried it to Mr. Graham, who rectified it, and then new screws were made, and the cylinder (after it had been turned a little smaller) was again fixed on, July 9, 1742.

While Mr. Graham was rectifying the cylinder, &c. I examined the position of the quadrant as it then hung, which was in the same situation that Dr. Halley left it, I having before the 6th of July neither altered nor examined the situation of the vertical line. By the memorandum entered that day, it appeared that upon supposition that the line of collimation of the telescope was truly adjusted to the beginning of the nonius, the zenith distance to the south shewn by the quadrant would have been too small by $34''\frac{1}{2}$.

After the beginning of August 1742 I was able to examine likewise the position of the plane of the mural quadrant, by comparing the observations of the times of the passage of the stars at the quadrant, with those observed with the transit instrument, which I got altered in several respects by Mr. Sisson: and before I made use of it, I had likewise contrived a proper apparatus for illuminating the wires in all positions, which was wanting in Dr. Halley's time. The telescope of the transit instrument was likewise now balanced, so that it would stand in any inclination, (whereas in Dr. Halley's time the eye end being heaviest, he was obliged to support it at the proper altitude, upon which account observations could not be made so expeditiously as now.) At the same time Mr. Sisson put in two other wires (15′ from the middle wire) to be made use of for taking of the transit of objects, in case clouds, &c. should hinder me doing it at the middle wire. I had likewise a new level made for adjusting the axis truly horizontal. These alterations were made, and the line of collimation of the transit instrument was adjusted about July 24, but it could not be properly directed till I had a clear view of the mark which Dr. Halley had made on the park wall, the sight of which was intercepted by the boughs of trees which had grown up since Dr. Halley had used this instrument. These being cut away about the end of July 1742, I then set the instrument by his mark, which I at first supposed to be exactly in the meridian, but afterwards found that it lay 12″ or 15″ in azimuth to the west of the true meridian.

Between the 1st and 14th of August several stars were observed by the transit instrument and the quadrant, and by comparing the times of their transits, the errors of the plane of the mural quadrant were found to be as entered in the Table before August 14, 1742.

The quadrant was not altered till Aug. 18, when it was rectified; so that the centre lay exactly over the beginning of the divisions or the 0 point, and then a plummet hung from the notch at top at the centre plate was found to correspond well with the point on the limb below, to which it had been at first adjusted by Mr. Graham in 1726.

Aug. 20th, I first altered the plane of the quadrant, and by the method then described I brought the plane to lie much truer than it did as left by Dr. Halley, as appears by the Table of errors then found. About the end of August the telescope of the quadrant was taken off and sent to London, where several alterations were made to it, and after it was brought back again I adjusted the line of collimation, (on Sept. 11,) Mr. Sisson having made for me a new apparatus for that purpose.

N. B. By the observations of the transits of low stars that were observed at both instruments about Aug. 20 and 24, I judged that the 3d whole ballister on Mr. Stanhope's house lies very near the true meridian.

After the line of collimation of the quadrant telescope had been adjusted on Sept. 11, several transits were observed at both instruments, from which a Table of errors was collected as entered about the 19th of September 1742.

These errors may be made use of till Oct. 2, 1742, when I made some alteration in the position of the plane; the errors of which are contained in the Table entered Oct. 6th, which may be made use of till the next rectification.

N. B. 1745, Aug. 30. I perceived that some little time after I had placed the axis upon its brass notches, that the telescope altered its direction, for when the south mark was well bisected, it would not correspond to the N. marks as before. And as I conceive this must be owing to the cooling of the parts of the axis which had been heated by handling, the next morn when all the parts were reduced to the same temperature, I altered the wire till such time as it corresponded to the S. and N. marks in the same manner as when I first put it in its place, (immediately after 'twas rectified.) While I was adjusting the line of collimation, I tied cloths about the axis to prevent the immediate contact of my hand, but notwithstanding this caution 'tis manifest that either the heat of my hand through the cloths, or perhaps of my body approaching the bars of the axis is sufficient to produce this effect.

⁂ According to the Table prefixed to Aug. 14, the plane of the quadrant varied from the meridian at the zenith 31″ of time, the error diminished gradually to 35° of Z. D. were it was 5″ −, and then gradually increased again to 8″ + at the horizon. On the 20th of August the errors were much diminished, being only 3″ at the zenith and 0 at the horizon, the maximum was 4¼ at 10° of Z. D., and under 20° it in no instance exceeded 1″; the variation however was not regular, nor always to the same side of the plane.'

After this the maximum of error was diminished, but no portion of the arc was very near the truth to so great an extent as on the 20th of August.

Experiments to determine the Length of the Pendulum vibrating seconds at Greenwich.

1743,	Sid. Time. H. ′	Little Therm.	Ball. Rev.	Brass Rod. Rev.	Great Therm. °	Steel. Rev.	Copper. Rev.	Wood. Rev.	Barom.	Length of simple Pendulum.	Arcs of Vibration.
Sept. 13.	1 15		4 3	4 11	64	4 15	4 12,5	4 18	30,0	39,1349	
14.	7 15		−2,8	−10,2	61	−14,5	−11,8		30,1	39,1357	28′ to 4′
	22 40		−3,0	−10,8	64	−14,7	−12,3	−18,5			
15.	22 30		−2,9	−10,3	63	−14,7	−12,0	−18,2	29,9	39,1369	30′ to 1′
16.	23 30		−3	−11	64	−15	−12,8	−18,3			
19.	9 10		−2,0	− 9,5	60	−14,25	−11,1	−18,1	30,1	39,1352	38′ to 1′
20.	18 10		−2,9	−11,2	65	−15	−12,8	−19,3			
23.	11 30		−1,9	− 8,5	58	−13,8	−10,3	−18,3	30,1	39,1384	30′ to 1′
24.	13 0		−2,0	− 8,4	57½	−13,8	−10,3	−17,8	30,0	39,1383	33′ to 0′,5
25.	1 30		c.−1,9	c.− 9,1	58	−13,8	−10,3	−18,0			
25.	15 0		−2,5	− 9,1	58+	−14,1	−11,05	−18,2	30,0	39,1395	32′—to 2′—
26.	14 0		−1,9	c.− 8,9	57	−13,8	−10,5	−17,8			
						Ball altered.					
27.	11 40		−7,0	− 6,9	52	−13,0	− 8,15	−16,75	30,0	39,1395	30′ to 1′
	13 20	55°	−7,25		57	little thermom. placed near the ball.					
28.	12 0	54	−6,9	− 7,8	52	−13,0	− 9,1	−17,1			
	13 30	53½	−7,3	− 7,55	55	−13,3	− 9,75	−17,2	29,9	39,1396	16′ to 1′½
	0 15	55	−7,0	− 7,8	54	−13,25	− 9,4	−17,1			
						Then the rod was screwed into another hole, viz. near the flaws, &c.					
30.	8 50	50+	−19,5	− 6,3	50+	−12,8	− 8,45	−17,6	29,8	39,1376	20′ to 2′—
	22 40	52½	−19,4	− 6,6	52½	−12,8	− 8,4	−16,2			
						The wire now screwed into the opposite hole.					
Oct. 3.	10 3	48	− 6,6	− 5,5	48	−12,8	− 7,9	−17,0	29,8	39,1421	24′ to 3′
	1 0	50½	− 7,0	− 7,0	52	−13,6	− 9,5	−18,2			
						The rod screwed again into the same hole as on Sept. 30.					
4.	10 30	50	−19, 5	− 6,1	50	−12,9	− 8,4	−18,2	29,8	39,1390	20′ to 3′
	0 30	50+	−19,25	− 6,75	50	−12,9	− 8,8	−18,25			
5.	10 0	45½	−18,55	− 4,8		−12,15	− 7,2	−17,9			
						Then the wire was screwed into the same hole as Sept. 27, &c.					
5.	11 20	46	− 6,1	− 5,35	46	−12,7	− 7,9	−18,3	29,8	39,1412	15′ to 2′
	0 15	49+	− 6,0	− 6,1	48	−12,8	− 8,0	−18,5			
6.	10 35	44	− 5,5	− 4,7	44	−12,1	− 7,1	−18,2	29,8	39,1410	60′ to 7′—
	23 40	48	− 6,0	− 6,2	47	−12,75	− 8,0	−18,8			
7.	11 30	46	− 5,95	− 5,25	46	−12,7	− 7,9	−18,8	29,6	39,1415	60′ to 7′
	0 20	49	− 6,3	− 6,6		−13,2	− 8,8	−19,5			
8.	10 45	47½	− 6,1	− 5,8	47½	−12,8	− 8,2	−19,5	29,5	39,1410	60′ to 5′+
	0 10	49	− 6,1	− 6,5	49	−13,0	− 8,8	−19,75			
9.	10 55	48	− 6,15	− 5,8	48	−12,8	− 8,25	−19,9	29,7	39,1410	60′ to 6′
	0 15	50	− 6,5	− 7,0	50	−13,4	− 9,2	−20,4			

	Sid. Time.	Little Therm.	Ball.	Brass Rod.	Great Therm.	Steel.	Copper.	Wood.	Barom.	Length of simple Pendulum.	Arcs of Vibration.
1743,	H. ′	°	Rev.	Rev.	°	Rev.	Rev.	Rev.			
Oct. 10.	10 25	49	4 6,2	4 6,3	49	4 13,0	4 8,5	4 20,2	29,8	39,1405	12′ to 2′
	22 10	50	−6,2	− 6,5	49½	−13,0	− 9,0	−20,25			
11.	11 0	47+	−6,0	− 5,5	47+	−12,5	− 8,1	−20,25	30,1	39,1422	12′−to 2′−
	21 40	49½	−6,5	− 6,9	49½	−13,0	− 8,8	−20,5			
12.	12 0	48	−5,8	− 6,1	47−	−12,55	− 7,8	−20,3		39,1417	11′ to 2′−
	12 15	48	−6,15	− 6,1							
	23 5	48+	−5,85	− 6,0	47	−12,5	− 7,85	−20,7			
13.	11 20	44	−5,2	− 4,6	44	−12,1	− 7,1	−20,5			
Then *rod taken down and altered before set up again.											
Dec. 28.	18 0	33	3 31,2	− 3,2	34	−12,0	− 5,8	−16,5		39,1462	14′ to 2′½
	5 35	35−	−31,5	− 2,5	36	−11,4	− 5,2	−21,0			
29.	16 0	33	−31,1	− 1,7	34	−11,1	− 4,7	−22,5			55′ to 2′½
Small ball.											
30.	23 30	35	0 11,5	− 2,5		−11,3	− 4,5	−24,5		39,1414	50′ to 1′½
1744,											
Jan. 4.	21 40	33	−16,7	− 1,8		−11,3	− 4,7	−27,5	29,6	39,1460	57′ to 2′
20.	2 0	42	2 1,5	− 4,3		−12,2	− 6,6	−29,5	30,2	39,1406	48′ to 2′−
Great ball.											
21.	18 0	40½	3 32,1	− 3,6		−11,9	− 6,2	−29,5	30,0	39,1490	15′ to 2′½
	6 0	41½	−32,2	− 4,1		−12,0	− 6,4	−29,3			
July 20.	12	70	4 1,9	−12,8		−15,9	−14,0	−18,9	30,0	39,1343	14′ to 4′
	19 30	68½	− 1,3	−12,2	68−	−15,4	−13,2	−18,4			
21.	7 0	65½	− 0,8	−10,6	64	−14,9	−12,4	−17,5	30,0	39,1355	17′ to 2′+
	19 30	68	− 1,0	−11,6		−15,0	−12,8	−15,2			
27.	7 10	69½	− 1,9	−12,2	68	−15,7	−13,7	−16,6	30,0	39,1332	17′ to 3′
	20 0	72½	− 2,0	−13,5	72	−16,2	−14,8	−16,8			
Small ball.											
28.	7 0	70+	0 32,6	−12,2						39,1244	53′ to 2′
	10 30		−33,3								
	14 30	74½	1 0,3	−13,8	72	−16,5	−15,1	−12,9		39,1249	56′ to 2′−
Aug. 2.	13 30	65	− 0,5							39,1292	50′ to 3′
	17 10	66½	− 2,5	−11,4		−15,4	−13,0	−15,0			
3.	8 30		− 3,8							39,1305	50′ to 2′−
	12 30	66½	− 3,5							39,1326	50′ to 2′−
	16 0	66½	− 4,9	−11,2		−15,4	−13,0	−16,8			
Great ball.											
4.	7 5	64	4 0,8	−10,3		−14,9	−12,1	−16,8		39,1374	19′ to 2′
	20 0	65½	− 0,9	−11,0		−15,1	−12,7	−17,5			
Sept. 15.		58+	3 33,6	− 8,3		−13,9	−10,3	−22,0	29,25	39,1385	19′ to 2′
			−33,8	− 8,7		−14,1					
Small ball.											
16.	9 45	59−	2 16,2						29,25	39,1339	52′ to 2′
			−15,1								
		59	−14,1	− 8,9		−14,1	−10,8	−22,7		39,1324	50′ to 5′−
26.	12	60+	−18						29,47	39,1358	50′ to 2′
			−17,8	− 9,1		−14,2	−11,0	−23,6			
Oct. 6.	13 45	53	−18,3						29,51	39,1372	50′+to 2′−
			−18,5	− 7,2		−13,3	− 9,3	−25,2			

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* It appears that the cocks which supported the pendulum were taken off and screwed on again, but no memorandum is made of any alteration of the rod on the 13th of Oct. 1743.

	Sid. Time. H. ′	Little Therm. °	Ball. Rev.	Brass Rod. Rev.	Great Therm.	Steel. Rev.	Copper. Rev.	Wood. Rev.	Barom.	Length of simple Pendulum.	Arcs of Vibration.
Great ball.											
1744, Oct. 7.		50	3 32,2	4 6,2	....	4 12,9	4 8,3	4 25,0	29,40	39,1411	23′ to 2′
			−32,4	− 5,9	....	−12,6	− 8,1	−25,0			
1745, Feb. 3.		33½	− 2,8	− 1,0	....	−10,8	− 3,8	−29,2	29,80	39,1475	25′ to 2′+
			− 3,3	− 1,8	....	−11,2	− 4,7	−29,0			
4.	19 30	34	− 3,2	− 1,2	....	−10,8	− 4,0	−28,1	29,85	39,1472	60′ to 7′−
			− 3,7	− 1,8	....	−11,3	− 4,8	−28,1			
Small ball.											
11.	21 19	35−	1 27,2	− 2,0					30,19	39,1409	60′ to 2′
	22 40		−29,0								
		....	−29,5								
12.	19 20	36	−33,3						29,98	39,1400	70′ to 2′
			−33,4								
		40	−33,6	− 4,3						39,1402	67′ to 2′
18.	20 5	33	2 2,4						30,49	39,1415	70′ to 2′
			− 2,5	− 2,1	....	−11,5	− 5,0	−24,7			
Great ball.											
July 12.		66	4 7,2	− 9,8	....	−14,5	−11,6	−21,3	30,13	39,1356	23′ to 2′½
			− 7,9	−12,0	....	−15,5	−13,6	−21,7			
13.		70	− 7,2	−11,0	....	−15,0	−12,7	−20,9	30,12	39,1344	21′ to 2′
			− 8,3	−13,0	....	−16,0	−14,3	−21,2			
16.		70	− 8,2	−10,5	....	−14,9	−12,2	−19,3	30,10	39,1342	60′ to 7′+
			− 8,5	−12,0	....	−15,5	−13,4	−19,7			
Small ball.											
29.	8 0	62	2 29,5						29,91	39,1296	65′ to 3′
			−32,0								
	12 25	62½	−32,0						29,95	39,1300	63′ to 2′
			−31,8	−10,0							
			−28,3								
Aug. 1.	6 15	65	−33,3						29,88	39,1318	63′ to 2′
			−33,3	−10,9							
1749,											
Great ball.											
Jan. 28.		34½	3 29,4	....	....	−11,2	....	−25	29,7	39,1488	22′ to 3′+
			−29,8	− 0,2	....	−11,5	....	−26,8			
29.	18 20	33	−29,5	− 1,3	....	−11,0	....	−27,7	30, 0	39,1491	22′ to 2′+
			−29,6	3 33,8	....	−11,0	....	−28,1			

The brass rod had remained immersed in snow all day with the little thermometer, which stood exactly at 32.

It appears that the brass rod lengthens about as much as corresponds to two seconds in time for every 5 divisions of the thermometer; or more exactly 0,00174 of an inch corresponds to 5 divisions of the thermometer, or 0,00348 to 10 divisions.

By taking a mean of the several experiments that were made with the great ball when the thermometer was above 62 divisions, the mean height of the thermometer comes out $66\frac{2}{10}$, and the mean length of the pendulum 39,13505.

But when the brass measure was kept in snow with the thermometer, which then

		Therm.
stood at 32, the length of the pendulum* was	39,1490	32,0
[*vibrating seconds of "solar time."]	39,1350	66,3
	diff. ,014	34,3
By the mean of 3 experiments in July 1745.	39,13472	68
2 ———— in Jan. 1749.	39,14900	32
	1428	36

Hence 36 divisions of the thermometer correspond to 0,01428 of an inch, and 10 divisions to 0,004 very nearly; or 0,01 of an inch corresponds to 25 divisions of the thermometer, or 25 divisions correspond to about 11″ per diem in the clock.

⁂ There is no detailed account of the apparatus for determining the length of the pendulum, but some particulars may be collected from incidental notices inserted in the account of the original experiments. The whole appears to have been constructed by Graham. There were two distinct methods of suspension. The smaller ball was hung by a hook to a fine fibre called *fil de pite*, the same as was used for a similar purpose by Mairan, Godin, and Bouguer. The larger ball, whose semidiameter was 1,957 inches, was screwed to a wire $\frac{1}{10}$ of an inch in diameter; this was supported on knife-edges. The lengths were determined by bringing a "steel mirror" to touch the bottom of the balls, and the distance from the axis of suspension was determined by standard rods of brass, steel, copper, and wood, with the extremities of which the steel mirror was brought successively into contact when the upper ends were coincident with the axis of vibration. Some drawings were found which contain all the particulars so exactly, that there can be very little doubt of their having been made from the apparatus: they have therefore been copied in plates III. and IV. at the end of the volume. Each division on the circumference of the screw moved the steel mirror through 0,001 inch.

The measures seem all derived from the length of the brass rod, which in a temperature of 64° was equal to 41,128 inches of Graham's scale. There is a loose paper, from which it appears that Bird compared it carefully in 1751 with the standard belonging to the Royal Society, and then determined it to be 41,125 inches long, at the temperature of 55°.

The distance of the centre of oscillation from the axis of suspension was calculated by a formula, which is the same in value "with that given by M. la Condamine, in the Memoirs for "1735," p. 538, but no allowance is made for the resistance of the air. The duration and number of oscillations were reckoned by marking "when the middle of a vibration happened at the "instant that the beat of the pendulum was heard from the clock." The whole was free, no maintaining powers appear to have been suffered to affect the motion, but it sometimes continued for many hours, as may be seen from the times at which the measures were taken, and which were generally at a short interval before the experiments began, and as soon as conveniently could be after they were finished. No regular correction is introduced for the variations of the arcs; but more dependence seems to have been placed on what resulted from the vibrations after the extent of them was diminished. The extremes have been annexed in the last column of the preceding Table: the rest of it, as far as August 4 (inclusive), was printed from a synopsis which had been drawn up by Bradley himself, and the remainder has been carefully extracted from the account of the original experiments.

MEMORANDA[s].

BEFORE the clock and instruments are landed, fix upon the apartment convenient for the principal observation of the transit of Venus, which will require an aspect open a little towards the north of the east and west points of the horizon, and likewise to the north, in order to see the sun near noon. If such an aspect cannot be had that will admit of the instruments remaining in the same place during the whole transit, the stands with the telescopes must be removed after the ingress has been observed, to such a position as will be necessary for seeing the nearest approach of Venus to the sun's centre, which may happen about noon at Bencoolen. The sun's altitude at the ingress may be near 40°, and not much different at the egress, and sun's altitude at noon will be 63°. If the external and internal contacts of Venus and the sun's limbs happen to be well observed, there will be less need of measuring the distance with the micrometer between their limbs afterwards; however, if time will permit, that sort of observation should be frequently repeated as soon as can be after the total ingress, and again near 3h. after the ingress, when Venus will be nearest to the sun's centre, and when the distance of the nearest limbs should be frequently taken, and the time noted. The diameter of Venus may be measured by the micrometer at any intermediate time, when the forementioned observations become less useful.

The clock ought to be fixed up in as firm a manner as the nature of the apartment will admit, and as near the place of observation as can be; but as it cannot be expected that you will be able to place [it] so near the telescopes as to enable the observers to hear the beats of the pendulum, if a person cannot be found qualified to tell the seconds loud enough to be heard by both the observers, the attempting to make use of both telescopes at the same time ought to be omitted, unless each observer is furnished with a stop watch shewing seconds; in which case, by setting the watch [in] motion at the instant the phenomenon happens, and going immediately afterwards to compare with the clock, the exact time by the clock may be determined; but an assistant at the clock would be most eligible, if a proper one can be procured, especially at the time of the transit of Venus; for then each observer, by taking notice what the time was when the contact seemed to happen, might, without interrupting the other, make [his own observa]tion independent of the other.

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[s] The original of these memoranda was found in a 4to book marked 176 in the library of Greenwich Observatory; there is the following note at the end of them:

N. B. These were the instructions that Dr. Bradley drew up at the desire of the council of the Royal Society, relating to my observing the transit of Venus in the East Indies.

C. MASON.

When you have made choice of the proper place to fix up the clock, the two pieces of wood (against which the back of the case is to rest) ought to be brought to lie in the same vertical plane as nearly as can be, and then the pieces that are already fastened to the back of the case being pressed against them, and the case set nearly upright by a plumbline, the pieces of wood which enclose the bob of the pendulum may be taken away, and the pendulum left to hang freely; and if the point of the rod below the nut is found to lie exactly over the edge of the small brass arc, and to correspond likewise with the middle division of it, the clock will be in its proper position, and the hole for the screws with which it is to be fastened may be boated after the steel point has been driven in a little, [and the screws] may be turned till the whole is sufficiently fastened: but if upon turning the screws, the lower joint of the rod should deviate from the edge of the small arc, or the middle division, it may be brought to correspond with either, by putting a small wedge behind the back or under the foot of the clock, as the case may require, and then the whole must be screwed fixed. N. B. The pieces of wood that enclose the bob must not be loosened till the clock is placed nearly in its proper situation, and before they can be taken away the small arc must be taken off, and afterwards screwed on again.

The bob of the pendulum may be reduced to the same distance from the point of suspension, (as when it was tried at Greenwich,) if the upper edge of it be brought to the mark that is made on the square part of the rod, and the nut at the bottom of the bob be turned till the index points at the division where it stood at Greenwich.

After the clock has gone for several days, and been compared with the sun or stars, so that you can well determine how much it loses in a sidereal day, the bob may be let down so much as will be necessary to make it go nearly solar time, at Bencoolen. N. B. One turn of the nut will alter the going of the clock about 28″ per day, each whole division on the nut corresponding to about 1″ per day. While the clock is going with the same length of the pendulum as it did at Greenwich, notice should be taken every day at what division [the] quicksilver stands in the small thermometer, which is to be hung within the clock case. N. B. It will be proper to set down how many turns of the nut, and how many divisions you alter it, in order to bring the clock to go nearly solar time at Bencoolen. N. B. The bob will require to be let down about three revolutions of the nut, to make it go solar time at Bencoolen. It may be most convenient to adjust your clock to go solar time, if you compare its motion by equal altitudes of the sun. And when the equal altitudes are taken of the stars whose right asc. are given, you will find how much the clock is too fast or too slow for solar time, by comparing the time of the star's passage over the meridian with its right asc., and that of the sun's computed for the meridian of Bencoolen.

As the exact time of the first contact of the limbs of the sun and Venus is somewhat uncertain, you must begin to look for it some time before the earliest compu[ta]tion gives it, which is about 7′ after nine of the clock in the morning of the sixth day of June; and if both telescopes can be made use of, and either observer should have occasion to alter much the position of his telescope, the other ought to avoid altering his at the same time, that one at least may always be looking on that part of the sun's limb where the first contact will happen. By computation the first contact will be about 48° from the apparent lower limb on the right hand, or near the middle point, between the vertical circle passing through the sun's centre, and an horizontal line passing through the same. Venus entering the sun's disc on its following limb more southerly than the parallel to the equator drawn through the sun's centre.

CORRESPONDENCE.

SIR,

I SEND you the following account of our observations of the sun's eclipse, November 27, 1722.

The day preceding being Monday, November 26th, several observations were made of the altitudes of stars on the prime vertical in the east for the more exact correction of the clocks; and by many repeated observations of the Lucida Pleiadum, and the Clara Pedis Aust. Aurigæ, none of them giving the time above 6″ or 7″ different from any other, it was discovered that about six o'clock that evening the clock was two minutes and about nineteen seconds too slow for mean time.

The evening after the eclipse the like observations were again repeated, and it having been all this day, and for some time before, a very bitter cold frost, the clock was then found to gain in the space of twenty four hours about 7″ of mean time, and the error was found on the 27th, at six at night, two minutes and about twelve seconds; so that I fix the true error of the clock, at the time of the eclipse, to have been about 2′ 15″ too slow, and by this the following observations are all corrected.

	Time by the clock.	True mean time.	
A. M.	11 30 0	11 32 15	The sun's body was viewed with a twenty-two foot telescope, and a small spot was observed upon the west limb thereof.
	12 32 0	12 34 15	An accident happening this very morning to my micrometer, I could not very exactly determine the situation of this spot; however, measuring it as accurately as we could, we found its declination was about 14′, or rather less, south of the north limb of the sun; and about 18′, or rather more, north of the south limb; and it preceded the west limb in right ascension not quite one minute, about 3″½ in time.
	1 23 0	1 25 15	Till this time we had some flying clouds, and

then the eclipse was begun, and I judged above half a digit was covered. The micrometer not being in so good order as I wished, I determined to make my observations mostly with the cross and diagonal hairs, which you know I make use of, and need not explain to you.

In all my observations I made the preceding or western cusp run along the parallel of right ascension, and it passed alway at the centre of my cross hair, so that in all cases the seconds lapsed between its passage and the passage of any other point over the perpendicular, do duly reduced express their difference of right ascensions; and the seconds lapsed between the passage of the said perpendicular by any other point and the passage of the same point over any of the diagonals north or south, do in like manner, duly reduced express the difference of declination of the said point from the said preceding or west cusp.

First Phasis.

Time by the clock.	True mean time.	
1 32 22	1 34 37	The preceding or west cusp running along the parallel of right ascension, crosses the perpendicular at the centre.
59	35 4	The last or east cusp passes perpendicular.
		The last cusp passes the south diagonal.

Second Phasis.

1 41 31	1 43 46	The preceding cusp passes perpendicular.
42 21	44 36	The last cusp passes perpendicular.
38	53	The last cusp passes south diag.
43 43	45 58	The spot passes the first south diag.
47	46 2	The spot passes the perp.
51	6	The spot passes the second south diag.

Third Phasis.

The eclipse advancing, the west or preceding cusp gained very fast to the northwards, so that now the last or east cusp was considerably southward thereof, and the spot passed the perpendicular now before the last cusp could attain to the south diagonal.

1 56 26	1 58 41	The preceding cusp passes perpendicular.
57 40	59 55	The last cusp passes perp.
58 37	2 0 52	The spot passes the first S. diag.
40	55	The spot passes perp.

Time by the clock.	True mean time.	
1 58 43	2 0 58	The spot passes second S. diag.
57	1 12	The last cusp passes the S. diag.
		Fourth Phasis.
2 3 10	2 5 25	

The distance of the cusps was taken grossly by the micrometer, and the mark was at 55 rev. 40pts. = and about this time the preceding cusp was nearly in the same declination with the spot.

Fifth Phasis.

The preceding cusp was now got to the southward of the spot.

2 5 44	2 7 59	The preceding cusp passes perp.
7 14	9 29	The last cusp passes perp.
58	10 13	The spot passes the first N. diag.
8 0	15	The spot passes perp.
2	17	The spot passes the last N. diag.
21	36	The last cusp passes the S. diag.
		Sixth Phasis.
2 19 42	2 21 57	The distance of the cusps was taken again grossly by the micrometer 59 rev. 10pts. =
		Seventh Phasis.
		About this time was the greatest obscuration.
2 32 32	2 34 47	The first cusp passes the perp.
34 36	36 51	The last cusp passes perp.
38	53	The last cusp passes the S. diag.
43	58	The spot passes the perp.
35 25	37 40	The spot passes the last N. diag.
		Eighth Phasis.
2 37 0	2 13 15	At this time the cusps were nearly in the same declination.
		Ninth Phasis.
		The last cusp now north of the first.
2 39 16	2 41 31	The preceding cusp passes perp.
41 16	43 31	The last cusp passes the perp.
18	33	The spot passed the perp.
34	49	The last cusp passes the N. diag.
42 10	44 25	The spot passes the N. diag.

Tenth Phasis.

Time by the clock.	True mean time.	
2 44 0	2 46 15	At this time the spot and the last cusp had nearly the same right asc.

Eleventh Phasis.

2 49 15	2 51 30	The preceding cusp passes perp.
51 2	53 17	The spot passes the perp.
3	18	The last cusp passes the perp.
44	59	The last cusp passes the N. diag.
52 7	54 22	The spot passes the N. diag.

Twelfth Phasis.

The sun growing very low, and appearing very tremulous from the vapours of the horizon, I gave over making these observations with the cross hairs and twelve foot tube, and I put out an excellent 22f. glass in the garden, where I could see within 2° of the horizon, in order to observe exactly the end, which I saw with great exactness.

3 19 0	3 21 15	By estimation near two digits were still covered.
25 0	27 15	Now we judged not quite a digit was covered.
32 15	34 30	The eclipse was ending, but still visible.
20	35	Certainly ended.

I wonder you did not see the rockets, they all succeeded very well, and were fired as follows, on the stone which you know I have on the garden wall for rectifying my meridional instrument.

First	5 56 47 by the clock.	Fifth	17 24 5 by the clock.
Second	6 2 1	Sixth	22 36
Third	6 54	Seventh	27 34 5 bad.
Fourth	12 5 6	Eighth	32 29

I will have some more fired in a little time on Richmond-hill, which may be you may see. I have not yet heard where they were seen.

I am, sir, your most humble servant,

1st December, 1722. S. MOLYNEUX.

⁂ There is no direction to this letter. At the end of it there are some numbers of Bradley's, but above them there are others in Pound's handwriting. It may therefore have been written to Pound, and have passed with his other papers into the hands of his nephew.

[To the Rev. Mr. Professor Bradley.]

London, July 15, 1732.

Dear Sir,

HAVING had an account of the success of our experiment at Jamaica by Mr. Harris, whose ill state of health obliged him to return to England, I believe you will not take it amiss to be acquainted with the simple fact, till all the circumstances of the observations are put into your hands. When the pendulum was reduced to the same length it had here, saving what was owing to the differences of heat, it went slower there than at London two minutes and about six seconds in a sidereal day; it was observed from the 22d of January to the 20th of February with great care and exactness. If you would have a copy of the observations sent you to Oxford, be pleased to let me know, for I have one by me; Mr. Harris has the original, which you will have when you come to town.

I am sir, your most humble servant,

GEORGE GRAHAM.

[To Mr. Graham.]

Oxford, July 18th, 1732.

Dear Sir,

I AM glad to hear that the experiment has succeeded so well in Jamaica, and could have wished Mr. Harris's health had permitted him to have made a longer stay: I hope the like misfortune does not attend Mr. Campbell.

The account you have favoured me with seems to confirm sir Isaac Newton's opinion, grounded on the like observations, that the equatorial parts of the earth are somewhat higher than the computation from the theory of gravity makes them, upon the supposition of an uniform density.

I should be much obliged to you for a more particular account of the circumstances of the observations, and also of the state of Mr. Hauksbee's thermometers in the greatest degrees of heat and cold of our climate.

I doubt not but you have observations by you proper to determine the greatest alteration that our summer's heat and winter's cold can occasion in the going of a clock; which, joined with the other particulars, would probably be sufficient to settle this matter, so as to leave little room for any farther dispute about it.

I would willingly see what the result of the whole will be before I have the pleasure of meeting you in London, which may not be till towards the end of August. If the copy of the observations be too long for a post-letter, Mr. Innys can convey any papers to me with safety.

I am, sir, your most obliged humble servant,

J. BRADLEY.

[To the Rev. Mr. Professor Bradley.]

Sir, London, July 22, 1732.

I HAVE sent you the copy of the observations I had of Mr. Harris; he would have given me the original, but I desired him to keep it in his own hands till he should see you. I shall in a week's time send you some account of the thermometer here, by which you will better judge of the difference between the two places. My present opinion is, that the clock could not go above 10″ slower there than here.

I am, dear sir, your most humble servant,

GEORGE GRAHAM.

[To the Rev. Mr. Professor Bradley.]

Dear Sir, London, July 25, 1732.

I SHALL now put down the reasons upon which I grounded my conjectures, that the difference of heat could not cause the clock to go above ten seconds slower at Jamaica than at London. The spirit thermometer that was sent to Jamaica with the clock was numbered downwards, so that the higher the number the colder. When it hung in my room, two pair of stairs next the street, which fronts the south, and where a fire was constantly kept every day, and the time it was observed was after the fire was made, must have caused it to stand higher than it would if it had been placed in the north side of the house and near no fire. In January 1730, it was at $65\frac{1}{2}$, which was the lowest I observed it at; and my quicksilver therm. at 24 below the temperate point. May 9, 1731, the spirit therm. was at $28\frac{1}{2}$, and the quicksilver one at 16, above temperate. After this day I kept no account of the spirit therm. whilst it hung in the fore-room, for I had then taken down the clock to finish the dial-plate of it, and had placed it in the back-room to prevent the pendulum from being disturbed by the coaches. You have in the paper I gave you the mean height of the therm. in the back-room at the time I observed the transits of the star, which was $28\frac{1}{2}$. The mean of the quicksilver therm., for the ten days the clock was observed, was $22\frac{1}{2}$ above: from the proportionate changes of the two thermometers, the spirit one would have stood at $22\frac{3}{4}$ if it had been in the fore-room. August 1st, the quicksilver therm. was at $36\frac{1}{4}$; by the same proportion the spirits would have been at 11 in the same room. To find the proportionate alterations of the two thermometers, I took the mean heights of twenty days for each, which were 23,6 to 20,6; and then the mean of five days for each, which were 36,54 to 32; which two proportions are nearly the same. I never had the opportunity of observing one of Mr. Hauksbee's thermometers before; that which I had made use of before for above twenty years made a very different scale from his, and which was broke a few years ago by an accident. The alteration from 65 to 11 is 54 divisions, and, had it been in a room without a fire, I believe it would have been lower above six divisions.

As 30″ a day is the most that I have observed a clock to alter when exposed to great degrees of heat and cold, I am of opinion, that allowing one second of time for two divisions of the thermometer is somewhat too much, but cannot differ much from the truth. Should we therefore allow the mean height at Jamaica to be about 8 or 9, which I think is manifestly too much; there would be a difference of 20 divisions between the two places, which at 2″ to a division would amount to 10″ in time. I observe, that in the paper I sent you, the heights of the thermometer in the morning, about the same hour, differ not above six divisions; and the mean of the morning and evening heights is about 13 for the whole time. Mr. Harris tells me the nights are cool, especially about four o'clock in the morning; but the heat in the day exceeds the cold of the night, which inclines me to think we should estimate the mean of the whole about ten or eleven divisions.

As Mr. Campbell continues to observe the clock and thermometer, we may be better enabled, by the next account he sends us, to determine this affair; however, we cannot be mistaken, I think, above 4 or 5 seconds at most.

Mr. Machin was with me the other day to inquire about this experiment, and told me that sir Isaac Newton would have been much pleased with it had he been living. Mr. Campbell, in his letter to me, gives his service to you in a very particular manner, and tells me he has not had so much as the headache since he arrived at Jamaica, and is much concerned that the place disagreed so with Mr. Harris. They had just finished their observatory about a week before my clock got there, so that every thing was ready for making the observations as soon as they received it; and no care seems to have been wanting on their part.

I am, sir, your most humble servant,

GEORGE GRAHAM.

[To the Rev. Mr. Bradley.]

London, July 31st, 1732.

Sir,

I AM now going out of town, and having spoke to Mr. Graham, he advised me to leave these observations we made at Jamaica, relating to the clock, sealed up for you. You have the rough journal just as we kept it; perhaps some of the notes in these papers may be incorrectly written, but my ill state of health at Jamaica would not permit me to be more accurate therein, though I took care that the materials are strictly exact; and I beg leave to assure you that these observations were made with the utmost care.

I hope, sir, to see you in town before these are laid before the society. Mr. Campbell hinted it to me, if something should be said in favour of what we have done, it would be of some sanction to him in that part of the world where astronomy is in but little esteem.

We made some few observations of the eclipses of Jupiter's first satellite. I shall look them all out when I come to town. In the mean time I beg leave to subscribe myself,

Sir, your most obedient humble servant,

JOSEPH HARRIS.

Mr. Campbell desired his service to you, and thanks for the favours we received at Wansted.

[To the Rev. Mr. Bradley.]

Tower-street, London, Nov. 24, 1733.

Dear Sir,

I WAS very sorry that I did not see you when last in town, because I wanted very much to have conversed about the experiment made in Jamaica, which I hear you have considered, as indeed I have also done. If the pendulum went slower there than here by 2′ 6″ in a sidereal day, and only 9″ or 10″ are to be allowed for the lengthening of it by heat, as Mr. Graham tells me, thence it would follow that the earth's diameters are as 189 to 190, or thereabouts, in which case the force of gravity at the equinoctial would be to the centrifugal force as $237\frac{1}{2}$ is to unity; which is impossible, unless the diameter of the earth were above 9000 miles, and that differs so much from the measures of Norwood, Picart, and Cassini, that it cannot be admitted, nor consequently the experiment from whence it is deduced: and besides, I can prove from undoubted observations in astronomy, that Cassini's measure is very near the truth, for the diameter of the earth can be found surer by them than by any actual mensuration. If 29″ could be allowed for the lengthening of the pendulum by heat, this experiment made at Jamaica would agree with other things, but Mr. Graham says that he cannot allow that by any means. I am very far from thinking that the experiment was not exactly made, and indeed a greater absurdity would follow from Richer's experiment made in the island of Cayenna, which is the only one that can be depended on, which is mentioned in sir Isaac's Principia.

Although I have treated of the problem about the figure of the earth in a manner which is new, yet I am still obliged to suppose the figure of it to be an exact spheroid, and although I be sensible that this supposition is not sufficient to determine the number of vibrations to 8″ or 9″ in a day, yet I know that the error cannot be so great as the Jamaica experiment makes it. If Mr. Graham be certain that not above 10″ can be allowed for the heat, it is as certain either that the mountains have a sensible effect on the pendulum, or some other thing, which will render the experiment entirely precarious.

I find that sir Isaac in his 3d edit. Princip. mentions three observations of Dr. Pound, which make Jupiter's diameter about 37″; I want to know if that be the greatest diameter of Jupiter; because if it be, then the lesser would be about 34″, which would make too great an odds in the thing for which I want it. And I should be glad to know if you can help me to any observation which ascertains the moon's middle distance from the earth, which I could depend more on than the common ones; if you could inform me of these things, I should be able quickly to make an end of what I shall say about the figure of the earth, which I would the more willingly do, because not only Mairan, but also Hugens, Herman, and Maupertuy, have all of them entirely mistaken the matter. I heartily wish you all happiness, and the sooner I hear, the more you will oblige,

Sir, your most humble servant,

J. STIRLING.

[To Mr. Stirling.] 1733.

DEAR SIR,

WHEN I was last in London, an unexpected accident obliged me to return hither sooner than I intended, and hindered me of the pleasure of waiting on you, which I purposed to have done, having been informed that you were then examining into the dispute concerning the figure of the earth. Not that I had much more to tell you, than what is contained in the account of the Jamaica experiment, which I left with Mr. Graham, wherein I have stated the fact as well as I could, and made such allowance for the lengthening of the pendulum, as former observations and experiments would warrant; and the result of all seemed to be that the clock went $1' 58''$ per diem slower in Jamaica than at London. I allowed only $8''\frac{1}{2}$ on account of the different degrees of heat, having no authority from former experiments to make any greater abatement; so that I apprehend this retardation of the clock (so much greater than what is derived by computation founded on the principles of gravity, and an uniform density in the several parts of the earth) must be rather ascribed to an inequality in the density of the parts of the earth near which the clock is fixed, than to the greater heat. As the greatest part of the force of gravity upon any particular body arises from the parts of the earth that are near it, (the action of remote particles being very small,) does it not thence seem likely that a body placed over or near a great quantity of rarer matter, as water, will not be attracted with so great force as if it were in the midst of a large quantity of denser matter, as in a great tract of land; and may it not thence follow, that in the same latitudes clocks, when placed on continents and islands, or on larger and smaller islands, will not go alike; or may not mountains, (as you observe,) according as they are made up of matter more or less dense, contribute something towards such inequalities? Upon this account 'twill be necessary to have a greater variety of exact experiments made in different places, both in the same and different latitudes; and I have therefore in forementioned paper proposed to have the experiment repeated in several places, in order to discover whether the density of the different regions be uniform or not, for till that point is a little settled, we may be at a loss to assign the true cause of this difference between the theory and experiment.

As to the diameters of Jupiter, I find from the mean of several observations which I made with the Royal Society's glass of 123 feet, that the greater diameter is to the less (both being measured with a micrometer) as 27 to 25. The greatest diameter at Jupiter's mean distance from the sun being just 39″. This is the case when the diameter was actually measured with the micrometer, but by other observations of the time of the passage of some of the satellites over Jupiter's disc, compared with their greatest elongations taken with a micrometer, the greatest diameter comes out 37″ or 38″, the difference, as I conceive, arising from the dilatation of light.

As to observations proper to determine the mean distance of the moon from the earth, having never made any myself with that view, I can give you no assistance therein; but I believe Mr. Machin has examined that point, and fixed it with all the accuracy that the best observations we have would allow him to do it.

You should have had my answer sooner, but that I was engaged in a course of lectures; upon the conclusion of which, I took the first opportunity of assuring you that I am with great respect,

Sir, your obedient humble servant,

J. B.

[To Dr. Smith at Cambridge.]

SIR,

I AM sorry it has so happened that I could not give a more speedy answer to your letter, which I received a little before I came from Oxford to this place; I had purposed soon after my arrival here to write to you, but was unhappily prevented by the sickness and death of a young gentleman of the family I am now in, upon which occasion I was obliged to go for some days into Berkshire. But lest a longer delay should prove any disappointment to you, I embrace the first opportunity I have of telling you the occasion of it.

I heartily wish I were now able, though thus late, to give you a satisfactory account of the particulars you inquire about; but fear what I can say upon the method of grinding and polishing speculums will not be of much use to you, having never made any memoranda in writing concerning the practice with Mr. Molyneux. 'Twas indeed upon this account that I deferred answering your letter till I came to London, designing to have consulted Mr. Hadley, who I know is best able to inform you, but since that would still occasion a longer delay, I venture to trouble you with such hints as the recollection of what we then did, and my present thoughts upon the method in general, will suggest to me.

And first with regard to the convex and concave plates, mentioned as necessary for grinding and polishing the speculum; though Mr. Molyneux used brass or marble, yet I believe a mixed metal, nearly of the same kind with that of the speculum itself, or a little coarser, would be much more easily worked into the required figure than brass, and would retain its figure better than marble, which might perhaps be somewhat affected by different degrees of moistness, &c.

Upon a convex plate of the same kind of mixed metal I should likewise advise the blue stones to be cemented; and if their figure were restored by a large concave plate of the same mixture it might not be improper; though this last I think almost needless, since the speculum itself will keep the figure of the stones as it ought to be, by varying the stroke of the hand, as is mentioned in the account you have sent me; only care must be taken that this be done very gradually, otherwise the figure of the speculum will not be truly spherical.

I think there can be no doubt but that a convex plate of the same mixed metal will be likewise very proper to fix the sarcenet on for the last polisher; for the figure of this must be as true as can be made, and not liable to alteration. Mr. Hadley, as I remember, made use of a large piece of glass for this purpose, but 'tis not easy to procure glass large enough, and therefore I should recommend the sort of bell-metal as before mentioned. And the last polisher being once made in the manner you describe, (with sarcenet daubed over with a pretty good coat of the solution of pitch,) its figure is supposed to be constantly the same, and that of the speculum must be altered by grinding it upon the blue stones always till it agrees with the polisher, which is easily effected by the different stroke of the hand, so that the plate for the last polisher should properly be first of all finished; and for this purpose it might be convenient to have a concave plate, and blue stones to finish it with; and perhaps the same concave stones would give the truest figure to the polisher, if the pitch on the sarcenet were laid on thicker than ordinary, and then ground with them. The figure of these concave stones may be kept the same, or altered at pleasure, by grinding them with the convex stones, at least this method may be hinted at for giving a true figure to the polisher, after you have described that with soap and putty, which Mr. Molyneux used.

As to the last article, about making use of a bruiser every time fresh putty is put upon the pitch, there will be no occasion for it, provided the putty be very fine and well washed, and there be a pretty thick body of pitch on the sarcenet; neither in this case will it be necessary towards the last to let the polisher grow dry, or wet it only with breathing, for it may be continually kept very wet, and the lustre will increase as far as the hardness of the metal will permit it. 'Tis best to put but a little putty at each time, and not to hasten on the polish too fast; for much putty will spoil the figure of the speculum. When the polisher is kept very wet, it does not require so much force to move the speculum as is intimated in your last paragraph, for then a speculum of five or six inches may be moved without the help of the engine described before, though the use of it even in such speculums may be very convenient. If these hints may be of use to you, be pleased to conceal the name of the author of them, and you will very much oblige.

If upon talking with Mr. Hadley I learn that he has any thing of more use to communicate to you on this subject, I shall desire he will do it as soon as possible.

If you think proper to make any alteration in the directions that you have already drawn up, and to insert any of the particulars that I have now intimated to you, I desire it may be without mentioning from what hand you had them, and you will much oblige your

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⁂ Bradley's letters are printed from the rough copies which he kept of them: this will account for the omissions with respect to signature, dates, and directions. In the present instance however it may be collected from what is said in the beginning, that it was probably written from Wansted in the beginning of 1733. For Matthew, the son of Bradley's friend, Mr. Wymondesold, died December 24, 1732, and was buried at Lockinge, near Wantage.

[To Mr. Professor Bradley.]

De Paris, 27 Sept. N. S.
rue͏̈ Ste. Anne, quartier St. Roch.

Sir,

THE rank you hold among the learned, and the great discoveries with which you have enriched astronomy, oblige me to give you an account of the success of an undertaking wherein the sciences are much interested, even if I were not inclined to do it through the desire which I have of the honour of your acquaintance, on account of the part you bear in our work itself; we being indebted for a principal part of the accuracy thereof to an instrument made upon the model of yours, and towards the construction of which, I understand, you were pleased to give your assistance.

I shall therefore, sir, do myself the honour to tell you, that we are now returned from the voyage (which we made by order of the king) to the polar circle. We have had so much good fortune as to be able (notwithstanding the rigour of the climate) to measure from Torneå towards the north, a distance of 55023,47 toises in the meridian.

A base (the longest that has ever yet been employed in this kind of operations, and measured upon the most smooth surface, ice) was situated in the middle of eight triangles, by which we determined this distance. The small number of these triangles, and the situation of this long base in the middle of them, seemed to promise us a great degree of exactness, and left us no room to fear that the errors could considerably increase each other, as may be apprehended in a much longer series of triangles.

We afterwards determined the quantity of this arc by δ Draconis, which we observed at the two extremities, with the sector, which you are acquainted with.

We observed this star first at Kittis, one of the extremities, on the 4th, 5th, 6th, 8th, 10th of October 1736. Immediately after, we carried our sector to Torneå in a boat, and with all necessary precaution, that there might not happen any alteration in it; and we observed the same star at Torneå, on the 1st, 2d, 3d, 4th, 5th of November 1736.

We found, by comparing the first and last observations, that the quantity of our arc (without making any other correction than what the precession of the equinox requires) was 57′ 25″,07; but allowing the necessary correction, according to your theory of the aberration occasioned by the motion of light, this quantity (on account of the time lapsed between the middle of the observations) ought to be greater by 1″,83; so that our quantity should be 57′ 26″,9.

We perceived immediately that a degree of the meridian under the polar circle, was much larger than that which had been measured formerly about Paris.

About the vernal equinox of the following year we renewed this whole operation; we observed at Torneå α Draconis on 17th, 18th, and 19th of March 1737, and afterwards we departed for Kittis. Our sector was carried this time upon the snow in a sledge, which went only a foot pace; and we observed the same star on the 4th, 5th, 6th of April 1737. We found by the observations at Torneå, and those at Kittis, the arc 57′ 25″,19; to which adding 5″,35 for the aberration of this star during the time lapsed between the middle of the observations, we have for the amplitude of our arc 57′ 30″,54; which differs from that found by δ, 3″½.

Taking therefore a mean between the two (amplitudes), our arc will be 57′ 28″,72; this, compared with the distance measured upon the earth, makes a degree 57437,1 toises, which is greater than the mean degree in France by 377,1 toises.

We believed the verification resulting from this agreement between our two arcs, deduced from two different operations, (considering also the care and precaution which we took in carrying the sector,) was more certain than any other which we could have made; especially as by the construction of our instrument it did not seem to be designed to be turned from side to side, and as we did not want (for our operation) to know exactly the point of the limb which corresponds to the zenith.

We afterward verified the arc of 5°½ of our instrument, by means of a radius of 380 toises, and a tangent, measured upon the ice; and, notwithstanding the high idea we had of Mr. Graham's ability, we could not but with astonishment see, that taking the mean of the observations made by five observers, who agreed extremely well with each other, the arc of the limb did not differ but 1″ from what it ought to have been according to its construction. In the last place, we compared the degrees of the limb with each other, and we were surprised to find, that between the two degrees which we had made use of, there was a little inequality that did not amount to 1″, and which will even bring the two arcs, which we found, nearer to one another.

You see then, sir, by actual measurement, the earth is an oblate spheroid as it ought to be according to the laws of Statics; and this oblateness appears to be even more considerable than sir Isaac Newton thought. I likewise am of opinion, both from the experiments which we have made in the frigid zone, and from those which our academicians have sent us from the equator, that the gravity augments more towards the pole, and diminishes more towards the equator, than sir Isaac Newton has supposed in his Table. And all this is agreeable to the reflections which you have made upon the experiments of Mr. Campbell in Jamaica. But I have one favour to ask of you, sir, and hope you will not refuse it me; and that is, to tell me whether you have any observations actually made of the aberration of the stars δ and α themselves, and whether we have made use of the proper correction for this aberration.

I shall have the honour to communicate to you hereafter our experiments relating to gravity, and the particulars of all our operations, when they are printed.

I have the honour to be, (with the greatest esteem,)

Sir, your most humble and most obedient servant,

MAUPERTUIS.

I shall be very much obliged, sir, if you please to communicate my letter to the Royal Society.

⁂ This was evidently written in 1737, and that year is found in the copy of the original French, which is preserved in one of the letter-books of the Royal Society. The translation is tolerably close; there is however one variation which may be worth noticing. In p. 404, there is mention of "your theory of the aberration;" Maupertuis's expression is "votre belle theorie," which Bradley had at first rendered "your curious theory," but he afterwards struck the epithet out altogether.

[Probably to Mr. Graham.]

Dec.

DEAR SIR,

MR. MAUPERTUIS having done me the honour to communicate to the Royal Society, through my hands, the observations which the French gentlemen made in the north, as I apprehend his account can be to none more agreeable than to you, who had so much trouble in the affair, and to whom we are principally indebted for the accuracy of the experiment. I shall therefore take the liberty of sending you the contents of his letter, as nearly as my small skill in French enables me to guess at it, imagining it may afford you some satisfaction, at least, till you can hear a more exact translation of it read before the Society; I having sent the original to Dr. Mortimer for that purpose.

After the introduction (which is wholly complimental) he proceeds thus:

I shall therefore, &c.

This account you see, sir, must entirely put an end to the dispute as to the figure of the earth in general: as to the observations of the stars made with the sector, I imagine you still wish, with me, to have that more particular detail of them promised in the last paragraph; for when we have each night's observation, we shall be better able to judge of their expertness in managing it, and whether the experiment had all the advantage of accuracy which so curious an instrument was capable of yielding.

The method which they took to verify the quantity of the arc, by making observations on different stars, and carrying the instrument twice backwards and forwards, does certainly remove all scruples relating to that point. But as they were at all that pains, might they not, with a little additional trouble, have been yet better assured of the true quantity of their arc, by turning the whole apparatus on which the instrument hung from west to east, &c. by which means they would have had on the arc of the sector the double distance of the star from the zenith at each station, and likewise have removed all objections relating to the alteration of the line of collimation, upon carrying the instrument from one station to another. Mr. Maupertuis, indeed, seems to intimate that the construction of the instrument was such as not to admit of being turned from side to side, or rectified in the manner I mention, but I thought it was made in the same manner with mine, with an arc of about 2°½ on each side the line of collimation, so as to be capable of taking a star either 2°½ to the north, or 2°½ south of the zenith; and if it was, they might certainly, without much trouble, have observed each star both ways, at each station, which would in effect have doubled the number of observations. If I am mistaken in this point, I shall be obliged to you for a line to set me right by the first opportunity, as likewise for your opinion as to the use the French gentlemen ought to put the sector to, upon its arrival in France; viz. whether they should not take the difference of latitudes with it, between some places in France that lie on their famous meridian, whose distances have already been measured. For then there can be no objection made as to the true quantity of angle observed, if it be taken with the same instrument in France as under the polar circle; and this may likewise be a means of discovering wherein the error of the observations formerly made in France lies; viz. whether in taking the true difference of latitude, or in measuring the exact distances of places by the triangles, since the wrong conclusions drawn by Cassini, relating to the different magnitude of a degree in the south and north part of France, may have taken their rise from either of those causes. Or if you think this unnecessary trouble, whether they should not be desired to examine with their sector into the motion of the fixed stars, and how far it is conformable to my observations and hypothesis about it. As I must soon send Mr. Maupertuis an answer to his request about the quantity of aberration on account of light in those stars which they observed, (which indeed they have already rightly computed,) I should be glad to hear from you before I write to him, and have your opinion as to such points as you may think proper to be suggested to them relating to the future use of their sector, and whether I should not at the same time intimate to him the other irregularities which I have observed in the situation of the earth's axis to the plane of the ecliptic, and of the unequal precession of the equinoctial points. Your speedy answer will much oblige,

If Mr. Celsius has sent you any account, I shall be glad to hear the particulars, as likewise your sentiments about their method of examining the true quantity of the whole arc of the sector.

Not knowing where Dr. Mortimer lives, I directed the letter to Crane-court, on which account I fear it will not reach him so soon as he desired, for he intended to have got it translated against the first meeting of the Society; but if you have an opportunity, I beg you will be pleased to let him know where it lies, as soon as you can conveniently.

[To Mr. Maupertuis.]

* * * * *

And on this occasion, sir, I beg leave to do myself the honour to tell you what I have farther discovered, relating to some other changes that happen in the apparent situation of the fixed stars.

In my account above referred to, I have taken notice that, so far as I could then judge from the observations of a single year, the annual precession of the equinox was at that time greater than 50″, the stars near the equinoctial colure (between the years 1727 and 1728) changing their declination $1''\frac{1}{2}$ or 2″ more than a precession of 50″ required. On the other hand, I observed that stars near the solstitial colure altered their declination less than they ought if the precession was 50″. Which seeming contradiction in the phenomena, with regard to the stars in those different situations, I now conceive to arise from the unequal action of the moon upon the equatorial parts of the earth, which, varying on account of the different inclination of her orbit to the equator, will cause a small nutation in the earth's axis, as also an acceleration and retardation in the precession of the equinoxes. The whole quantity of this nutation, I find, amounts to about 9″ each way from the mean, the obliquity of the ecliptic being greatest when the moon's ascending node is in Aries, and least when in Libra; for by my observations γ Draconis appeared 18″ more northerly in Sept. 1736, than Sept. 1727, after I have allowed the usual difference on account of the precession of the equinox; but this year, 1737, I perceive the star is returning again towards the south, and if I have guessed rightly as to the cause, it must continue to move southerly till the year 1746, when it will again have the same situation as in 1727, allowing only for the usual alteration on account of the precession, unless it shall farther appear upon trial, that there is a gradual diminution of the mean obliquity of the ecliptic, as many astronomers suppose; and in that case the variation from the mean obliquity must be less than 9″, the quantity I determined by my observations, on supposition that the mean obliquity has continued the same for nine years last past, and that all the alteration which I have found is owing wholly to the cause I have now suggested; in which case, the period of this nutation must be the same with that of the moon's node, viz. about 19 years.

The same cause must also produce an inequality in the precession of the equinoxes, making the greatest annual precession when the ☊ is in ♈, about 57″; and the least about 42″, when the ☊ is in ♎.

Upon comparing many observations of different stars, which I have made during the ten years last past, I have not yet met with any that disagree with the hypothesis here laid down, more than what I conceive may arise from the uncertainty of the observations themselves.

But as you will now have an opportunity, by means of your sector, to examine into the several motions of the fixed stars, which I have hitherto observed with my instrument, I hope, ere long, to have the satisfaction of hearing the truth of my own observations confirmed by the testimony of so exact an observer as Mr. de Maupertuis. And, indeed, with this view it is that I have taken the liberty to trouble you with the particulars above mentioned, in hopes thereby to excite you to undertake this task; though I am sensible 'twould be unreasonable in me to expect that pleasure very soon, since I presume you will first think it convenient * * *

* * * * *

The pleasure, sir, that I have had in perusing the particulars you have already honoured me with, have so heightened my desire of seeing a more minute detail of the whole series of your observations and experiments, relating both to the measurement of a degree, and the force of gravity, that I cannot forbear requesting you to hasten, as much as possible, the publication of what will afford to all the curious the most agreeable entertainment. And, indeed, were it not for the pleasure I now enjoy in this seeming, though distant conversation with you, I could not forgive that delay to the fulfilling my wishes which I may occasion by this tedious letter; but I am sensible this consideration is too selfish to justify me in detaining you at present any longer; and therefore I shall only add, that when I have made all proper allowance for the precession and light, and all the motions I have hitherto taken notice of, I compute that 1″,38 added to the arc observed by δ Drac., and 4″,57 added to that observed by α Drac., will give the correct differences of latitudes; which corrections, I presume, scarce sensibly differ from what you have already made use of. I am, with the greatest esteem, sir,

Your most humble and obedient servant,

Oct. 27, 1737. J. BRADLEY.

[To M. Maupertuis.]

Oxford, Oct. 28, 1738.

SIR,

HAVING had the honour, since I wrote to you, of receiving two most obliging letters, and likewise your curious account of your northern expedition, I cannot but be sensible that this long delay in making my acknowledgments for them may need some apology. But before one who shews such ardent desires of discovering truth, I shall not scruple to lay this apparent defect in good manners on its real cause, viz. my desire of communicating to you the result of the observations on the fixed stars, which I usually make about this season of the year. I have already given you an account of the phenomena which induced me to think that there is a small nutation of the earth's axis, which I conceived was occasioned by the unequal action of the moon on the equatorial parts of the earth. What I can farther collect from this year's observations tends to confirm my former conjecture; the principal stars which I observe, in order to discover this motion, being now evidently returning back again with a contrary motion to that which they had during my first nine years' observations. But whether they will entirely recover their original situation by the end of the period of eighteen years, I cannot yet determine from the observations of these two last years only. There seems, indeed, from my latest observations, some reason to suppose that they will not; but the difference between the hypothesis and observations scarce amounting at present to more than a single second, I cannot pretend to say whether that is owing to any thing besides the uncertainty of the observations themselves, and therefore must refer the decision of this point to future experience.

Having given you the true motive for my long silence, 'tis time to add my hearty thanks for your most acceptable present, which afforded me an agreeable entertainment, accompanied with the highest satisfaction. So faithful and exact an account of your whole proceedings cannot fail of having a proper influence on the minds of the true lovers of knowledge; it bearing such genuine marks of care and ability in the observers, of exactness in the instruments, and propriety in the means made use of through the whole affair, that I imagine few points in practical mathematics have attained greater degrees of certainty, or merit more to be relied on, than those which are its principal subject. It has, indeed, in every part such deep characteristics of truth and accuracy, that I have not yet met with one person here to whom it did not afford the fullest conviction, and who does not express himself highly pleased with the conduct of your whole company, who, by their unwearied pains and application, so happily perfected those experiments, though attended with great and seemingly insuperable difficulties. The bare relation of these, together with the hardships and fatigues you were continually subject to, cannot but affect the mind of every common reader with a dread of their consequences, and at the same time raise in them just sentiments of gratitude to such truly heroic philosophers. But the frequent and honourable mention you make of me does not permit me to look on myself merely as a common reader, being sensible that the honour you do me calls for my most particular acknowledgments; for could I hope to make any advancement in knowledge, I should esteem its highest encomium to be the approbation of such a judge. I cannot therefore but be extremely pleased to find you express in your letter an intention to examine with your sector the apparent motion of the stars which I have formerly given an account of; for though the smallness of its quantity may not seem to merit much attention, yet as it may possibly throw new light into some branches of philosophy, it will be a satisfaction (to me at least) to have it inquired into by others; especially since I find Mr. Clairaut has so far honoured my hypothesis, for solving those phenomena, as to lay the demonstration of its consequences before your illustrious academy.

I could not without concern hear of the objections that were raised against the certainty of your observations; but the world, I imagine, must soon be convinced of their invalidity; and you, I doubt not, will have the satisfaction of finding the truth of this saying: "Magna est veritas et prævalebit."

I am, with the greatest esteem, sir,

your most humble and obedient servant,

J. B.

[To the Rev. Mr. Professor Bradley.]

DEAR SIR,

I SEND you M. de Lisle's unpublished observations of Jupiter's satellites, as I promised you. They were all taken at the Imperial Observatory at Petersburg.

N. S.	Temp. Appar.	
	H. ′ ″	
1735, May 21.	13 52 37	Immers. 1. with a 5 foot Newtonian reflector, made by G. Hearne; doubtful for the twilight, and a foggy air.
Aug. 16.	9 19 33	Emers. 1. with the great reflector; air quiet and serene.
Oct. 10.	6 31 50	Emers. 1. with the great reflector; air serene, Jupiter low.
1736, June 1.	13 30 21	Immers. 1. with the great reflector; :: to a $\frac{1}{4}$ of a min. Jupiter being low and in the twilight.
21.	12 50 0	Immers. 4. with the great reflector; :: to 1′, Jupiter being low and in the twilight.

N. S.	Temp. Appar. H. ′ ″	
1736, July 10.	14 10 39	Immers. 2. with the great reflector; : : for foggy air.
17.	13 40 43	Immers. 1. with a 15f. glass of Campani; clear air.
	40 53	With the great reflector.
Aug. 2.	11 56 42	Immers. 1. or rather an occultation of the satellite under Jupiter's disc, for the one could not be distinguished from the other; sky serene, and Jupiter near the meridian. I observed with the great reflector. The satellite, before it entirely disappeared, seemed to adhere to Jupiter's body for some minutes, and lost itself gradually under it.
22.	8 50 58	Emers. 2. with the great reflector; sky serene and quiet, a certain observation.
29.	11 31 12	Jupiter coming out of a cloud which had covered him 24′, the second satellite appeared, but was still weak; with the great reflector.
Sept. 13.	7 51 37	Immers. 4. This satellite was several minutes diminishing, whence the true time of its disappearing may be uncertain to some seconds; moreover, when it seemed just upon the point of disappearing, it recovered new light for some seconds, and then decayed away again.
23.	9 5 30	This moment Jupiter, having been hidden by a cloud for half an hour, appearing, the second satellite seemed to be got out of the shadow, but was still much smaller than the rest of the satellites. This with the great reflector; but the observation was greatly incommoded by a strong wind.
Oct. 5.	7 45 23	Emers. 1. with the great reflector; very exact, sky serene and calm, Jupiter near the meridian.
1737, July 4.	13 39 55	Immers. 2. with the great reflector; the twilight strong.

N. S.	Temp. Appar. H. ′ ″	
1737, July 9.	12 51 1	Immers. 3. with the great reflector; clear air.
22.	11 39 42	Immers. 1. with a 15f. glass of Campani.
	39 53	With the great reflector; clear air.
Aug. 7.	9 56 44	Immers. 1. with the great reflector, through interstices of clouds.
21.	12 57 17	Immers. 3. by estimation, the satellite having been covered by a cloud about 15″ before, when it was very small; with the great reflector.
28.	15 43 26	Immers. 1. with the great reflector; :: to some seconds from the nearness of the satellite to Jupiter, and the strength of the twilight.
30.	10 12 13	Immers. 1. with Campani's 15 foot glass.
	12 22	.... with a 23½ foot glass.
	12 38	.... with the great reflector; the sky calm and serene, but the nearness of the sat. to Jupiter might produce an error of some seconds.
	10 44 18	Immers. 2. with the great reflector.
Sept. 19.	8 20 30	Emers. 3. with the great reflector; fine sky.
26.	12 22 4	Emers. 1. with the great reflector.
	22 21	.... with a 22 foot glass; very fine sky.
29.	14 42 23	Emers. 1. great reflector.
	42 41	.... 22f. glass.
Oct. 8.	11 8 7	Emers. 1. great reflector.
17.	7 33 45	Emers. 1. great reflector.
	33 59	.... 22f. glass; serene sky.
19.	8 14 12	Emers. 2. 22f. glass; serene sky.
Nov. 6.	8 50 12	Immers. 4. great reflector; serene sky :: to several seconds from the smallness of the sat. and the slowness of its motion.
9.	7 50 21	Emers. 1. by estimation, great reflector; foggy. I did not see the satellite till 20″ later, but then 'twas pretty big.
13.	5 27 37	Emers. 2. great reflector; serene sky.
16.	9 44 14	Emers. 1. great reflector; fine sky.
	44 26	.... Campani's 15f. glass.
	44 28	.... 23f. glass

N. S.	Temp. Appar. H. ′ ″	
1737, Nov. 25.	6 7 35	Emers. 1. great reflector. It might be ¼ min. sooner. Foggy.
1738, Jan. 3.	4 25 32	Emers. 1. great reflector.
	45 47	………… 23f. glass; clear sky.

I send you also my observation of yesterday's eclipse at Newington.

Sid. Clock.	
7 37 35	The beginning of the eclipse; perhaps one or two seconds sooner.
9 6 40	The cusps horizontal accurately. This by the instrument for equal altitudes.
37 58	Sun's preced. limb passed.
38 43	The end of the eclipse.
40 9	Sun's conseq. limb.
19 38 41	α Lucid. Aquilæ. N. B. The clock loses about 2″½ sidereal time per diem.

I should be very glad to receive the stars with which you took the comet, and I will take the best care I can to verify their places.

When you have leisure to transcribe them, I should take it as a great favour to peruse yours and Mr. Pound's observations for the parallax of Mars in 1719; to which if you'll please to add a word or two about the choice of opposite stars that will be most affected in right ascension by the motion of light, you will still farther oblige me.

I am, sir, with sincere esteem,

your very humble servant,

J. BEVIS.

You may please to direct to be left
for me with Mr. Graham.

⁂ The post-mark on this letter is August 5th; and the eclipse which is mentioned in it shews that it was written in 1738.

[To the Rev. Mr. Professor Bradley.]

DEAR SIR, London, April 27, 1739.

I HERE give you my observations of several transits of the pole star, and two others, transcribed from our Newington Journal, and compared together in several parts of the year, which I presume may be sufficient to prove that the laws you have delivered about your great discovery, take place as well in the right ascensions as in the declinations of the fixed stars. I have not gone to the utmost precision in the equations, though exact enough for my purpose, none of them being, I believe, one second of time wide of the truth; nor have I allowed for the precession of the equinox. I despair of bringing such observations as these to agree much better, till I, or somebody else, shall be able to discover the true cause of the transitory's deviating from the mark by which it is rectified, in that constant manner which I have observed it to do ever since it was first set up.

[A long account is here omitted of the deviations of Dr. Bevis's instruments from the plane of the meridian. It appears that they constantly moved in one direction during the day, and back again during the night. He gives no estimate of the quantities of this error which varied at different times according as the sun shone out more or less brightly.]

I took the sixteen stars with which you observed the comet as often as I could, till they were lost in the day-light; but it fell out unluckily that I got the altitude of but two or three of the biggest before Mr. Sisson took away the quadrant to put a new limb to it. By the transits of one or two of those I observed, it seems as if they were not the same with those in your list; yet I could find none nearer, with your declinations, in which particular I was forced to be guided wholly by the graduated semicircle of the transitory, which cannot give altitudes much nearer than 10′. If there be any thing else you would be willing to have observed, you'll do me a particular pleasure in letting me know it. I should esteem it a great favour, if at your leisure you'll be so good to transcribe for me your observations for the parallax of Mars.

I am, dear sir,

your affectionate humble servant,

J. BEVIS.

When you favour me with an answer, be pleased to direct it to be left for me at Mr. Heath's, mathematical instrument maker, in the Strand.

[To the Rev. Mr. Professor Bradley.]

DEAR SIR, London, May 8, 1739.

OUR friends Mr. Jones, Mr. Machin, and Mr. Graham, having seen those observations I sent you of the pole star, are of opinion that it would not be improper to send them to Paris, to Mr. Folkes, who is now there, and frequently visits the academists; and this the rather, because it seems the French are now upon observations of the like nature, for executing which, 'tis certain they have no fit instruments. However, in this I would be wholly guided by your advice, which I ask as of a friend; besides your leave for taking such a step, which I am satisfied I ought not to do without it. There is a gentleman of Mr. Folkes's acquaintance who sets out for Paris in a few days, on which account your speedy answer would greatly oblige,

Sir, your very obedient servant,

J. BEVIS.

If you'll subjoin a word or two of what will be best to say, in a concise manner, about the small disagreement of some of the observations, in case you think it needful, I shall acknowledge it as a further favour.

The above named gentlemen send you their compliments; and Mr. Machin desires, that in case Mr. Salvador, the gentleman he wrote to you about, should chance to come to Oxford, you'd be so good as to recommend him to Dr. Shaw, in order to his being shewn the university.

I believe I erroneously transcribed the transit of the pole star Oct. 27th; it ought to be 44′ 46″.

[To Dr. Bevis.]

DEAR SIR,

I AM much obliged to you for the favour of your observations of the stars with which I compared the late comet, as also of the transits of the circumpolar stars; which latter, I suppose, will be thought sufficient to prove that the same cause takes place in producing an aberration in right asc. as I had before found in declination; since the time when the maxima happen, as well as their quantities, corresponds as nearly to my numbers as can be expected in observations of the stars made with so small an instrument, especially since you find that the direction of your axis is itself liable to some little alterations.

I confess that at present I apprehend the cause of that apparent change to arise from something within, rather than without your observatory, because since I received your first letter, I have diligently attended to what has happened with regard to an axis which I have here, that is fixed in a very firm manner between the stone jams of a north window, and have not yet been able to discover any such regular change in the object as you have found at Newington. My mark is not indeed so far from my axis as yours, but a friend of mine here has one with which he can command, I believe, objects at very different distances, so that by comparing both, I hope we may be able to determine whether this change arises from any such cause as you suggest, or from the unfixedness of what supports the instruments.

I make no doubt but a considerable part of the difference that appears upon the comparing of the transits of the pole star on the different nights not far asunder, arises from this change in the direction of your instrument; and yet, if the axis had been invariable, one could scarce have hoped to have made observations of this nature correspond much better than these do; and therefore you need not, I think, scruple to lay them before the astronomers in France, especially if you mention the circumstances with which they were attended, since a change of the direction of your axis of a few seconds only, conspiring with the error that may arise in judging of the exact time when the pole star passes, will occasion a difference as great as any that occurs here.

The observations of the two stars that are 0 32, 2 22 farther from the pole, and which neither of the forementioned causes can be so much affected by, seem to correspond extremely well, and these alone sufficiently evince the certainty, as well as quantity of this aberration in right asc., and make the quantity of it be exactly conformable to my hypothesis. I imagine therefore that it would be very proper to let them know what has been attempted here, and what is the event, that they may, if they are inclined, make the like observations of right asc.; they having before this time, I suppose, examined with the sector (which Mr. Graham made) what are the phenomena with regard to declination.

I have sent you a copy of the observations which I formerly made for discovering the parallax of Mars, and if you take the trouble of computing what it was from them, I shall be glad to know the result; the method I made use of was to take a mean* of several observations made near one another, since it being too tedious a matter to make distinct computations for each, it may be proper first perhaps to compare them together grossly, to see whether any deviate too much from the truth, and reject such as do, before you find a mean by adding several together. I beg you will present my compliments to all my friends you mentioned, &c.

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* This explains the brackets annexed to the observations, p. 353.

By Dr. Bevis, his observations of the transits of the pole star, compared with ϵ Urs. Major, the pole star preceded ϵ at a mean 1′ 30″ in time, or 22′½ asc. r. about Sept. 1738. Hence (supposing his axis exactly adjusted to the meridian) the pole star ought to precede ϵ Urs. M. in December 1739, 1′ 38″, or 24′ 40″ in right asc.* According to his observations about December 1738, the mean precedence of γ Cassiop. before ϵ Urs. Maj. was 1′ 15″. Hence in December 1739, it would be 1′ 15″½ or 1′ 16″ in time.

*By Mr. Flamsteed, diff. in time is 2′ 24″ = 36′ 0″ asc. rect.

In Connoissance de Temps......... 3′ 5″ = 46′ 14″ asc.

These stars come upon the meridian rather too soon after one another to be nicely observed, and therefore it may be proper to make choice of others, as α and β and δ Cassiop. and α, β, γ, δ, ζ, and η Urs. Maj.

N. B. The stars that pass the meridian about six of the clock will be most convenient for this purpose, and at that time the difference of their passage will give their true difference of right asc.; they being then unaffected by the aberration from light.

[To the Earl of Macclesfield.]

My Lord,

WHEN I got up yesterday morning about five, I had a fair prospect of succeeding in the observations which I intended to make; but about six, when the principal stars which I wanted to take were passing the meridian, some clouds came and prevented my observing according to my wish. However, the few that I had the fortune to catch, are sufficient to satisfy me that the mark which I made last on the garden wall, as well as the stake in the field, are extremely near the meridian, I think they can scarcely err more than 5″ in azimuth; and 'twill be needless to attempt a more exact rectification, till the clock is fixed near the instrument; and then I am induced to think, from the experience I have already had, that the true meridian may be found within 2″ or 3″ in azimuth.

I had a pretty quick passage from Shirburn to Oxford, and having just before I set out compared my watch with your lordship's clock, I imagined that a good opportunity offered of settling the difference of our meridians.

By the observation which I made October 23, with the transit instrument at Shirburn, I collect that the sun was on the true meridian 11h. 23′ 0″$\frac{1}{2}$ by your lordship's clock; and supposing it to lose about 12″$\frac{1}{2}$ per diem, it would be on the meridian yesterday (or Oct. 25) 11h. 22′ 38″.

I found by my instrument at the Museum that the sun passed the meridian here at 12h. 1′ 17″$\frac{1}{2}$ by my watch, which I reckoned to be then 37′ 31″$\frac{1}{2}$ faster than your lordship's clock. Hence the difference of meridians comes out 1′ 8″. But if the time of the sun's transit was actually observed by your lordship yesterday, and appears to be different from what I supposed above, (viz. 11h. 22′ 38″,) your lordship, by allowing for that, will find the difference of our meridians more exactly perhaps than by any other kind of observations; for my watch has lately gone so extremely regular, that I think it may be depended upon even while I was riding, and I compared it with your lordship's clock just before I set out, and with my own immediately after my arrival here, and have made the necessary allowance.

Since I left your lordship, I have been considering different methods for moving the lamp, in order to illuminate the cross wires; and the most convenient I can at present think of, is to have a pretty large circular piece of board fixed either to the face of the eastern pier, or to a plank over the top of it, in such a manner that the centre of its circumference may coincide with the axis of the cylinders on which the instrument hangs. The middle part of which board must be cut away, so that the brass work may not hinder it from coming close to the face of the pier, and that the level may be hung on. Then another circular board, whose inner diameter fits exactly the outer one of the former, may turn upon it, moved by an handle of such length as to be conveniently reached by the observer; opposite to which handle must be fixed (to the same moveable board) the arm for carrying the lamp, which should be hung pendulous at about the same distance from the axis' centre as the oval piece of ivory. And it may not be amiss, if this method is made use of, to shorten the arm which now carries the ivory; for by bringing that nearer to the object-glass, a smaller lamp may be sufficient to illuminate. I suppose it will be necessary to add some weight to the handle, that it may in all circumstances counterbalance the lamp, and the arm that carries it. I have cut out two pieces of card in the form I apprehend the boards should be of, but their true dimensions cannot be settled without being upon the spot.

If your lordship will be pleased to send me by Sunday's post the eyeglass which you formerly had of me, and the exact dimension of the inner and outer diameter of the tube of the transit instrument, together with the distance of the cross wires from the end of the tube, or (which will be near enough) the distance of the square key-hole from the end, I will make an apparatus which may serve for the present to use that eyeglass, by which your lordship will be better enabled to judge what focus the eyglass should be of. If the weather permit I will wait upon your lordship some morning with it, when I can likewise more fully explain my design for illuminating.

I am, with the greatest respect, my lord,

Your lordship's most obliged and most obedient servant,

J. B.

⁂ This was probably written in 1739, that being the only year from 1735 to 1742 inclusive, on which Bradley has left us any notice of his having observed the sun at Oxford on the 25th of October.

[To the Rev. Mr. Professor Bradley.]

SIR,

I RECEIVED the favour of yours, and did imagine the term at Oxford to be too busy a time for you to find leisure to come here, till Mr. Jones informed me that I might expect you here at the time I mentioned in my letter to you. However, it is a pleasure to me to know, that the time which will be most agreeable to me, will be so to you. When you examined the quadrant, I remember you found an error of 30″, which I think was to be added to the zenith distance; but as I am not sure of it, I should be glad you would let me know whether I am in the right or not, in case you can recollect it, at this distance of time. If you have made any computations of the latitude from the last observations which you made here, I should be obliged to you if you would favour me with them.

I am your faithful humble servant,

MACCLESFIELD.

If you cannot recollect how the error of the instrument was to be rectified, I believe I can, with the help of some instruction from you, observe it again.

[To the Earl of Macclesfield.]

HAVING just now had the honour of your lordship's letter, I looked into the papers containing the observations made at Shirburn, and found that the error of the instrument was judged to be 30″, to be added to the observed zenith distance; and your latitude by the observations of the sun comes out 51° 40′, but by the stars 51° 39′$\frac{1}{2}$ or 39′. If your lordship be inclined to take the trouble of examining into the error of the instrument again, it may be done as readily in the manner I tried, as any other; viz. by placing two marks at a distance, directly over and as far asunder from each other as the difference between the height of the centre of the object-glass of the telescope (that is fixed on the quadrant) when it is in the erect and inverted positions, which difference of heights may easily be found by trial; then looking at the uppermost object when the quadrant is erect, observe its apparent distance from the zenith. Afterwards taking off the pin on which the wire hangs, as also the piece of brass into which it is screwed, you may invert the quadrant; and then bringing the lower object to appear on the horizontal thread of the telescope, hang up the plummet from the limb of the quadrant, so that it may just beat over the middle of the hole into which the small end of the pin before entered; then if there be no error to be allowed, the thread will in this situation be just as far from the beginning of the divisions or 90° as when the quadrant was erect, only that it must now fall on the contrary side to what it did before; and if it be nearer or farther from the beginning of the divisions, the middle point between the two will be that which is truly perpendicular to the line of collimation; whence you will readily judge if there be any error, of what kind it is.

[To the Rev. Dr. Bradley.]

DEAR SIR, Oxon. Dec. 15, 1742.

I HEARD with great pleasure of your safe arrival at Greenwich; we were in great concern for you, as you had such dismal weather for your journey. I hope the present clear air will make you some amends for the long succession of clouds that preceded it, though I suppose it would have been more acceptable two or three days before the solstice. Mr. Hunt tells me, my lord lay two nights last week at the observatory, and I suppose had an observation of the sun on the solstitial day. I have sent on the other side what observations I made of the first satellite of Jupiter last spring; they are but few, because I was so long in London; I have also set down opposite to them, Whiston and Weaver's calculations. I have many services to you from your friends here; in particular from our next door neighbours. Mrs. Bliss joins with me in proper respects to yourself and Miss Pound. I hope to see you at Greenwich soon after New Year's day, and am in the mean while

Your most affectionate brother,

N. BLISS.

P. S. I beg my humble service to all my friends, in particular to the president and Mr. Jones.

	By Observation at Oxford, 5′ west of Greenwich.		Whiston's Calculation.	Weaver's Calculation.
	Mean Time.	App. Time.		
	H. ′ ″	H. ′ ″	H. ′ ″	H. ′ ″
1742, Jan. 12. Immers. of Jupiter's 1st sat. ...	9 46 16	9 33 40	9 40 31	9 39 40
21. Emers. of Jupiter's 1st sat.	8 29 18	8 15 3	8 18 36	8 17 2
March 15. Emers. of Jupiter's 1st sat. nearly It was not seen one minute before, then a thin cloud till the time set down, when it was seen to be emerged.	10 48 30	10 42 46	10 47 28	10 46 46
April 23. Emers. of Jupiter's 1st sat.	9 18 44	9 22 16	9 29 44	9 29 0

[To the Rev. Dr. Bradley.]

Dear Sir, Shirburn, Saturday Evening, Dec. 24, 1743.

THE evening of yesterday being very thick, and unfavourable for observations, I did not go to the observatory. But Phelps perceiving some little breaks in the clouds, though generally attended with a haziness, attempted to observe the transits of some stars; and taking notice of one that was near passing, and appeared to be about the third magnitude, though none of that size was set down in the Catalogue, he directed the glass to that parallel in order to take its transit, when to his great surprise he found it to be bushy, and immediately suspected it to be a comet. He observed its passage, which was at 0h. 22′ 7″ by Graham's clock, and the clock was found, by a subsequent observation of Rigel and Sirius, to be about 1′ 5″ too slow for true sidereal time; so that the right ascension of this star or comet was 0h. 23′ 12″ in time: and the index of the transit instrument gave its polar distance 68° 20′. He then applied the two foot finder to it, which discovered a stream of light issuing from it, just in the direction the tail of a comet in that place would be in. Upon this he gave me notice of it, and by the best remarks I could make of it, I judged it to be really a comet. The weather then began to thicken so much that I gave up all hopes of making any observations of it last night; but late in the evening I found it was grown clear, and I applied to it my 14 foot glass with the micrometer, in order to compare it with ζ Andromedæ, which was the only considerable star within any tolerable reach of it; and the light of the moon prevented my seeing any star (with the naked eye) of a less size than that, though at the same time I feared, what I found afterwards to be true, that the field of that glass was not large enough to admit both those objects. I then proposed to prepare the quadrant and transit instrument, in order to take its right ascension and declination as exactly as I could by them this night, and to be satisfied thereby whether it was a comet or not. But it has now passed without the weather's having afforded me the least glimpse of it for the whole evening; but if it should clear up, (of which I have little hopes,) I will endeavour to observe it with the micrometer, and compare its place with some star, and will add the result at the end of this letter, which to-morrow being Christmas-day, I choose to write to night. Last night, δ Andromedæ passed at 0h. 24′ 36″ per clock, being 2′ 29″ after this phenomenon. As to the place of it to the naked eye, the supposed comet formed a triangle with α Andromedæ, and γ Pegasi, which was rather more than right angled (or somewhat obtuse) at the angle where the comet appeared, and the distance betwixt that and γ Pegasi was much greater than between the comet and α Andromedæ. If this should be a comet, may it not be the same that was observed in the autumn or end of the summer, in its way to the sun, from which it may now be returning. I am sorry I was disappointed of the pleasure of seeing you in your way to London, and more so for the occasion of it, though I hope you received no inconvenience from having been so wet in your journey from Gloucestershire. I am, with great truth,

Your very affectionate friend,

and faithful servant,

MACCLESFIELD.

P. S. I had a view last night, just for a moment, of the supposed comet, through the two foot finder, and as far as I could trust my memory, with regard to the situation of two small stars that appeared in the glass with it, I judged it to have moved from the place where it appeared the preceding night, and to have increased its declination, and lessened its right ascension. The wishes of the season from all here wait upon you.

Sunday morning.

[To the Rev. Dr. Bradley.]

Dear Sir, Saturday, Dec. 24, 1743.

Last night, about half hour after seven, I thought I saw a comet, and afterwards found it to be one; the nucleus in the telescope seemed considerably bigger than Jupiter, with a large capillitium about it, though little of a tail; 'twas as easily seen as a star of the second magnitude. I went as fast as I could to Mr. Sisson's, but his son was out, and had locked up the sector. But with Mr. Graham's sector I observed as follows:

Clock. H.	′	″		Divis. on the Sector's Limb. °	′
10	45	17	Comet	2	50
	50	40	: : preced. η Androm. ...	5	9
11	0	49	Comet	4	51
	6	12	preced. η Androm.		
	11	7	Comet	2	57
	16	33	preced. η Androm.	4	29
	22	2	Comet	2	59
	27	32	preced. η Androm.	4	34
	33	13	η Androm..................	4	12
	37	30	Comet	3	2
	42	58	preced. η.	4	36

Clock. H.	′	″		Divis. Limb. °	′
11	45	53	Sirius passed merid.		
	48	39	η Androm..................	4	15
	52	33	....	3	5
	58	2	....	4	39
12	3	44	....	4	18
	7	17	....	3	8
	12	52	....	4	42
	18	32	....	4	21

N. B. Comet south of both the stars.

The comet growing low and sunk in vapour, I could observe no more. I can't well depend on the observations of the preceding star, the moon shone so bright; but those with η are, I believe, pretty well, considering the size of the instrument.

I wish you a merry Christmas, and fair sky. I shall be glad to know by a line, Monday, to Mr. Sisson's or Mr. Heath's, what you have seen.

I am, sir, your very humble and affectionate servant,

J. BEVIS.

Doubtless you may take the comet on the meridian soon in the evening.

Oxon. Jan. 12, 1743-44.

DEAR SIR,

I SHOULD sooner have given you the trouble of a letter, but was unwilling to write till I could give you a better account of the state of my family. We have all had slight colds, but my wife has been most violently seized; she has been confined above five weeks, and has undergone as much pain in her head and face as can possibly be conceived, and as much medicinal discipline under Dr. Frewin's directions to get rid of it; which I thank God is now happily effected, and she went abroad for the first time on Wednesday last.

I have enclosed the right asc., decl., and long. and lat. of the comet, as observed at Shirburn, which I have computed; the micrometer observations, my lord lays no stress on, as his eyeglass was not well fixed, and himself not used to make them; they were taken from η Andromedæ. The 27th of December I broke the thread of my micrometer. The 28th, I took its distance from some stars with Hadley's quadrant, but I find by the Shirburn observation that night, that I either mistook the stars, or the divisions of the quadrant, because we differ near two degrees; I took its place that night and the 31st from some telescopular stars by the micrometer; and also its place by the quadrant, which agrees within half a minute with the meridional observation made the same night at Shirburn. I have not had one opportunity here, nor at Shirburn, to observe its place since. As we have had so much leisure from observing, Mr. Betts has been trying to compute the orbits of comets, and has taken that in 1742 for an example. One day he is in raptures, and is full of his praises of yourself and Monnier. Another day's trial sinks him again, and he is sure Monnier is wrong in his elements, because they won't agree with his, which he shall believe true till he sees they disagree with yours, which he begs you would send when you write to me, to save him from distraction. This day he is in raptures, having found out something that he has been long trying after in vain, I wish it may hold till to-morrow. I should be glad to know if you have made any computations which way the present comet is going, whether direct or retrograde; and if you know enough of its orbit, as to enable us to find him when he becomes invisible to the naked eye, and whether it be the same you saw going down to the sun in September last, &c. Messrs. Betts and Camplin desire to join with me and my family in humble service to yourself, Mr. Bradley, and Miss

Pound, and in all the compliments of the season, but no one more sincerely than

Your most affectionate brother, and humble servant,

N. BLISS.

1743. At Shirburn.

			Mean Time.		Comet's r. a.			Decl. north.			Long.			Latit. north.		
			H.	′	°	′	″	°	′	″	°	′	″	°	′	″
By the Transit only.	Dec. 23.	at	5	22½	5	48	1	21	39	30	♈ 14	9	12	17	31	26
Micr.	26.	at	11	8	4	4	2	21	12	27	12	25	2	17	47	10
By T. & Q.	27.	at	4	54	3	41	7	21	7	23	12	2	25	17	51	29
Micr.		at	7	2	3	39	24	21	7	25	12	0	54	17	52	11
T. & Q.	28.	at	4	48	3	11	8	20	59	22	11	32	11	17	55	54
Micr. dub.		at	6	54	3	4	24	20	59	13	11	26	7	17	58	23
T. & Q.	31.	at	4	28	1	44	40	20	36	37	10	4	57	18	9	3

P. S. I have computed the solstice in June last from my lord's observations, and also in December.

The former on the 10th day, 11h. 28′¼ Shirburn apparent time.

The latter on the 10th day, 16h. 3′½ Shirburn ap. t.

The zenith distance in the summer by your Table of refractions, which my lord has given me at a medium of four days' observations, allowing for the alteration of the sun's decl. = 28° 10′ 57″; in the winter 75° 7′ 53″; diff. = 46° 56′ 56″; obliquity = 23° 28′ 28″.

[To the Rev. Dr. Bradley.]

à Paris le 21 Janvier 1744.

JE n'ai reçu, Monsieur, qu'aujourd'hui à midi la lettre obligeante que vous m'aves fait l'honneur de m'écrire, et je vous repond sur le champ, quoiqu'un peu à la haste: Une autre fois j'aurai plus de loisir; Et ne me reproches pas d'avoir été si long temps dans l'inaction. Au sujet de la comete, j'en ai écrit à Mr. Folkes, mais assé rapidement, sans lui envoïer d'observations quoique ce fût le 4 Janvier. J'ignorois aussi qu'elle parût lorsque j'ai écrit à Mr. Sisson. Cependant M. Cassini, à qui M. de Reaumur avoit communiqué

une lettre venant de Suisse, l'avoit observée le 30 Decembre sans m'en avertir. Au reste on l'a observée le 13 Decembre n. st. à Lauzane, et son mouvement étoit assé rapide, car il étoit de 40′ or 45′ par jour, du moins à ce que M. Cassini a dit de vive voix à l'académie.

Quoiqu'il en soit, mes observations ne commencent qu'au 3 Janv. n. st. de même que les vôtres.

A 5h. 31′ 25″$\frac{3}{4}$ de la pendule (dont 23h. 56′ 8″$\frac{1}{2}$ = 360°) la comete a passé au méridien et la luisante du Belier à 7h. 0′ 44″$\frac{1}{2}$, ce qui donne pour différence en asc. dr. apparente 22° 13′ 17″ le soleil avoit passé 0h. 3′ 38″$\frac{1}{2}$ de la pendule.

Dist. au zenith observées
° ′ ″
la comete......27 10 40
ζ Androm. ...25 59 50
α du Belier ...26 37 45

Le 4 Janv.
La Comete 5 25 23
α du Belier 6 56 52$\frac{3}{4}$
donc 22° 55′ 57″. En decl.

Au Méridien.
Petite * 27 5 45
LaCom. 27 19 15
ζ Andr. 25 59 50
α ♈......26 37 40

La petite étoile précédoit la comete de 1′ 49″$\frac{3}{4}$ et ζ Androm. suivoit la comete de 12′ 43″$\frac{1}{2}$.

Le 5 Janv.
5 19 24 Comete pass. au merid.
5 19 42$\frac{1}{2}$: : la petite étoile qui précédoit hier.
5 34 14$\frac{1}{2}$ bon passage de ζ d'Andromede.

On aura donc depuis la luisante du Belier 23° 18′ 16″$\frac{1}{4}$ ou 23° 27′ 59″$\frac{1}{2}$ bon.

Le 6 Janv.
5 13 26 la Comete passe......27 36 22$\frac{1}{2}$: : rectius 27° 36′ 0″
5 30 23$\frac{1}{2}$ pass. de ζ d'And. ...25 59 47$\frac{1}{2}$

Ainsi la dist. en asc. droite, à compter depuis α du Belier, seroit de 23° 59′ 50″.

Le 7 Janv.
5 7 33 la comete passe au fil du Q. de C. mural à dist. de zénith observée de 27 44 30.
5 11 59$\frac{3}{4}$ la petite étoile qui précédoit le 4 au soir 27° 5′ 40″.

J'ai rapporté toutes ces observations à la luisante du Belier, parceque j'en ai estimé l'ascension droite, il y a quelques années, par des hauteurs correspondantes de cette étoile et de Procyon.

Le 10 Janvier le centre du soleil a passé au méridien à 0h. 7′ 15″$\frac{1}{2}$ de la pendule, et avec mon micrometre j'ai observé, à 6h. 0′ 12″ de la pendule, la comete au fil horaire, et à 6h. 6′ 5″ pass. d'une étoile qui étoit plus septentrionale de 14′ 42″$\frac{1}{2}$ on 40″.

Aiant eu occasion d'écrire à Mr. Folkes au mois de Nov^{bre} je lui envoiai la plus grande partie de mes observations sur le passage de Mercure, que j'espere qu'il voudra bien communiquer à la S. R^{e}. Cependant comme l'observation faite au méridien ne s'accordoit pas par mes deux Quarts de Cercles, quant à la différence en déclinaison, je ne l'ai pas encore communiquée. On n'a pas assé de temps pour estimer, sur le Quart de Cercle mural, les hauteurs quand deux astres passent à la fois, aussi je fais plus de cas de mes observations faites au Q. de C. de M. de Louville, où j'ai un bon micrometre.

11h. 51′ 11″ pass. du premier bord du soleil au méridien.
55′ 38″ pass. de Mercure.
57′ 25″¼ pass. du second bord du soleil.

Dist. au zénith.	Mon Q. de C. mobile fixé.
° ′ ″	° ′ ° ′ ″
64 35 45 : : ☿ cent.	☿ 25 20 + 166½ P. = 25 24 25
64 46 22½ ⊙ bord inf.	⊙ 25 20 − 246 P. = 25 13 32½
64 46 27½	Differ. en déclinaison 10 52½
	on plus exactement 10 53

⁂ This letter has no signature; but there can be very little doubt of its being from Le Monnier, since a letter of his to Foulkes, dated 15 Nov. 1743, and containing his observations on the transit of Mercury, was communicated to the Royal Society at their meeting on the 8th of the following December.

[To the Earl of Macclesfield.]

SINCE my return to Greenwich, the weather has proved so unfavourable for astronomical observations, that I have made none worth communicating to your lordship; but the new year approaching, for which my nephew has computed the sun's and moon's transits, &c., I take the liberty of sending them, with my hearty wishes (in which my wife likewise joins) of the compliments of the season to your lordship, lady Macclesfield, and Mr. Parker.

I am, my lord, with the greatest respect,
Your lordship's most obliged humble servant,

à Paris, ce 28 Juillet, 1744.

Monsieur,

J'AI pris la liberté de faire remettre pour vous dans un envoi que le S. Guerin libraire fait actuellement à Londres au S. Vaillant, un livre d'Ephémérides que j'ai calculées l'année derniere. Je souhaite de tout mon cœur qu'il vous fasse plaisir.. J'y ai eu principalement en vuë la commodité des astronomes, et tout ce qui est nécessaire pour préparer les observations. Et quoique je n'aye pas l'honneur d'être connu de vous, j'ay cru que vous ne refuseries pas ce petit présent. C'a été pour moi une occasion de vous faire connoître l'estime singuliere que j'ai de vos talens, et combien j'applaudis à la réputation que vous vous étes acquise parmi les astronomes, qui ne font aucune difficulté de vous reconnoître pour le premier.

Si j'eusse eu en mains les tables auxquelles votre illustre prédécesseur M. Halley a travaillé, peut-être eussé-je fait des calcules plus conformes aux vrays mouvemens célestes. Je vous serois infiniment obligé si vous voulies bien nous faire sçavoir ce que nous pouvons espérer dans la suite, et si les observations et les tables qu'il a faites et même imprimées paroitront bientôt. Nous les attendons avec impatience.

M. Bouguer un des astronomes François envoyés au Perou est icy de retour depuis peu, il n'a pas encore rendu compte de ses opérations; mais on sçait qu'ils ont trouvé la terre applatie, et le degré sous l'équateur de 56550 toises de Paris. J'ai fait imprimer aussi le détail de toutes les opérations géométriques et astronomiques que j'ai faites avec M. Cassini le fils, pour réformer l'ouvrage de son pere sur la figure de la terre. Je vous l'aurois envoyé en même tems, si je n'eusse été choqué du procédé de M. Cassini, qui s'est entierement emparé de ce livre, auquel il n'a presque mis que son nom, et dont à la vérité il a payé les frais de l'impression: s'il eut été en vente j'en aurois achepté un exemplaire pour vous, et un pour moi; mais ce livre n'est pas encore publié que je sçache. Ce que je vous en dis icy n'est pas pour me plaindre de M. Cassini, mais seulement pour vous prévenir sur l'auteur et sur l'ouvrage. Lorsque vous l'aures vu, vous pourres vous assurer de ce que j'ai l'honneur de vous dire, en vous en informant à ceux de notre académie que vous connoisses. Cependant vous me feres plaisir de ne témoigner à personne que je vous en ai parlé moimême. Je vous demande mille excuses de la liberté que j'ai osé prendre avec vous. Mais aussi je vous prie de croire qu'il n'y a personne qui soit plus porté que moi à vous rendre tous les services en ce pays-ci dont vous me jugeres capable. J'ay l'honneur d'être très-parfaitement,

Monsieur, Votre très-humble et très-obéissant serviteur,

LACAILLE.

De l'Ac. Roy. des Sciences, Prof. de Mathem. au coll. Mazarin.

Si vous me faites l'honneur de m'écrire, vous pouves le faire en Anglois. Je l'entens assés bien.

[To the Rev. Dr. Bradley.]

April 24, 1745.

DEAR SIR,

IN the course of printing the Præcepta Calculi, which I have drawn up for Dr. Halley's Tables, I contrived to comprise every thing relating to the moon in a certain number of sheets, which that I might the better consider, digest, and alter from time to time, I have reserved to be wrought off last at the press. There are some things still in which I am a little embarrassed; upon which I should think myself under great obligations for your advice and directions. And first I cannot think, or be informed, of what end the Dr. had in the form of lunar epochs and motions which he has made to anomalistic years, and uses with the sun's mean anomaly, besides that of getting the moon's place without regard to any particular style or other form of year, as Julian, Gregorian, &c.; and at the same time involving the first or annual equations of the ☽ apog. and ☊ in the epochs themselves. I never could hear the Dr. explain himself upon this matter, though I have hinted it to him; nor do Mr. Machin, Mr. Morris, or myself remember that he ever used them in his own computations. Mr. Machin fancies he might have some design in it, which he afterwards dropped, and would have no notice taken of them in the precepts: yet, I think, something at least should be said about them, unless it be judged better to castrate the five leaves which contain them. Pray, sir, be so good to let me have your thoughts about this.

The next thing is, that the Dr. having given no radixes or other numbers of the ☽'s motion from the ☉, except only in centuries of Julian years, I want to know how he intended the mean conjunctions and oppositions of the luminaries should be found out; whether by a Table of epacts (of which he has none neither) or by some arithmetical operation, or what other way?

About a year ago you were so kind to help me out of a difficulty I was in about applying that equation of the interval in the Saros, for future lunar eclipses, which is taken out with the argumentum annuum; by informing me, that in such eclipses the argument. ann. was to be altered six signs, because the Tables were made to the sun. This, I find, brings out the mean time of the middle of the future eclipse pretty near the truth. For example, in the first period of the present century, the mean time of the middle of the ☽'s eclipse, which happened in Nov. 1714, was observed:

		H.	′	″
	1714, Nov. 10.	1	4	0
Add the equat. on the sun's mean anom.			39	21
and the equat. on the arg. ann. +6 S.			16	0
and the period besides	18 years. 10.	7	43	24
Gives the middle of the next corresponding eclipse	1732, Nov. 20.	9	42	45
Which I observed in Buckingham-street	1732, Nov. 20.	9	39	15
Error of the computation			3	30

I suppose that in the Tab. of the moon's hor. mot. diam. and parallax in eclipses, the arg. ann. is also to be altered in like manner, when the eclipse is lunar.

However, I am still at a loss as to the use of the numbers in the last column but one of the Saros: for indeed I am not sure of the right meaning of its title, Lat. Lunæ à Sole. I guess these numbers, together with those in the third column of the Table of the equations, entitled Diff. Lat., are for determining the moon's absolute latitude in the next corresponding eclipse; but in what manner I know not. In the eclipse of 1714, above mentioned, the Lat. Lunæ à Sole is marked 2′ 42″ Bor. Asc.*; and the Diff. Lat. answering to ⊙ mean anom. in the Table of equations, 0′ 42″.†

I flatter myself, sir, you will not only forgive the troublesome request I make you, but favour me with your answer as your leisure suits, which you may please to order to be left at Mr. Graham's for,

Sir, your affectionate humble servant,

J. BEVIS.

P. S. My friend Dr. Stephens, one of the late duchess of Marlborough's executors, formerly of Corpus Christi coll., and, I think, of your acquaintance, desires me to ask the favour that you would permit him to pay you a visit; he having thoughts of erecting an observatory at Marlborough-house.

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* Desc. in MS.

† 47″ in MS.

[To the Rev. Dr. Bradley.]

SIR, May 11, 1746.

I HAVE been here twice to wait upon you without having the satisfaction of finding you. It was to make you the compliments of Mr. Euler, pro-

fessor in mathematics at Berlin, and to communicate to you the lunar Tables, (which I leave here,) and upon which he very much desires to have your opinion, being thoroughly acquainted with the value of it. I'll do myself the honour to wait upon you again before long, not only to receive an answer as to these Tables, but to have an opportunity to be more acquainted with a man of your merit, and to assure you of the sincere respect with which

I am, sir, your most obedient humble servant,

C. WETSTEIN.

I'll only add this little extract of professor Euler's letter relating to these Tables.

Les Tables lunaires que je vous envoye, je les ay calculées de la théorie de feu monsieur le chevalier Newton; je ne les donne pas encore pour parfaites, mais la correction doit être tirée des observations——quoyque le titre, qui a été mis à mon insçu, porte, *Tabulæ correctæ*, je ne les ai données que comme un Essay, où l'on ne doit regarder que la forme et les titres des Tables. Elles sont imprimées comme la théorie, jointe aux autres Tables, me les a fournies. J'ay trouvé moy-même à y faire diverses corrections depuis par les observations.

[à M. le Docteur Bradley.]

MONSIEUR, à Paris le 27 Nov. 1747.

LES occupations que j'ay eues pour mon retour de Russie en France ne m'ont pas permis de répondre plutôt à votre derniere lettre de Greenwich le 1-12 May 1746. Je vous suis bien obligé du détail curieux que vous avez bien voulu me communiquer de votre observation du dernier passage de Mercure sur le soleil. Quoyque vous aiez été fort incommodé par le mauvais tems, vous en avez cependant pu faire des observations fort exactes, et qui peuvent être bien plus utiles pour la théorie de cette planete que bien d'autres observations du mesme passage faites par d'autres astronomes qui avoient le tems plus favorable. J'ay surtout été ravi que vous aiez observé le diamètre apparent du soleil le jour mesme de ce passage, et cela avec la mesme lunette de quinze pieds avec laquelle vous avez observé Mercure. Cette longueur de lunette est plus grande que l'on n'en emploie ordinairement dans l'observation des diamètres apparens du soleil et de la lune; aussi cela vous a-t-il procuré de vous assurer d'une diminution dans les diamètres apparens du soleil dont

M. Halley n'a pas eue connoissance. Vous dites que cette diminution que vous avez trouvée par différens moiens est de $\frac{1}{8}$ de minute; il seroit extrêmement curieux et utile pour la perfection de l'astronomie et la précision des observations, que vous voulussiez bien publier tout le détail de vos observations sur cela, avec les différens moiens que vous y avez emploiez. Si vous ne voulez pas les publier vous-mesme bientost, je vous prie de vouloir bien me les communiquer, je tacherai d'en faire le meilleur usage qu'il me sera possible dans un traité que je me propose de publier bientost de tous les passages de Mercure sur le soleil, dans lequel on verra la nécessité qu'il y a de connoître le diamètre apparent du soleil avec toute la précision possible, et tel qu'il doît paroître aux lunettes avec lesquelles on observe les passages de Mercure sur le soleil; car je suis dans la persuasion, comme je l'ai insinué dans les Mém. de l'Acad. année 1743. p. 424. que les diamètres apparens du soleil paroissent de différentes grandeurs selon les différentes longueurs de lunettes que l'on y emploie. J'en ai la preuve par des observations trez exactes des diamètres apparens du soleil que j'ai faites, il y a prez de trente ans, avec des lunettes de treize et vingt pieds, par lesquelles j'ai trouvé constamment les diamètres apparens du soleil de plusieurs secondes plus petits que l'on ne les avoit trouvez jusqu'alors avec des lunettes ordinaires de six, sept ou huit pieds tout au plus.

Je souhaiterois bien que vous eussiez pu trouver le loisir d'examiner et de mettre en état d'être publiées toutes les observations que vous avez faites à Wansted depuis plus de vingt ans sur les variations des étoiles fixes, qui ont à ce que vous me marquez un petit mouvement apparent différent de celui qui provient de la propagation de la lumiere; j'ay appris que M. le Monnier (qui se vante fort ici de la correspondance qu'il a avec vous) a entrepris quelque chose sur cela; mais je n'ay pas encore sçu exactement ce qu'il a fait, ni s'il tient de vous ce qu'il dit avoir découvert.

Je persiste toujours dans le dessein de publier les Tables des satellites de Jupiter dans la forme dans laquelle je les ai mises il y a plus de vingt ans, et je les accompagnerai de leur comparaison avec toutes les observations que j'ay pu recueillir jusqu'ici: ces calculs sont fort avancez depuis long-tems; mais je n'ai pas encore reçu les caisses de mes papiers où ils se trouvent.

Depuis que l'Académie des Sciences de Paris a proposé pour le prix de l'année prochaine 1748 une théorie de Saturne et de Jupiter, par laquelle on puisse expliquer les inégalitez que ces deux planetes paroissent se causer mutuellement principalement vers le tems de leur conjonctions, j'ay fort travaillé sur ce sujet, non pas dans le dessein de concourir au prix, dont je suis exclus comme membre de l'Académie, mais parceque c'est une des plus curieuses et des plus utiles recherches de l'astronomie. J'y ai emploié principalement les oppositions de Saturne et de Jupiter au soleil que M. Halley s'est donné la peine de calculer et de comparer avec ses Tables depuis l'an 1657 ou 58 jusqu'à l'édition de ses Tables en 1719. Vous me feriez un trez grand plaisir, Monsieur, si vous pouviez m'en donner la suite jusqu'au tems présent, et d'y joindre aussi les oppositions de Mars au soleil pendant le mesme interval de tems : je ne doute pas qu'avec l'aide d'une si grande suite et aussi exactement observée que vous l'avez pu faire aprez M. Halley, on ne puisse découvrir pour le tems présent la loy de la variation de l'aphélie de ces planetes, et du changement de leur excentricité, &c.

Depuis deux ou trois ans on s'est fort appliqué dans l'Académie d'ici à rechercher géométriquement les effets de la pesanteur mutuelle des corps célestes, suivant les loix de Kepler démontrées par M. Newton. M. Clairaut entr'autres a cru trouver que la lune ne suivoit pas exactement ces loix, et que par conséquent M. Newton s'y étoit trompé considérablement, surtout dans le mouvement de l'apogée, qui suivant ces loix devoit être de dix-huit ans, au lieu que M. Newton l'a toujours supposé de neuf ans seulement, comme les observations astronomiques le donnent. Je ne sçay ce que M. Machin en dira, et surtout ce qu'il répondra aux objections que M. Clairaut a publiées contre son système dans les derniers Mémoires de l'Académie pour l'année 1743, qui paroissent depuis peu. Il me semble que c'est à tort que Messrs. nos astronomes et géomètres disent que les Tables dressées d'aprez les principes de M. Newton ne s'accordent pas toujours exactement avec les observations, aiant vu moy-mesme avec quelle précision les tables de M. Halley se sont accordées avec toute la suite des observations que ce grand astronome a faites sur la lune pendant dix années entieres depuis le commencement de 1722 jusqu'au commencement de 1732, et sachant le peu qu'il y a à ajouter à la théorie de M. Newton et aux tables de M. Halley pour les rendre entièrement conformes aux observations. C'est à quoi je m'appliquerai aussi lorsque j'aurai pu obtenir la suite des observations de M. Halley, qui me manquent depuis le commencement de 1732 jusqu'à sa mort, et celles qui ont été faites depuis. C'est pourquoi je vous prie trez-instamment de vouloir bien m'envoier le résultat de celles que vous pouvez avoir recueillies depuis le commencement de 1732 jusqu'à présent. Si vous en avez fait le calcul sur les tables de M. Halley, comme cet astronome avoit accoutumé de le faire aussitôt aprez chaque observation, vous m'obligerez aussi trez sensiblement de vouloir bien m'en communiquer le résultat. J'y joindrai aussi la suite de plus de dix ans d'observations que j'ay faites à Petersbourg, lorsque je les aurai calculées, ce qui produira la plus grande suite que l'on ait encore eue d'observations exactes sur la lune, qui sera la meilleure pierre de touche dont on pourra se servir pour éprouver les hypothèses de nos géomètres.

M. Clairaut nous a annoncé parmi ses nouvelles découvertes une nouvelle théorie des mouvemens de Saturne et de Jupiter, dans laquelle il a déterminé géométriquement toutes les irrégularitez apparentes du mouvement de ces planetes suivant les loix de la pesanteur. Lorsqu'il nous l'aura communiquée, j'en ferai l'essai sur les observations de M. Halley et sur celles que j'attends de vous, et je vous marquerai ce que j'aurai trouvé. Je vous prie de vouloir bien me dire en attendant s'il y a quelqu'un en Angleterre qui travaille à présent de la mesme maniere que l'on fait ici, non seulement à déterminer géométriquement ce qui doit résulter des hypothèses physiques, mais encore à rassembler des observations pour les comparer avec ces hypothèses.

Je souhaiterois sçavoir si M. Machin, à qui je vous prie de faire mes complimens, ne se déterminera pas à publier son traité complet de la pesanteur promis depuis si long-tems? qu'est-ce qui l'oblige d'en différer tant la publication? mandez-moi aussi si l'on va bientost rendre publiques les tables de M. Halley, et si M. Bevis (à qui je vous prie de faire aussi mes complimens) a fait l'introduction qu'il avoit promise à ces tables.

Il y a plusieurs années que j'avois prié M. le chevalier Hans Sloane de me communiquer quelques cartes manuscriptes du Japon et des terres voisines, qu'il a acquises avec les papiers de Kemfer. M. Hans Sloane étoit prêt de le faire alors si j'avois été plus à portée de lui étant en Russie, et s'il n'avoit eu lui-mesme quelqu'envie de publier sur ces cartes un petit atlas Japonois; mais comme je crois qu'il n'y pense plus à présent, je vous prie, en le saluant trez-respectueusement de ma part, de lui demander quel est à présent son dessein sur cela, et s'il voudroit bien m'envoier ici ces cartes que je ferois traduire par des Chinois qui sont à Paris. Je les joindrai ensuite aux mémoires que j'ay apportez de Russie sur ces pais-là, surtout aux nouvelles découvertes faites par les Russes, et je composerois sur le tout de curieuses dissertations, que je soumettrois au jugement et à l'usage que M. le chevalier Hans Sloane en voudroit faire.

Il y a ici un astronome fort laborieux, nommé l'abbé de la Caille, qui a entrepris entr'autres ouvrages de calculer toutes les cometes dont il a pu recueillir les observations, et de déterminer les élémens de leur cours suivant la théorie de M. Newton. Il en a déjà ajouté plusieurs à celles de M. Halley et aux vôtres; lorsque j'aurai pû avoir tout ce qu'il a fait sur cela, je vous en donnerai avis pour exempter M. Betts de faire ce que M. l'abbé de la Caille auroit déjà exécuté. La comete de 1729-30 a attiré l'attention de bien des astronomes à cause de la durée de son cours; vous pouvez voir dans le dernier tome des Mém. de l'acad. de Paris pour l'année 1743, p. 195, les élémens que M. Maraldi y donne du mouvement de cette comete, suivant le résultat des observations de M. Cassini, rapporté dans les Mém. de 1730. Il y a plusieurs années que j'ay aussi recherché les élémens de cette comete sur les mesmes observations de M. Cassini, sans avoir eu connoissance auparavant du travail de M. Maraldi. J'ai communiqué mes élémens de cette comete à plusieurs astronomes, comme à M. le Monnier, qui les a publiés à la fin de ses Institutions Astronomiques, p. 657. Comme ces élémens représentent beaucoup mieux les observations que ceux de M. Maraldi, j'ay crû vous faire plaisir de vous les communiquer avec la différence du calcul à l'observation. Je crois que l'on ne peut pas approcher plus prez des observations telles que M. Cassini les a rapportées; mais il n'en a donné que le résultat, il reste pour une plus exacte recherche de la théorie de cette singuliere comete à avoir tout le détail des observations de M. Cassini, et à le rectifier sur les plus exactes et les plus nouvelles positions des étoiles fixes auxquelles cette comete a été comparée, et c'est ce que M. l'abbé de la Caille a entrepris aprez avoir eu, pour ce dessein, communication du journal et des observations particulières de Messrs. Cassini et Maraldi sur cette comete. Lorsque j'aurai connoissance de ce que M. l'Abbé de la Caille aura achevé sur cela, je vous en ferai part.

Elémens de la comete des années 1729 et 1730 suivant moy.

Distance du Perihélie au soleil 408164 dont la moienne distance de la terre au soleil est 100000.

Long. du Perihélie 22° 27′ 3″♒.

Passage de la comete par le Perihélie tems moien à Paris le 25 Juin 1729 à 9h. 21′.

Lieu du nœud ascendant 10° 32′ 55″♒.

Inclinaison de l'orbite de la comete à l'écliptique 77° 1′ 0″.

Cette comete est directe.

Différence du calcul à l'observation.

	en Long.	Lat.		en Long.	Lat.
	′ ″	′ ″		′ ″	′ ″
1729, Aoust. 31	2 15−	3 19+	1729, Nov. 10	0 5−	0 37−
Sept. 2	0 2+	0 7−	14	0 13−	1 16+
3	0 30+	0 28−	16	0 5−	1 44+
10	0 36+	3 8+	17	0 27+	0 27+
11	0 31+	0 35−	18	0 25−	0 44+
12	0 36+	0 1−	20	0 39+	0 24+
15	1 2+	0 45+	22	0 42+	2 15+
16	0 48+	1 26+	24	0 43−	0 38−
18	1 40+	0 21+	30	0 5+	0 4+
19	0 24+	0 20+	Dec. 2	2 34−	1 19+
21	0 27−	1 4+	3	1 5−	1 19+
23	0 7+	0 47+	9	1 25−	1 20+
26	2 32−	1 35−	14	1 50−	0 15−
Octob. 10	1 22+	0 17−	19	2 8+	1 4+
11	0 57+	0 31+	20	2 46+	0 22−
12	0 29+	0 17−	24	1 42+	5 32+
14	1 31+	0 40+	27	0 11+	0 11+
19	3 14+	0 47−	1730, Janv. 7	2 21−	1 47+
22	2 7−	0 58−	8	2 33−	2 8+
24	0 7+	1 13+	16	1 23−	0 47+
26	1 40+	0 15+	17	0 0	0 6−
27	1 19+	0 22+	18	1 50−	0 29−

Je suis avec une trez parfaite considération,

Monsieur, votre trez humble et trez obéissant serviteur,

DE L'ISLE.

[à M. le Docteur Bradley.]

ce 2 May 1748.

MONSIEUR,

Ayant trouvé une occasion sure de vous faire tenir cette lettre, j'ai pris la liberté de vous écrire, afin de vous prier instamment de me faire la grace de me marquer ce qui s'est passé en Angleterre pour l'avancement de l'astronomie, au cas que vous en ayés le loisir et l'occasion par la suite. Nous sommes icy à Paris sur cet article dans une parfaite ignorance depuis plus de trois ans: nous ne voyons ni livres ni nouvelles littéraires de votre pays, d'où cependant les plus intéressantes ont coutume de venir. Je ne sçais s'il en est de même de vous à notre égard: dans ce cas serois-je assés heureux de vous faire plaisir en vous disant en peu de mots ce qui me revient actuellement à la mémoire sur ce qui s'est passé icy à Paris depuis quelques années? Quoique vous le sçachiés peut-être mieux et de meilleure part, je ne laisserai pas de vous le dire.

Depuis le retour de nos mathématiciens du Perou, on n'a pas encore publié de relation de leurs operations. Il en paroît seulement un abrégé dans les Mémoires de l'Académie pour l'année 1744, qu'on va publier incessamment. J'ai quelque idée de vous avoir envoyé la grandeur du degré qu'ils ont déterminée, mais je soupçonne de ne vous avoir pas donné la vraye, ne la sçachant alors que sur le rapport de M. Cassini, qu'il m'avoit fait par mémoire. Cette vraye mesure du degré de l'équateur réduite au niveau de la mer est de 56746 toises.

Nos géométres ont beaucoup travaillé sur la théorie de la lune. Le résultat de leurs calculs est qu'en résolvant dans la rigueur le problême de trois corps animés de forces centrales en raison inverse du quarré de la distance, et en appliquant la solution à la lune, il n'en résulte qu'environ la moitié du mouvement de l'apogée, de sorte que M. Clairaut prétend qu'il faut apporter quelque modification à cette loi de la raison inverse du quarré de la distance: M. Dalembert et lui ont donné de nouvelles méthodes pour construire des tables de la lune d'une manière bien différente de celle de M. Newton.

M. Clairaut a donné des formules très-simples pour calculer l'aberration de la lumière dans les planetes et les cometes. Celle des planetes se reduit à cette analogie. Comme le mouvement horaire du soleil multiplié par le rayon du grand orbe, est à la distance de la terre à la planete multipliée par 20″, ainsi le mouvement horaire de l'astre vu de la terre est à la quantité dont l'aberration a diminué sa longitude, son ascension droite, sa déclinaison, &c. selon que le mouvement horaire étoit en longitude, en ascension droite, ou en déclinaison, &c.

M. Bouguer vient de nous communiquer une construction de lunette propre à mesurer fort exactement les diamètres du soleil et de la lune, et surtout leurs variations. Ce sont deux objectifs de même foyer, les plus longs sont les meilleurs, pourvu qu'ils puissent être renfermés dans une lunette facile à manier. Ceux dont il s'est servi pour exécuter son idée sont de 18 pieds de foyer. Il les met à côté l'un de l'autre au bout de son tuyau, et leur donne un seul oculaire commun: ces deux objectifs sont placés à-peu-près dans un même plan, mais tellement écartés que les images des deux bords opposés du soleil se touchent à l'endroit de leur foyer, en sorte que lorsque le diamètre du soleil vient à croître, les images de ces bords anticipent l'une sur l'autre d'une quantité facile à mesurer avec un micromètre. Cet instrument est très-propre à faire voir si les diamètres horizontaux et verticaux du soleil sont égaux: car en tournant la lunette, le contact des deux images se fait en différens points de la circonférence du soleil: Par ce moyen M. Bouguer soupçonne, sans l'assurer cependant, que les diamètres du soleil ne sont pas parfaitement égaux, ce qu'on pourra vérifier dans la suite. Je ne sçais si je me suis énoncé clairement, mais pour une personne aussi intelligente que vous, je crois que j'en ai dit assés, ce que vous y suppléerés facilement, aussi bien qu'à ce que je ne vous ai pas dit sur le reste de ses usages.

M. le Monnier nous a donné quelques observations sur l'inégalité de la précession des équinoxes, qu'il trouve conforme à ce qui résulte de la théorie de M. Newton. Mais c'est de vous que nous attendons la vraye détermination de cette inégalité, dont la découverte vous appartient, et je vous avoue que je serois charmé d'avoir là-dessus quelque chose de certain de votre part, étant actuellement occupé à un travail considérable sur les étoiles fixes. Je sens qu'il me sera impossible de faire rien de complet, si je ne suis assuré de la vraye manière d'avoir égard à la précession des équinoxes. Quoique je sois assés bien fourni de bons instrumens, et que j'aye un lieu très-commode pour observer, je n'en ai pas d'assés grands pour des déterminations si délicates, ce qui d'ailleurs demandent un tems considérable et une longue suite d'observations. C'est pourquoi, sans vouloir vous engager à me donner des éclaircissemens particuliers sur cette matière, je vous prie de me faire sçavoir si vous avés publié quelque chose là-dessus, ou si nous avons lieu d'espérer quelque dissertation de votre part sur cette matière.

Le même M. le Monnier prétend avoir découvert que les diamètres du soleil apogée sont sujets à des inégalités périodiques.

Pour moi j'ai beaucoup travaillé sur la théorie des cometes. J'ai complété le catalogue de M. Halley en y ajoutant la théorie de toutes celles dont on a un nombre suffisant d'observations. Parmi celles que j'y ai ajouté, il y en a deux qui vous appartiennent. J'ai calculé toutes les autres: j'ai celles de 1729, 1742, 1744 aussi exactes qu'elles puissent être, ayant vérifié toutes les observations de la premiere, et fait exactement celles des deux autres. Vous trouverés ce catalogue dans le livre cy-joint, que j'ai l'honneur de vous adresser, non comme un présent, car il ne mérite pas ce nom, mais comme une ébauche d'un traité élémentaire d'astronomie, que je compte retoucher, et mettre en état d'être publié. Car quoiqu'il soit imprimé, je ne l'ai distribué qu'à quelques amis, et à mes écoliers à qui je l'ai expliqué. J'en ai tiré peu d'exemplaires, pour n'en donner au public que la seconde édition; si en le parcourant vous y trouviés quelque chose qui méritât la réforme, vous m'obligeriés beaucoup de me l'indiquer. Je ne voudrois pas hazarder de faire imprimer une ésquisse aussi grossière, si je n'étois obligé de mettre entre les mains de mes disciples des matières à expliquer. Vous y trouverés la méthode dont je me suis servi pour calculer l'orbite des cometes; j'ai retouché cette méthode, et l'ai mise dans un meilleur état pour être insérée dans les Mémoires de l'Académie.

On n'a pas publié icy de livres sur l'astronomie depuis plus de quatre ans, si ce n'est une traduction de l'Introduction de M. Keill, dans laquelle on a inséré bien des choses que l'auteur n'auroit peut-être pas avouées. On y a mis les tables de la lune. Si le livre en eut valu la peine, ou si l'on en avoit donné quelque autre digne d'attention, je me serois fait un plaisir de vous le faire remettre par la même occasion.

Nous n'avons encore rien vu icy d'ouvrage posthume de M. Halley, pas même la table imprimée dont vous me fites l'honneur de me parler, sur les différences entre les lieux de la lune observés et calculés. Peut-on sçavoir si l'Angleterre jouit à présent du fruit de ses longs travaux? et si ses tables astronomiques sont enfin imprimées et publiées. Oseroit-on vous demander si vous avés établi la théorie des variations de l'obliquité de l'écliptique que l'on attend de vous, et sans vous obliger à détailler cette théorie, quelle est à-peu-près la loi de ces variations, et quelles en sont les limites, afin de les comparer aux observations que j'ai faites tous les ans avec beaucoup de soins? enfin a-t-on publié quelque découverte nouvelle, ou donné quelque bon livre sur l'astronomie depuis quatre ans?

Je ne sçais, Monsieur, ce que vous penserés de ma hardiesse, je me flatte que vous l'excuserés, en l'attribuant au grand désir que j'ai de voir perfectionner une science qui fait mon unique passion. Je ne vous aurois pas importuné directement, si la guerre ne nous fermoit tous les passages et toute communication avec l'Angleterre, ou si ceux qui peuvent avoir des nouvelles littéraires de ce pays-là, n'en faisoient pas de mystère, pour se faire passer dans l'occasion pour des hommes merveilleux. Nous en avons malheureusement quelques-uns de cette humeur dans notre académie.

Au reste je ne prétens pas vous obliger en rien sur la réponse à cette lettre; si vous en avés le loisir et l'occasion, vous me ferés beaucoup d'honneur; et si vous m'indiqués quelque bon ouvrage nouveau, j'aurai soin de le faire venir, sans vous charger de cette peine.

J'oubliois de vous marquer que j'ai fait une suite aux éphémerides pour dix autres années: le travail n'en a duré que six mois et demi: j'ai gardé la même forme que dans le premier volume, j'y ai seulement mis quelques calculs plus précis : j'en rendrai compte dans la préface. J'y joindrai une dissertation sur l'usage des observations de la lune, et une sur le passage de Venus sur le disque du soleil, lequel doit se faire en 1761. Je vous avouë ingénument que je ne crois pas cette observation susceptible de la précision que M. Halley lui donne, et cela par le peu d'accord que je vois entre nous sur le moment de l'observation de la sortie de Mercure de dessus le disque du soleil. Ainsi je pense qu'il y a beaucoup à rabattre sur ce que M. Halley nous promet pour la parallaxe du soleil. Je serois cependant bien aise que vous m'en disiés un mot : car je m'en rapporterois plus volontiers à vous qu'à tout autre. Le livre ne paroîtra que dans deux ou trois ans.

Je vous demande mille excuses de mon importunité, et du peu d'ordre que j'ai mis dans cette lettre. J'ai eu à peine une heure pour l'écrire. J'ay l'honneur d'être avec l'estime la plus singulière et la venération la plus profonde,

Monsieur,
Votre très-humble et très-obéissant serviteur,
LACAILLE.
Prof. de Mathém. au College Mazarin, à Paris.

[à M. le Docteur Bradley.]

à Paris, le 3 Juin 1748.

MONSIEUR,

DEPUIS votre agréable lettre du 1-12 May 1746, j'ay eu l'honneur de vous écrire par la voie de Hollande le 27 Nov. dernier. J'aurois bien souhaité que cette lettre vous fût parvenue, et que vous m'eussiez fait l'honneur d'y répondre ; mais je n'ay pas encore la certitude que vous l'aiez reçue, aiant été privé de votre réponse jusqu'ici. Sur ce que je vous ai mandé dans ma dernière de la véritable grandeur des diamètres apparens du soleil, j'ajouterai que c'est une matière qui a été fort agitée ici à l'académie depuis le commencement de cette année, et sur laquelle on s'est proposé de faire de nouvelles observations cet été, dont je vous ferai part.

Vous avez pû apprendre par les nouvelles publiques que quoyque l'on ait accordé le prix proposé par l'académie pour l'année 1748, sur l'inégalité des mouvemens de Saturne, l'académie a cependant jugé à propos de proposer encore le mesme sujet pour le prix de l'année 1750. Je vous en envoie l'annonce pour vous engager d'y travailler, étant une matière des plus curieuses et des plus utiles de l'astronomie et de la physique céleste, et que vous êtes en état de traiter mieux que personne.

Je vous envoie aussi un exemplaire d'un avertissement que j'ay fait imprimer à Paris sur l'éclipse annulaire prochaine du soleil. Comme cette éclipse doit paroître telle en Ecosse (ainsi que celle de 1737) je souhaiterois bien que l'on l'y observât exactement, et que vous voulussiez bien en écrire pour cela à ceux que vous y connoissez, afin que l'on y fît toutes les observations dont j'ay parlé dans mon avertissement.

Je vous ai déjà marqué ma demeure à Paris, qui est au college Roial, place de Cambrai : aprèz l'agréable nouvelle des préliminaires signez entre l'Angleterre et la France, nous pouvions nous écrire plus souvent. Je souhaiterois seulement que vous me fournissiez plus d'occasions de satisfaire votre curiosité, et de vous donner des preuves de la singulière estime et du parfait attachement avec lesquels j'ay l'honneur d'être, Monsieur,

Votre trez-humble et trez-obéissant serviteur,

DE L'ISLE.

[à M. le Docteur Bradley.]

à Paris, le 13 Juin 1748.

Monsieur,

QUOYQU'IL n'y ait que dix jours que je vous ai écrit, en vous envoiant par la voie de Hollande un exemplaire rogné de mon avertissement aux astronomes sur l'éclipse prochaine du soleil, que l'on espère devoir être annulaire dans plusieurs endroits de l'Europe, et principalement en Ecosse ; aiant l'occasion de vous en faire tenir un autre exemplaire mieux conditionné, je ne l'ai pas voulu manquer. C'est le zèle de M. le Monnier pour l'astronomie qui l'a déterminé à aller observer cette éclipse à Edimbourg. Il est accompagné dans son voyage par un jeune Allemand Prussien, nommé, Grischow, qui est de la Soc. R. de Prusse, et que j'ay ammené à Paris pour le perfectionner dans l'astronomie : j'ay l'honneur de vous le recommander de la manière la plus forte, vous priant de lui permettre l'accez et l'usage de vos instrumens à Greenwich, et d'assister quelquefois à vos observations : il en aura toute la reconnoissance possible ; et outre cela, comme il est déjà fort avancé dans la connoissance de ce qu'il y a de plus parfait dans les instrumens d'astronomie et de plus fin dans la pratique de cette science, le surcroît de connoissance qu'il y acquérera par vos lumières ne tournera qu'à l'avantage de l'astronomie et à l'honneur de la nation Angloise, lorsque de retour dans son pais il établira un observatoire sur le modèle de tout ce qu'il aura vû de plus parfait.

Si vous ne m'avez pas encore fait l'honneur de répondre à mes dernières

lettres par la poste, vous en pouvez charger Monsieur Grischow, qui ne fera pas un long séjour en Angleterre. Je suis en attendant avec la considération la plus parfaite, Monsieur,

Votre trez-humble et trez-obéissant serviteur,

DE L'ISLE.

Clariss. et Rev^{me} D. D. Jacobe Bradley.

MITTO tibi, reverendissime præses, observationem cometæ, qui hoc anno 1748 sub finem Aprilis nobis apparuit, ad cujus calcem tabellam revolutionum ejus adjeci, ut a te corrigatur, augeatur, et perficiatur; unde mihi, et rei sidereæ cultoribus probe constet, quam verum sit clariss. D. Isaaci Newtoni systema cometicum, meis, et tantorum virorum Regiæ Academiæ vestræ observationibus comprobatum. Spero illam videre amplissima eruditione tua correctam, et auctam, et accuratis observationibus tuis perfecte ornatam. Londinum, et oppidum sanctæ Mariæ majoris, sunt loca inter se valde dissita, ac proinde ex observationibus utrobique institutis hujus cometæ parallaxes indagare licebit. Vellem insuper a te scire, num visus sit alter cometa, qui eundo et redeundo periodum suam breviori tempore peragat, quam iste pyrobolus æthereus a me nuncupatus.

Anno superiori 1747, mense Septembri, misi clariss. domino Matthæo Sarayva, medico Brasiliensi, Reg. Acad. vestræ socio, observationes cometæ a clar. D. Josepho Betts institutas anno 1744, mensibus Jan^{o}. et Febr^{o}. quibus meas adnexui, et manu mea delineavi: nam ab initio Martii ejusdem anni a vespertino ad matutinum idem cometa transiens se nobis videndum præbuit usque ad finem Junii. Sane toto tempore apparitionis ejus incommoda valetudine pressus haud potui illum dietim observare.

Tabulas planetarum, et satellitum Jovis juxta novum systema celeberrimi geometræ, et angelicæ subtilitatis viri Isaaci Newtoni a summo astronomo doctore Halley diu elaboratas, jam pridem videre desidero. Rogo te, sapientissime præses, ut, si fieri potest, illarum participem me facere digneris. Precor Deum Opt. Maximum ut te impleat omni benedictione, prosperitate, felitate, salute, et gaudio. Ex oppido S. Mariæ majoris in missionibus Paraquariæ 16 Julii, ann. 1748.

Sapientiss. et Rev^{me} D. D.

Tuus addictissimus et devotissimus servus,

BONAVENTURA SUAREZ.

Observationes Cometæ habitæ in oppido S. Mariæ majoris missionum Paraquariæ a P. Bonaventura Suarez, soc. Jesu, anno 1748. a 24. Aprilis usque ad 30. ejusdem, horis post med. noctem.

Aprilis manè.			Longitudo.				Latitudo.				Asc. Recta.		Declin.		
D.	H.	m.	S.	G.	′	″	G.	′	″		G.	′	G.	′	
24	4	0	♓	12	53	38	21	31	0	b	336	0	13	10	b
25	4	6	♓	16	30	33	26	6	5		337	15	18	40	
26	4	36	♓	20	19	42	30	37	30	b	338	30	24	10	b
27	5	40	♓	23	47	4	34	56	0		339	16	29	20	
28	4	10	♓	28	0	15	38	32	18	b	340	45	34	6	b
29	5	40	♈	2	13	36	42	10	0		342	0	38	50	
30	6	0	♈	6	34	33	45	42	30	b	343	6	43	30	b

Die 1. Maii Cometa horizonti proximus sub crepusculo matutino ægre conspiciebatur; die vero 2. properans ad Boream sub horizonte latuit.

Loca Fixarum,

juxta observationes habitas 8. Junii, et deinceps, labentis anni 1748. calculo supputata, in quo supponitur maxima eclipticæ obliquitas gr. 23. m. 29. s. 0.

Denominationes Stellarum.		Longitudo.				Latitudo.				Asc. Recta.			Declinat.			Magn.
		S.	G.	′	″	G.	′	″		G.	′	″	G.	′		
γ	Quæ in cubito dextro Aquarii	11	3	10	0	8	18	0	b	332	5	17	2	38	a	3
π	In dextra manu borealior	11	5	8	0	10	31	0		333	5	36	0	9	a	5
ζ	In eadem manu australior præced.	11	5	27	6	8	53	0	b	333	57	0	1	14	a	4
η	Sequens	11	6	53	0	8	10	0		335	35	47	1	25	a	4
ζ	Lucida colli Pegasi	11	12	35	50	17	48	34	b	337	10	33	9	37	b	3
η	Dextrum genu Pegasi	11	22	14	4	35	9	30		337	47	9	28	57		3
ξ	Lucidam colli sequens	11	13	24	23	18	37	28	b	338	30	30	11	2	b	5
λ	Præcedens duarum in pectore	11	19	26	38	28	47	44		338	36	4	22	12		4
μ	Sequens duarum	11	20	49	29	29	26	43	b	339	28	57	23	18	b	4
o	In extr. catenæ annulo Androm.	0	4	14	0	43	47	0		342	31	28	40	59		4
β	Crus Pegasi, Scheat	11	25	54	12	31	8	20	b	342	53	25	26	44	b	2
α	Primæ alæ, Markab	11	19	59	42	19	24	50		343	3	16	13	51		2

⁂ A sketch is annexed to the observations, shewing the places of the comet among the stars with which it was compared; and for this the Greek letters have been substituted, which distinguish them. Dextrum genu Pegasi might seem to direct us to ι rather than to η, but this is the description which is used both by Ptolemy and Flamsteed, and the drawing shews that another smaller star is very near on the south of that in question, as ο is with respect to η.

It was thought unnecessary to reprint the longitudes and latitudes è catalogo Bartschii, which are added to the places determined by observation: and some long speculations are omitted on the supposed identity of the comets of 1748, 1742, 1737, 1723, which are built on little more than the possibility of its return at intervals of about six years.

[To the Rev. Dr. Bradley.]

Margaret-street, Cavendish-square, Aug. 1, 1748.

REVEREND SIR,

THE enclosed paper is a transcript of one which I received last Friday by course of post from a correspondent of mine at Elgin, in the shire of Murray in Scotland; it contains his observations of the late eclipse of the sun, which I think are very curious, and do therefore hope it will not be unacceptable to you. The latitude of Elgin is 57° 40′, and in our common maps it is 2° 45′ west of the meridian of London.

In his letter to me (dated July 15th) he expresses himself thus: "To " my great joy, the day of the solar eclipse was as favourable here as I could " have wished. I have sent you enclosed a note of such observations as I " made; and am sure of being exact as to the duration of the annulus, for " its formation and breaking were most sensible; and it was exceeding pleasant to behold the tremulous, irregular, bright spots* of the sun betwixt his " cusps in the instant before they united and broke. The weather was cloudy " about noon some days before, so that on the preceding day I could not get " my clock adjusted so well as I could have wished. But if there is any " error, I am sure it must be very small; for on this day it was the least I " could possibly perceive."

By this it appears to me that my friend has been very accurate in his observations; for the clocks were then going faster than the sun; and at the end of the enclosed paper, he mentions his clocks being a very little before the meridian. With my best wishes to Mrs. and Miss Bradley, I am,

Rev. Sir, your most obliged humble servant,

JAS. FERGUSON.

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* I cannot think how any part of the sun could be seen betwixt his cusps before they united and after they broke, unless it has been through some inequalities of the moon's limb; and whether the tremulous motion at that time might be occasioned by any thing besides an atmosphere about the moon. (J. F.)

ENCLOSURE.

Observations of the Solar Eclipse July 14th, 1748, made by Mr. Alexander Irvine, Min^{r}. at Elgin, North Britain.

HAVING some days before adjusted my pendulum clock with a meridian line, with a telescope above three feet long I observed the eastern limb of the moon to enter on the western limb of the sun about 30° from the zenith of the sun, at 8h. 57′; though I suspect the eclipse began sooner. The upper part of the breach reached the zenith of the sun about 9h. 6′. The eastern limb of the moon reached the centre of the sun at 9h. 39′. The cusps of the sun joined on the western limb about 30° from the zenith of the sun, and so the annulus began at 10h. 20′. The annulus appeared most perfect at 10h. 22′ 45″; and as nearly as I could observe, I thought it a little narrower on the south-west limb of the sun, than it was on the opposite side. The annulus was observed to break on the south-east limb of the sun, about 30° from the south point, at 10h. 25′ 30″. Before the joining of the cusps of the sun, and at the breaking of the annulus, a tremulous motion was observed, and irregular bright spots of the sun 'twixt the cusps; and the motion of the moon's body was thought to be a little quicker about the time of the annulus, than any other time of the eclipse. Before the western limb of the moon reached the sun's centre, the sun was hid by clouds, and continued so till within some little time before the ending of the eclipse, which was at 11h. 50′.

The darkness during the annular eclipse was not so great as immediately before it; and by the naked eye a bright star (which I took to be Venus) was seen eastward of the sun, a little below the ecliptic. A very good burning glass could not burn during the continuance of the annulus, though it did a little before and after it. The day was pretty fair, only a little cloudy and some rain while the sun was hid. And by the above account the eclipse continued 2h. 53′, and the annulus continued 5′ 30″.

July 15th. Having examined my clock this day at noon, (not having had sunshine for some days past,) I find it is a very little before the meridian; and therefore some small time is to be deducted in all the above observations.

[To the Rev. Dr. Bradley.]

de Londres 12 Aoust 1748.

Monsieur,

JE vous rends encore une fois infiniment grâces de la bonté dont vous m'avez honoré, comme j'avois l'honneur d'observer avec vous et avec M. Bevis le passage de β Draconis par votre grand murail: en effet j'en fus extrêmement charmé, et la seule grâce que je demande encore, c'est de vouloir bien me communiquer la distance vraie ou apparente de β Draconis au zénit de Greenwich, pour que je puisse m'en servir à mon retour à Berlin, parceque cette étoile passe tout proche du zénit de l'observatoire de Berlin. J'ai pris aussi la liberté de vous envoyer un verre objectif de 8 pieds pour l'examiner et centrer. Je suis extrêmement mortifié de la peine que je vous cause; mais je vous assure, Monsieur, que je n'oublierai jamais toutes ces bontés, et je ne souhaite que d'avoir l'occasion de vous témoigner ma reconnoissance. Je me souviens que vous m'avez demandé les dernieres corrections des tables de la lune de M. Euler, c'est pourquoi je m'en vais vous donner un abrégé de l'histoire de ces tables. Il y a environ trois ans que M. Euler a trouvé une formule générale pour déterminer à tout tems le lieu vrai de la lune dans son orbite; mais il avoue lui-même que cette formule n'étant pas absolument intégrable, il a été obligé de changer cette formule en une series, de sorte qu'il a intégré chaque terme en particulier. Ayant donc trouvé cette series pour déterminer le lieu vrai de la lune, il a publié en 1745 pour la première fois ses tables, fondées principalement sur les observations de Mr. Flamsted, et je crois que cette première édition vous sera peut-être inconnue: bientôt après M. Euler a changé la correction, ou equationem centri lunæ établie dans la première édition, et a fait imprimer l'equationem centri sur une feuille à part; mais voyant que le lieu de la lune calculé sur ces tables différoit le plus souvent d'un ¼ de degré de l'observation, il a publié en 1747 la seconde édition de ses tables lunaires, en ajoutant encore quelques corrections qu'il a trouvés en intégrant encore quelques termes de la series pour le lieu de la lune, c'est cette édition qui se trouve dans les opuscules de M. Euler: mais un mois après que ces tables furent publiées, il a trouvé que le lieu calculé sur ces tables ne s'accordoit pas encore avec l'observation, de sorte qu'il a été obligé d'intégrer encore quatre termes de la series; mais les tables étant déjà imprimées, il étoit impossible d'ajouter ces quatre corrections; c'est pourquoi je les ai calculées moi-même pour m'en servir et puisque ces quatre nouvelles équations ne sont pas imprimées, j'ai jugé à propos de vous les communiquer dans cette lettre que je prends la liberté de vous écrire. Vous savez, Monsieur, que dans les dernières tables de la lune de M. Euler (que vous avez sans doute) il y a huit corrections ou équations pour le lieu vrai de la lune dans son orbite ; pour avoir donc le lieu vrai plus exactement, il faut corriger le lieu de la lune corrigé par ces huit corrections encore par ces quatre équations suivantes.

TAB. IX.

Arg. Ab anomalia lunæ subtrahatur anomalia solis.

Gradus.	O. +	I. +	II. +	III. +	IV. +	V. +	
	′ ″	′ ″	′ ″	′ ″	′ ″	′ ″	
0	0 0	0 43	1 14	1 25	1 14	0 43	30
5	0 7	0 49	1 17	1 25	1 8	0 36	25
10	0 15	0 55	1 20	1 24	1 5	0 29	20
15	0 22	1 0	1 22	1 22	1 0	0 22	15
20	0 29	1 5	1 24	1 20	0 55	0 15	10
25	0 36	1 8	1 25	1 17	0 49	0 7	5
30	0 43	1 14	1 25	1 14	0 43	0 0	0
	— XI.	— X.	— IX.	— VIII.	— VII.	— VI.	Gradus.

TAB. X.

Arg. Anomalia lunæ addatur ad anomaliam solis.

Gradus.	O. —	I. —	II. —	III. —	IV. —	V. —	
	′ ″	′ ″	′ ″	′ ″	′ ″	′ ″	
0	0 0	0 38	1 5	1 15	1 5	0 38	30
5	0 7	0 43	1 8	1 15	1 1	0 31	25
10	0 13	0 48	1 10	1 14	0 57	0 26	20
15	0 19	0 53	1 12	1 12	0 53	0 19	15
20	0 26	0 57	1 14	1 10	0 48	0 13	10
25	0 31	1 1	1 15	1 8	0 43	0 7	5
30	0 38	1 5	1 15	1 5	0 38	0 0	0
	+ XI.	+ X.	+ IX.	+ VIII.	+ VII.	+ VI.	Gradus.

TAB. XI.

Arg. A dupla distantia solis a luna subtrahatur (anomalia lunæ—anomalia solis.)

Gradus.	O. +	I. +	II. +	III. +	IV. +	V. +	
	′ ″	′ ″	′ ″	′ ″	′ ″	′ ″	
0	0 0	0 44	1 16	1 28	1 16	0 44	30
5	0 8	0 50	1 20	1 28	1 12	0 37	25
10	0 15	0 57	1 23	1 27	1 7	0 30	20
15	0 23	1 2	1 25	1 25	1 2	0 23	15
20	0 30	1 7	1 27	1 23	0 57	0 15	10
25	0 37	1 12	1 28	1 20	0 50	0 8	5
30	0 44	1 16	1 28	1 16	0 44	0 0	0
	— XI.	— X.	— IX.	— VIII.	— VII.	— VI.	Gradus.

TAB. XII.

Arg. A dupla distantia solis a luna subtrahatur (anomalia lunæ + anomalia solis.)

Gradus.	O. +	I. +	II. +	III. +	IV. +	V. +	
	′ ″	′ ″	′ ″	′ ″	′ ″	′ ″	
0	0 0	0 32	0 55	1 3	0 55	0 32	30
5	0 5	0 36	0 57	1 3	0 52	0 27	25
10	0 11	0 41	0 59	1 2	0 48	0 22	20
15	0 16	0 45	1 1	1 1	0 45	0 16	15
20	0 22	0 48	1 2	0 59	0 41	0 11	10
25	0 27	0 52	1 3	0 57	0 36	0 5	5
30	0 32	0 55	1 3	0 55	0 32	0 0	0
	— XI.	— X.	— IX.	— VIII.	— VII.	— VI.	Gradus.

Voilà, Monsieur, les quatre équations qu'il faut encore ajouter à ces huit équations qui se trouvent dans les tables de la lune de M. Euler in Opusculis ejus. Mais les observations de la lune que vous avez envoyées à M. Euler ont fait voir que le lieu de la lune calculé sur ces tables ne s'accorde pas encore avec l'observation ; c'est pourquoi M. Euler s'est mis à corriger de nouveau ses tables; mais il ne m'a envoyé que les formules, sans avoir calculé les tables; c'est la dernière correction des tables de la lune de M. Euler, qui ne differe pas pourtant beaucoup des tables qui se trouvent dans les Opusculis, en y ajoutant les quatre équations précédentes. Voilà donc, Monsieur, les dernières formules de M. Euler.

Elémens des tables de la lune.

Ayant pris le lieu moyen de la lune sur les tables de M. Cassini, on y ajoutera 1′ 26″ pour avoir la longitude moyenne de la lune. Ayant aussi supputé le lieu de l'apogée sur les tables de M. Cassini, on en retranchera 14′ 31″ et du ☊ il faut soustraire 6′ 5″. Après cela on cherchera l'anomalie excentrique du soleil = ♉, l'anomalie excentrique de la lune = υ, et on corrigera la longitude moyenne par les formules suivantes;

$$\begin{array}{rl}
-\overset{''}{22700} & \sin.\ \upsilon \\
+\ 157 & \sin.\ 2\upsilon \\
-\ 4 & \sin.\ 3\upsilon \\
+\ 680 & \sin.\ ♉ \\
-\ 75 & \sin.\ (\upsilon + ♉) \\
+\ 85 & \sin.\ (\upsilon - ♉)
\end{array}$$

Ayant corrigé le lieu de la lune par les formules précédentes, on retranchera le lieu vrai du soleil du lieu corrigé de la lune; soit donc ☽ − ☉ = η, et on aura les formules suivantes pour calculer le lieu vrai de la lune dans son orbite;

$$\begin{array}{rlcrl}
-\ \overset{''}{128} & \sin.\ \eta & & -\overset{''}{56} & \sin.\ (2\eta + ♉) \\
+2040 & \sin.\ 2\eta & & -75 & \sin.\ (2\eta - ♉) \\
+\ 30 & \sin.\ 4\eta & & +89 & \sin.\ (2\eta - \upsilon + ♉) \\
+\ 80 & \sin.\ (\eta - \upsilon) & & +63 & \sin.\ (2\eta - \upsilon - ♉) \\
+\ 56 & \sin.\ (2\eta + \upsilon) & & +33 & \sin.\ (2\eta - 2\upsilon + ♉) \\
-4725 & \sin.\ (2\eta - \upsilon) & & & \\
+\ 20 & \sin.\ (4\eta - \upsilon) & & & \\
+\ 570 & \sin.\ (2\eta - 2\upsilon) & & & \\
+\ 60 & \sin.\ (4\eta - 2\upsilon) & & & \\
-\ 30 & \sin.\ (4\eta - 3\upsilon) & & &
\end{array}$$

Ayant corrigé la longitude moyenne de la lune par toutes ces formules, on aura la longitude vraie dans l'orbite. Vous voyez, Monsieur, que ces formules sont extrêmement embarrassantes, c'est ce qui m'a empêché que je n'ai pas calculé beaucoup de lieux de la lune sur ces formules, pour les comparer avec les observations; je crois cependant que j'ai trouvé assez souvent encore une différence de 5 ou 6 minutes entre le calcul et l'observation. M. Euler a publié dernièrement une dissertation de tabulis vera momenta noviluniorum ac pleniluniorum exhibentibus; dès que j'aurai reçu ce mémoire, j'aurai l'honneur de vous le communiquer. Par la dernière lettre que j'ai reçue de M. de l'Isle, j'ai été in [formé qu]e l'Académie Royale de Paris vous a reçu m[embr]e à la place de M. Cervi. M. de l'Isle me marque en même tems que toute l'académie s'est applaudie de cette choix, ayant acquis un si digne membre que vous, Monsieur; en vérité je m'en réjouis beaucoup, et j'ai l'honneur de vous en féliciter de tout mon cœur. A cause du mauvais tems on n'a pu observer à Paris ni le commencement ni la fin de l'éclipse du soleil. M. de l'Isle attend impatiemment ce qu'il vous a demandé dans les lettres qu'il vous a envoyées. Je viens de recevoir une lettre de M. le Monnier; il m'a marqué qu'il est allé jusqu'à quarante milles au-dessus d'Edinbourg, pour observer l'éclipse des Pleiades par la lune qui est arrivée le 5 Aoust au matin. Il est parti le 8 Aoust d'Edinbourg pour Londres par la poste, et il arrivera ici Lundi ou Mardi prochain. Enfin je vous prie, Monsieur, de vouloir bien continuer de m'honorer de votre faveur et amitié, vous assurant que je serai toute ma vie avec beaucoup d'estime, Monsieur,

Votre très-humble et très-obéissant serviteur,

A. N. GRISCHOW.

[à M. le Docteur Bradley.]

à Paris, ce 19 Aout, 1748.

MONSIEUR,

J'AI reçu avec un très-grand plaisir l'ouvrage que vous m'avés fait remettre par M. Anderson. Outre l'envie que j'avois depuis longtems de voir ce que vous aviés fait sur les mouvemens apparens des étoiles dus à l'action de la lune sur la terre, je souhaitois d'avoir quelqu'occasion de m'entretenir avec l'astronome dont je respecte le plus les lumières. Et je profite avec bien de l'empressement de celle que m'offre le présent obligeant que vous venés de me faire.

Sans doute que vos observations exerceront beaucoup les géomètres pour chercher la cause des mouvemens que vous avés remarqués. C'est une nouvelle route que vous leur ouvrés; mais la théorie qui doit y conduire est bien difficile, et peut-être toute entière à découvrir. Car celle que M. Newton a donnée sur la précession des équinoxes me paroit bien loin de suffire pour cela. D'ailleurs je vous avouerai que cette théorie ne m'a jamais satisfait, je n'ai point reconnu dans sa méthode de prendre en total les mouvemens des parties de la terre ni la subtilité ni la sureté ordinaire de ce grand homme. Je n'entrerai pas actuellement dans un grand détail sur cette matière, parcequ'il y a déjà fort longtems que je ne l'ai examinée, et que je suis occupé à une autre partie du système du monde, qui mérite aussi une grande attention. C'est la théorie de la lune. Vous aurés peut-être entendu parler d'un mémoire que j'ai lu à l'assemblée publique de la S. Martin dernière. Quoiqu'il en soit, voici de quoy il étoit question dans ce mémoire. J'avois premièrement résolu le problème connu sous le nom de problème des trois corps, dans lequel je déterminois la courbe décrite par un corps autour d'un autre, par la lune, par exemple, autour de la terre, pendant que ces deux corps sont attirés l'un et l'autre par un troisième corps. J'avois toujours trouvé de grandes difficultés dans la théorie de la lune de M. Newton, à cause que tous les points différens qui la composent sont examinés chacun en particulier, comme si les autres ne s'y compliquoient pas, et je ne pensois pas qu'on put être satisfait sur cette matière avant d'avoir vu une solution complète du problème où toutes les considérations entrent à la fois.

La mienne m'a conduit heureusement à une équation[*] de l'orbite de la lune dont tous les termes sont fort simples ; ils ne renferment chacun qu'un sinus d'angle très-aisé à réduire en tables, aussitôt que les quantités qui entrent dans les coefficiens sont déterminés par les observations. Ce que mes recherches m'ont donné de plus important, c'est cette remarque, à laquelle je ne m'attendois pas, que le mouvement de l'apogée n'étoit que d'un peu moins de la moitié de ce que les observations apprennent, en sorte qu'en ce point l'attraction, du moins telle que M. Newton l'a supposée, n'est pas suffisante pour expliquer les mouvemens de la lune.

Ce qui m'a paru de plus simple pour remédier à cet inconvénient, c'est de supposer une autre loy d'attraction que celles des quarrés des distances. En exprimant cette loy ainsi $\frac{1}{d^2}+\frac{a}{d^m}+\frac{b}{d^n}+$&c. on peut rendre les exposans et les coefficiens de ces termes tels que les mouvemens des planètes principales seront sensiblement les mêmes que dans la loy ordinaire, et que le mouvement de l'apogée de la lune qui en résultera sera le réel. De plus on pourra expliquer par la même loy tous les phénomènes pour lesquels on avoit recours à des loix particulières, comme l'ascension des liqueurs dans les tuyaux capillaires, la réfraction de la lumière, &c. La généralité de cette loy m'a parû un avantage. Au reste je ne suis pas attaché à mon explication, et je suis prêt à en accepter toute autre qui répondra aux phénomènes. Je ne m'éloignerois pas, par exemple, de supposer entre l'attraction ordinaire de M. Newton quelqu'autre espèce d'attraction particulière à la terre, et qui agiroit sur la lune. Mais quelque chose qu'on imagine, il me paroît démontré qu'il faut autre chose que l'attraction ordinaire inversement proportionelle aux quarrés des distances.

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[*] La forme de cette équation dans le cas où l'on suppose l'orbite du soleil sans excentricité est celle-cy $\frac{1}{r}=\frac{1}{k}(1-e \text{ Cos. } Y+\alpha \text{ Cos. } 2T+\beta \text{ Cos. } \overline{2T-Y}+\gamma \text{ Cos. } \overline{2T+Y}+$ &c.); r représentant le rayon vecteur k, e, α, β, γ des constantes, Y l'anomalie de la lune, T la distance ou l'élongation des deux luminaires. Les termes qui suivent γ Cos. $\overline{2T+Y}$ sont des cosinus d'angles de même nature comme $2T-2Y$, $2T-3Y$ &c. dont on pourra prendre un nombre plus ou moins grand suivant l'exactitude qu'on voudra obtenir, mais toujours très-petits. Lorsqu'on a égard à l'excentricité du soleil, il entre quelques termes de plus, et le problème ne renferme pas de nouvelles difficultés.

Au reste la théorie de la lune qui résulte de ma solution est fort différente de celle de M. Newton: je ne trouve point comme lui des variations d'excentricité et des inégalités dans le mouvement de l'apogée. Cependant quoique les observations s'accordent avec ces variations, il n'en faut pas conclure que les tables qui résulteront de ma théorie seront démenties par la nature, parceque les différentes espèces de termes qui sont dans mon équation pourront bien faire le même effet que les variations dans l'excentricité et dans le mouvement de l'apogée. Et si les tables de M. Newton cadrent si bien avec les observations, je ne crois pas non plus qu'on doive en inférer que sa théorie l'a mieux conduit au vray, parceque je ne vois pas du tout qu'il ait tiré, comme il l'auroit du, ses tables de la seule supposition de l'attraction mutuelle du soleil, de la terre et de la lune. Mais il me paroit au contraire qu'il a fait ses tables sur les loix du mouvement de la lune déterminées par les astronomes, et qu'il y a fait cadrer sa théorie par des raisons vagues et peu propres à satisfaire les géomètres. J'ai peur de vous paroître trop hardi de parler ainsi de M. Newton; si j'étois connu cependant de vous, vous verriés qu'on ne sauroit en avoir une plus haute idée que celle que j'ai du reste de ses ouvrages, et de l'obligation que tous les mathématiciens lui ont. Mais ce que je dois encore plus craindre, c'est de vous ennuyer par une si longue lettre. Je finis donc en vous assurant de l'estime parfaite, avec laquelle j'ai l'honneur d'être,

Monsieur,

Votre très-humble et très-obéissant serviteur,

CLAIRAUT.

Si vous me faites l'honneur de me répondre, je vous serai bien obligé de me dire ce que M. Machin a fait et publié depuis son petit ouvrage The Laws of Moons, &c. Je n'ai pas été satisfait de ce que j'ai pu découvrir de sa théorie, et j'ai exposé mes raisons dans un mémoire que j'ai lu à l'Acad. en 1743. Je serois bien charmé de savoir ce que vous en pensés au cas que vous ayés pris la peine de le lire, et je voudrois bien savoir aussi si M. Machin en a été informé.

[à M. le Docteur Bradley.]

à Paris, ce 22 Aoust, 1748.

MONSIEUR,

J'AI été très-sensible à l'honneur que vous m'avés fait de m'envoyer votre lettre sur la nutation de l'axe de la terre. C'étoit la pièce que j'attendois de vous avec le plus d'impatience ; depuis que le bruit de cette découverte s'étoit répandu, c'est-à-dire depuis près de dix ans, j'étois las de voir plusieurs de nos astronomes se disputer la gloire d'avoir découvert des variations dans les étoiles, dans la hauteur du pole, dans l'obliquité de l'écliptique, pour pouvoir se vanter de n'avoir pas été prévenus : ce n'étoit que de vous dont on devoit tenir la vérité du fait et les règles de ces mouvemens : aussi dès que j'eus receu votre lettre addressée à My lord Macclesfield, j'en fis une traduction, dont je lus l'extrait à notre académie, qui se félicite de vous avoir acquis pour un de ses membres. J'ai depuis vérifié tous vos calculs pour m'y accoutumer, j'ai aussi construit des tables, tant pour les différentes étoiles du ciel, que pour le soleil, dont les tables doivent prendre l'ancienne forme que Copernic et Lansberge leur avoient donnée, à cause de la précession inégale des équinoxes et de la variation de l'obliquité de l'écliptique. Je n'ai pas encore calculé des tables pour avoir égard à l'effet de la nutation sur les ascensions droites ; il me paroit même par quelques essays que j'en ai fait, que ce calcul n'est pas aisé, mais je n'y ai pas encore beaucoup travaillé.

Comme j'ai beaucoup observé le soleil avec des instrumens très-exactes, je voudrois en construire des tables pour mon usage seulement ; mais je souhaiterois sçavoir ce qu'on pense en Angleterre de l'équation que M. Euler a introduite dans les tables insérées dans ses opuscules, laquelle dépend de la distance de la lune au soleil. J'ai fait quelques essays pour voir si mes observations s'accorderoient à cette équation ; mais faute de sçavoir les règles de la nutation que je n'avois pas alors, j'ai suspendu la suite de cette recherche, qui est fort délicate.

J'ai pris la liberté de vous écrire il y a quatre mois par la voye du Signor Honorati, avant que le bruit de la paix se fût répandu. Je ne sçais si vous avés receu ma lettre avec un livre qui y étoit joint: mais soit que vous l'ayés receuë ou non, je n'ai garde de vous gêner à l'egard de la grâce que je vous y demandois, parceque nous pourrons désormais avoir quelques correspondances en Angleterre, et qu'il ne sera pas nécessaire de vous importuner sur les détails des découvertes qui s'y font tous les jours.

Le projet de souscription pour un catalogue d'étoiles fixes, sous la direction de M. Bevis, m'a fait beaucoup de plaisir. Quoique je travaille sur cette matière, je serai charmé de voir en quoi on aura perfectionné ce qui nous est déjà venu d'Angleterre sur ce sujet.

J'ay l'honneur de vous faire mille rémercimens de votre souvenir. Je serois bien flatté de vous en témoigner ma reconnoissance par quelque service, qui pût vous faire plaisir, n'y ayant personne qui soit plus parfaitement que moi, Monsieur,

Votre très-humble et très-obéissant serviteur,

LACAILLE.

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de Greenwich, ce 24 d'Aoust. v. s. 1748.

MONSEIGNEUR,

PERMETTEZ-moy, s'il vous plait, de vous témoigner la vive reconnoissance dont je me sens pénétré, à l'honneur de votre lettre, et à la marque éclatante qu'elle me donne de la grande bonté du roy, qui a bien voulu me nommer pour remplir une place, dans son illustre Académie Royale des Sciences. Qu'il me soit permis en même temps, Monseigneur, de vous assurer de mon zêle pour la personne sacrée de sa majesté et des vœux que je ne cesserai de faire continuellement pour sa conservation.

Accordez-moy encore, Monseigneur, la permission de faire profession du plus profond respect, et du parfait attachement, avec lesquels j'ay l'honneur d'être, Monseigneur,

Votre très-humble et très-obéissant serviteur,

J. BRADLEY,

de l'Académie Royale de Sciences et Astronome de S. M. Britannique.

A Monseigneur Monseigneur le Comte de Maurepas,
ministre et secrétaire d'état, &c. à Paris.

[To the Rev. Dr. Bradley.]

Friday, Dec. 23, 1748.

DEAR SIR,

AS I am afraid, by being every day in a manner tied down to the direction or correcting of the press in Mr. Neale's affair, that I cannot well spare a day to wait on you before the holidays, I chose to send you the foregoing* extracts for your perusal in the meantime, and propose to wait on you next Wednesday, unless that day shall be inconvenient to you; of which pray advise me by Mr. Oliver. Before this offer of the Abbé de la Caille, it was intended only to print the British Catalogue, Hevelius's, &c. reduced in longitude to the present time, in which case the Abbé's Tables would be an impertinent addition; but as they are done, and he has made so generous an offer of them, I know not with what grace to refuse them, especially considering of what advantage and conveniency they would be to astronomers, along with a catalogue of well determined right ascensions and declinations. If therefore you would be pleased to permit, that those which you have so determined (in the zodiac at least, with a few of the principal extrazodiacals) might be printed by way of Appendix to the Catalogue of Longitudes and Latitudes, it would add infinite value to Mr. Neale's undertaking; who would readily come into the additional expense, without reckoning it to his subscribers, and make you the most grateful acknowledgments for the favour. But in case you should have any thoughts of obliging the public with a catalogue yourself, his request ceases; and he must content himself with furnishing something of this kind hereafter, from my Newington observations; which at present I have no time to reduce, nor is it fit his subscribers should wait the time it would require.

I wish you, sir, a happy Christmas,
and am your most obedient servant,
J. BEVIS.

[Extract of a letter from M. de l'Isle, at Paris.]

...........I have already received so many testimonies of Dr. Bradley's esteem, that I must intreat you to take upon you some of the commissions which I ventured to trouble him with in my last letters, which, as I have received no answer, I fear have incommoded him too much; and for this reason I would not oppress him with any more letters at present. However this be, I should neglect nothing that might be for his service here, of which you may please to assure him; as likewise of the most perfect consideration I have, and ought to have, for so great a man. Pray, sir, pay him a visit on my account, and learn of him what answer he would be pleased I should have to the particulars I wrote to him about. Mr. Grischow tells me, Dr. Bradley was desirous a parcel of his letter to lord Macclesfield, about a new motion of the fixed stars, printed in English*; but the person intrusted therewith has so executed his commission, as to give them into some other hand, two or three of which have fallen to the share of some of my friends, but I never had one: however, M. l'Abbé de la Caille, who understands English very well, having borrowed M. Cassini's copy, has made an extract thereof, which he communicated to the Academy, and I have gotten it printed in the Memoires de Trevoux.

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* In printing it was thought better to prefix the letter to the extracts.

[Extract of a letter from M. l'Abbé de la Caille, at Paris.]

...... It much surprised me to learn from M. de l'Isle, that you have made me a present of the Uranographia: I am at a loss to think how I could deserve so honourable a mark of your esteem. As it is not positively said in the proposals that there are to be any Tables for easily taking out Dr. Bradley's two motions, I presume to inform you, that having calculated such for my own use, they are computed from formulæ far more simple than any I have seen yet published. But the main advantage of them for use consists in their titles, whereby all the perplexity of the signs + and − is avoided. 'Tis likely that a more skilful astronomer would have executed the design better; be that as it will, I absolutely give them up to you and Dr. Bradley, with a discretional liberty to print them all, or in part, or to suppress them entirely. To avoid loss of time, I have put the precepts only into Latin; the titles, which were in French, may be easily made Latin or English.

Permit me to subjoin a few reflections on these Tables, which may the better enable you or Dr. Bradley to make what alterations in them you shall think proper.

1°. The term Deviation, which you mention in your letter to M. de l'Isle, seems properer for denoting the effect of the nutation of the earth's axis; therefore I have used it in the precepts, and it should be introduced in the titles of Tables XI. XII. XIII. and XIV.

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* So in the MS.

2°. In Tables XV. XVII. and XIX. I have put the greatest aberrations in natural numbers, instead of their logarithms, which must be sought in the Tables; this I have done for two reasons: 1. because one sees better what one is about. 2. Because 'tis troublesome to take the proportional parts of the logarithms of such small numbers. However, if these reasons seem insufficient, I can send you these three Tables in logarithms, for in such they were computed.

3°. In Tab. XVIII. it might perhaps be better every where to subtract six signs, to have the places of the sun when the aberration makes the declinations of the northern stars the least of all, and those of the southern ones the greatest. In this case, what is said in precept VII. of the distance of the star from the north pole of the equator, must be cancelled, and in the room of precept XI. should be inserted that which is in note III. putting it Si declinatio stellæ sit australis, instead of Si declinatio stellæ sit borealis As I did not make this reflection till I was about the translation of precept XI. I have made no alteration in Tab. XVIII. This latter choice seems to me the more commodious, and more agreeable to the other foregoing computations.

If you will permit me to say a word or two about my present occupations, you'll please to understand that in the college Mazarin I have procured a very convenient place for an observatory; where I have a mural arch of 4 feet radius, and 140 degrees; a quadrant of 3 feet radius; two excellent clocks; a sextant of 6 feet radius; and I am making a kind of sector, with which I shall be able to measure to an extreme exactness, and with the utmost facility, the distance of any two stars which are not more than three degrees and a half asunder; its radius is 9 feet, and there is no need of illuminating the wires in the focus. I employ myself chiefly in observing the fixed stars. Their right ascensions I observe by corresponding altitudes, six of which, at least, I take on the east, and as many on the west: I compare them all with Procyon or Lyra, whose corresponding altitudes I likewise take every time I observe. I have thus determined, with a surprising accuracy, about 200 stars; that is to say, all those of the 1st, 2d, and 3d magnitude. This winter I intend to verify the principal ones of them, by taking their meridian altitudes with my sextant. My design is to proceed in such manner that there shall not be in the heavens a space of more than three degrees, without a well known fixed star, since with my sector I shall be in condition to determine the distance of any star, from two stars already determined, and I shall not stand in need of a catalogue of the stars of all magnitudes. If this project should not succeed, I shall have the advantage of the right ascensions and declinations of the principal stars to all requisite exactness, for deducing from them the right ascensions and declinations of any of the rest, by the ordinary methods.

As I never fail to take corresponding altitudes of the sun, if I can, I have a very large number of differences of right ascension between Procyon, Lyra, and the sun; and I shall be tempted to make use of them for constructing solar tables for my own use, wherein I shall employ the theory of the nutation, which requires an equation for the true place of the first point of ♈. But would ask your opinion and Dr. Bradley's, if I should employ likewise the equation which Mr. Euler gives in his Lunar Tables, without its demonstration or analysis.

[To the Rev. Dr. Bradley.]

June 20, 1749.

Sir,

I HOPE you will excuse the trouble I give you, as the deference I pay to your judgment is the occasion of it. I had sent the enclosed paper to Mr. Morris last week, desiring him to shew it to you; but happening to see him before he went to Greenwich, he told me you thought we had better say nothing of Dr. Halley's oversight in using the apparent, instead of the true diameter of the moon, and that Mr. Machin was of the same opinion; so I took my paper back again. Upon farther consideration, I find great objections to passing it over in silence. For first, the thing is no secret now, and if the book comes out without taking any notice of it, many people will hear there is something wrong without knowing what, which will do more hurt to the reputation of the book, than any known error can. But what I think of still greater consequence is, if any foreigner should discover it, the blame will not only fall upon Dr. Halley, but upon the English astronomers in general; for if one of his reputation could make such a mistake, and continue in it so many years, and his editors take no notice of it, a foreigner may be apt to conclude that his authority had misled the whole nation. Besides, as it must be discovered, I think it more for Dr. Halley's reputation that it should be mentioned by his editors than not, as it may prevent others from mentioning it in an illnatured manner. I think we should give true precepts, according to the paper you favoured Mr. Morris with; for though following Halley's steps would make our computation agree with his in the particular case of the moon's transit over the meridian, it would only disguise the error there, and be of no service where the moon's apparent diameter ought to be used, as in occultations, eclipses, &c. If you have any objections to what I have said in the enclosed, be pleased to make what alterations you think proper, and I shall have the greatest regard to them. I have sent the table of longitudes and latitudes for your perusal; most of the observations in the Transactions are very unsatisfactory. I would have added the Land's End, or the Lizard Point, but want materials. I have not omitted the odd seconds of longitude or latitude of such places as are taken from the French tables, (or deduced from them, as Babylon, Bagdad,) that I might not seem to endeavour to conceal my borrowing from them; the places to which the letter C is prefixed are from the Connoissance of 1749, which differs from the preceding ones, and from Cassini; therefore I conclude that those places are determined from later observations. I am, sir,

Your obliged humble servant,

MATT. RAPER.

[To the Rev. Dr. Bradley.]

SIR,

June 30, 1749.

I THANK you for the correction of my mistake about the parallaxes, and am sorry I have given you so much trouble. When I had the pleasure of seeing you last, you thought it would be proper to give a hint in the preface of the usefulness of publishing the observations. If you do not approve of what follows, and will be pleased to let me know in what manner you would have it altered, I will endeavour to make it to your mind.

Catalogo Britannico ad lunæ ascensiones rectas ex fixarum observatione indagandas usus est noster, in quo fixarum quarundam loci minus accurate determinati inveniuntur; et si observationum ejus editionem sperare licuit, fixas illas indicare ut inde lunæ loci ab illis derivati corrigerentur, operæ pretium foret. Utinam auctor dum viveret illas edidisset; magni enim interest ut observationes astronomicæ publici juris fiant, quippe quæ nunquam, ut tabulæ, tractu temporis exolescant, sed si diligenter factæ fuerint et bona fide traditæ utilitatem ex antiquitate ducunt. Optandum propterea foret ut sumptibus publicis subinde ederentur observationes ab astronomo regio factæ; quo enim magis sedulus observator fuerit, eo magis publici interest ut ejus edantur observationes, eoque minus privati hominis facultatibus sumptus illi conveniunt.

I suppose it will be proper to insert the corrections Dr. Halley made in the numbers of Mercury (Phil. Trans. N° 386.) in the preface. I hope your building goes on to your mind, this is fine weather for it. Be pleased to make my compliments to your lady and nephew. I am, sir,

Your obliged humble servant,
MATT. RAPER.

[To the Earl of Macclesfield.]

I WAS informed at Mr. Basket's that the tables were all printed off, so that those alterations could not be made which your lordship proposed. I have sent your lordship a copy of the general tables in the form which I at present judge they may be disposed. If it be proper to print these general tables before the bill is read the first time, I believe your lordship will think it needless to add any thing concerning the rules or manner of continuing them at pleasure, since they will be so very obvious from the bare inspection of the tables.

I have put table B two ways; in the first the figures at the top of the columns, and the Sunday letters go on in direct order, but then the centuries increase from the right to the left. The other is just the reverse.

I have not time to examine whether I am mistaken in the disposition of the numbers or letters, but am unwilling to omit the opportunity of sending this, as your lordship could not otherwise have heard from me till near the end of the week, and as your lordship may perhaps before that find leisure to try whether all is right or not.

Time will not permit me to word the rules properly, that are for finding the Sunday letter and the order of the golden numbers for any particular century; your lordship will consider that matter more at leisure; I shall likewise when I get to Oxford.

[To Dr. Mortimer.]

Greenwich, Oct. 12, 1750.

SIR,

MR. MORRIS informed me on Wednesday, that you intended soon to write to M. de l'Isle, and could at the same time send any letter, &c. to him, if I transmitted it to you. I have enclosed one for him, but not knowing his proper address, I beg you will please to supply what is wanting in the direction. I have sent him a copy of about 200 observations of Jupiter's satellites, that were formerly made at Wansted, &c. and which you wrote to me about when I was at Oxford. I did not make any observation on the 4th satellite of Jupiter in December last, which M. de l'Isle wrote for in his letter Mr. Morris shewed me.

I am, sir,
Your most obedient servant,
J. B.

[à M. de l'Isle.]

Greenwich, Oct. 12, 1750.

SIR,

DR. MORTIMER having informed me that you are preparing to publish a collection of all the observations you can procure of the eclipses of Jupiter's satellites, and that you desired I would communicate to you those which had been made at Wansted, either by Mr. Pound or myself, I have enclosed a copy of those that have been taken there, and hope they will not arrive too late to be inserted in their proper place.

The time in which all our observations (except the first seven) are registered, being mean time, it may be proper to take notice, that the equation of time which we made use of is the same as may be collected from Dr. Halley's Tables.

The greatest part of these observations were made with the 15f. refracting telescope; a few that are marked with an asterisk were made with the Hugenian glass of 123f. focus; and those which have the letter R annexed were taken with a very good 5f. reflecting telescope of the Newtonian construction, which was made by the late Mr. John Hadley, and presented by him to the Royal Society; of which some account has formerly been given in the Philosophical Transactions, N°. 376 and 378.

Besides the observations taken at Wansted, I have added some that were made at Oxford, either by myself or my worthy colleague Mr. Bliss, which are denoted by the letter B. Oxford lies 5′ 0″ in time westward; and Wansted 0′ 8″ eastward of Greenwich.

I am, sir, with great respect,
Your most obedient humble servant,
J. B.

Observations at Wansted	170
at Oxford	30
in all, about	200

[à M. le Docteur Bradley.]

à Paris, le 31 Janvier, 1751.

MONSIEUR,

J'AY reçu par le canal de M. Mortimer votre lettre de Greenwich du 12-23 October dernier, avec toutes les observations des satellites de Jupiter, que vous et M. Pound avez faites à Wansted, auxquelles vous avez bien voulu ajouter celles d'Oxford de M. Bliss. On ne peut être plus reconnoissant que je le suis de ce beau présent, donc je tacherai de faire le meilleur usage qui me sera possible pour l'utilité de l'astronomie, et surtout pour votre honneur et celui de la nation Angloise, qui a procuré une si grande quantité de bonnes observations. Vous avez pû voir mon sentiment sur les tables que vous avez publiées des satellites de Jupiter avec celles de M. Halley, à sçavoir que je ne crois pas qu'il y ait de meilleurs fondemens pour la théorie de ces satellites, en restant à y ajouter que ce que les nouvelles découvertes et recherches feront connoître dans la théorie de ces satellites, ce qu'il sera toujours aisé de faire lorsque l'on se sera assuré par les observations des inégalitez du mouvement de chaque satellite. Les observations que vous m'envoiez finissent au 5 Nov. 1740. Ces observations n'ont-elles pas été continuées, et ne se continuent-elles pas encore à présent, soit à Londres ou à Greenwich? comme il seroit avantageux que cela se fit pour s'assurer toujours de plus en plus des inégalitez de leurs mouvemens. Comme je crois que vous avez des aides dans vos observations, il seroit à souhaiter que l'on n'en obmît en Angleterre aucunes de celles que le ciel vous permettra de faire. Vous m'obligerez trez-fort de vouloir bien me dire ce qui en est, et me procurer les observations

d'Angleterre, sur ces satellites, postérieures à celles que vous m'envoiez, dans quelqu'endroit et par qui que ce soit qu'elles aient été faites.

Monsieur Mortimer m'aiant mandé le mois d'Aoust dernier que vous étiez fort occupé à ranger et mettre en ordre les nouveaux instrumens dont votre Amirauté avoit fait présent à l'observatoire de Greenwich, je l'ai prié de me dire en quoy consistoient ces instrumens; et il m'a répondu, qu'il vous en avoit parlé en vous priant de m'en envoier le catalogue. Je me joins à lui pour cela; rien ne me sera plus utile dans la suite de la correspondance que je vous prie d'avoir avec moy au sujet des observations astronomiques, que de sçavoir les instrumens que vous avez.

Lorsque j'ay annoncé à M. Mortimer il y a quelques mois le voyage de M. de la Caille au Cap de Bonne Espérance, il m'a fait réponse, que cette nouvelle avoit fait beaucoup de plaisir aux astronomes de la société. Comme vous en êtes le chef, je ne doute que vous n'y preniez plus de part que tout autre; ainsi c'est principalement à vous que doit être envoié l'avertissement imprimé de M. de la Caille, dans lequel il marque les étoiles qu'il se propose de comparer avec la lune, Mars, et Vénus, pour en conclure la parallaxe de la lune et du soleil par la comparaison que l'on fera de ses observations avec celles d'Europe.

Personne n'est en Europe fourni de meilleurs et plus grands instrumens pour faire ces observations que vous l'êtes à Greenwich; et comme je ne doute pas que vous n'aiez ou ne puissiez avoir aisément tous les aides qui vous seront nécessaires, c'est de vous, Monsieur, que l'on attend les plus exactes observations que l'on fera sur ce sujet en Europe. Vous aurez apparemment différens instrumens, avec lesquels vous pourrez déterminer avec la mesme précision les mesmes différences de déclinaison de la lune et des étoiles que demande M. de la Caille: il seroit avantageux que vous prissiez par le moien de vos aides ces différences de déclinaisons avec tous les instrumens qui les peuvent donner assez exactement, ce qui servira à confirmer les différences que l'on cherche.

Outre ces différences de déclinaisons, il seroit fort à souhaiter que l'on voulût bien faire, en mesme tems dans quelques uns des plus célèbres observatoires de l'Europe, les observations qui peuvent servir à déterminer exactement la parallaxe horizontale de la lune. Vous en verrez aisément la raison fondée sur ce que ces parallaxes doivent varier suivant les différentes figures que l'on donne à la terre, et s'il est vray qu'elle soit applatie par les poles, comme elle l'est effectivement, il en doit résulter différentes déterminations de la parallaxe de la lune pour les différentes latitudes; surtout lorsque l'on comparera ces parallaxes horizontales déduites des parallaxes en ascension droite avec les parallaxes que l'on tirera des observations de M. de la Caille, comparées avec celles qui se feront en Europe.

J'ay parlé de la nécessité de ces différentes sortes d'observations dans une lettre circulaire manuscrite, que j'ay écrite aux astronomes, et dont j'ay envoié une copie à M. Mortimer, qu'il vous communiquera, si vous en avez besoin. Je me suis étendu davantage sur ce sujet dans les lettres imprimées, dont je vous enverrai un exemplaire par la première occasion: si vous approuvez mes raisons, j'espère que vous voudrez bien nous procurer des observations pour la parallaxe horizontale de la lune par les parallaxes horaires dans les tems et les circonstances que vous trouverez les plus avantageuses.

Vous verrez dans la lettre circulaire manuscrite que j'envoie à M. Mortimer, que je recommande aux astronomes les observations exactes des hauteurs méridiennes du soleil, et celles des principales étoiles fixes, à mesure qu'elles approcheront du parallèle de ses bords; c'est une observation que M. de la Caille a obmise dans son avis imprimé, et dont il m'a chargé d'avertir les astronomes, qui sont capables d'observer ces différences de déclinaison à une ou deux secondes prez.

Je vous prie, Monsieur, de vouloir bien continuer à m'honorer de votre correspondance par le canal de M. Mortimer, et d'être persuadé des sentimens d'estime et d'attachement, avec lesquels j'ay l'honneur d'être,

Monsieur, votre trez-humble et trez-obéissant serviteur,

De L'ISLE.

[à M. le Docteur Bradley.]

MONSIEUR, à St. Petersbourg, ce 20 Juillet 1750.

LE P. TAUBERT ayant représenté à M. le Président que vous vous ferez plaisir d'être en correspondance avec l'académie, il m'a chargé, Monsieur, de vous en marquer sa satisfaction par l'envoy du premier tome de nos nouveaux commentaires, avec le supplément d'un traité de M. Euler, qui sera certainement agréable aux savans d'Angleterre, qui font de cette matière leur étude favorite. J'ay l'honneur d'être,

Monsieur, votre très-humble et très-obéissant serviteur,

SCHUMACHER.

[à M. le Docteur Bradley.]

à St. Petersbourg, ce 2 Avril 1751.

MONSIEUR,

L'ACADEMIE Impériale des Sciences de St. Pétersbourg ayant résolu de mettre l'observatoire d'ici dans un état qui promet les plus grands avantages pour l'astronomie, a trouvé nécessaire de prendre la résolution de l'enrichir d'un quart de cercle mural propre à faire avec la dernière exactitude toutes les observations qui se font avec cette espèce d'instrument astronomique. Pour cet effet elle a jugé à propos de faire faire à Londres par le S^r. Bird un quart de cercle mural de huit pieds Anglois de rayon, semblable à celui que j'ai vu à Greenwich dans le temps que j'avois le bonheur de profiter de votre agréable conversation. Or ces sortes d'instrumens ne demandant pas seulement un habile ouvrier, mais encore un directeur expérimenté qui dirige ce grand ouvrage, l'Académie Impériale m'a chargé de vous supplier, Monsieur, de vouloir bien lui accorder le service d'instruire le dit méchanicien sur tout ce qui concerne une construction exacte de cet instrument, et d'avoir ensuite la bonté de l'examiner selon la rigueur d'observer qui vous est propre.

Quoique je n'aie nullement besoin, Monsieur, de vous dire quelque chose touchant la construction et l'exactitude de cet instrument, me confiant entièrement en vos propres lumières, vous me permettrez pourtant de vous expliquer un peu la façon la plus propre dont il me semble qu'il faudra construire cet instrument, afin que le S^r. Bird puisse se régler là-dessus.

1. Il faudra en général prendre pour modèle le quart de cercle mural de votre fameux observatoire, et surtout celui que le S^r. Bird a fait en dernier lieu pour votre usage, vû que je ne doute nullement que vous n'ayez fait appliquer quelque chose qui tende à l'exactitude et à la commodité d'observer.

2. Il faudra le faire de huit pieds de rayon; la lunette qui roulera sur quatre petites roues sur le limbe du mural, et sa carcasse garnie d'un double contrepoids (comme au mural de My lord Macclesfield) auront environ la même longueur.

3. La carcasse et tout l'instrument peut être, si on le juge à propos, de cuivre fort, surtout le limbe de l'instrument, qui doit être d'une épaisseur considérable.

4. Sur le limbe il y aura deux divisions, dont l'une divise l'arc de 90° en

96 parties, et l'autre en 90. Moyennant la division de Nonius et le petit micromètre appliqué en bas de l'alidade (comme au quart de cercle de Greenwich) on aura les secondes.

5. Le limbe doit comprendre environ 100 degrés, afin qu'on puisse observer jusqu'à 5 degrés, ou environ au-delà des points de 0 et de 90°.

6. Pour ce qui regarde la manière de diviser l'arc de 90° en parties égales, je crois qu'il ne sera pas mal à propos, quand on a déterminé une fois les points de 0 et de 90°, de diviser tout de suite l'arc total de 90° en 1080 parties égales, en essayant par plusieurs reprises jusqu'à ce qu'on ait attrapé la juste grandeur de la $\frac{1}{1080}$ partie de l'arc total, ou d'un intervalle de cinq minutes. Il est vrai que cette manière de diviser est un peu pénible au méchanicien: mais il n'est pas impossible d'y réussir, vû que j'ai fait essayer cette manière de diviser sur un quart de cercle de trois pieds de rayon, et en ai tiré un bon succès. Si l'on divise soigneusement un quart de cercle de cette manière-là, (en observant neanmoins les autres conditions, dont j'ai parlé dans la lettre au S^r^. Bird) on peut au moins être sûr que l'arc de 90 est exactement divisé en parties égales, c'est ce qui n'est pas un petit article dans l'astronomie pratique. Il faut au reste bien prendre garde de ne point frapper les points dans le limbe, comme quelques méchaniciens ont accoutumé de faire, mais de les y porter successivement, en faisant promener le compas à verge du point de 0 au point de 90, et du point de 90 au point de 0, jusqu'à ce que les points soient suffisamment marqués.

7. Les points de divisions doivent être aussi fins que cela se pourra, et tous également gros; et pour mieux connoître les points de division, il sera nécessaire d'appliquer au limbe du quart de cercle deux bonnes loupes, qui soient toujours perpendiculairement mises sur chacune de ces deux divisions. Aux points sur lesquels tombent les fils à plomb il doit y avoir également une bonne loupe.

8. Le centre du quart de cercle doit être marqué sur un petit clou d'or, afin que la rouille ou le verd de gris ne s'y mette point.

9. Il faudra appliquer à l'instrument deux fils à plomb, de façon qu'on ne puisse pas seulement le placer verticalement, mais encore observer les variations ou déviations, tant latérales que verticales, que l'instrument pourroit souffrir avec le temps.

10. Le limbe de l'instrument, dont le plan exige un soin tout particulier du méchanicien, doit être couvert d'une espèce d'étui de différentes pièces, qu'on peut ôter et remettre après l'observation.

11. Le méchanicien appliquera à l'instrument toutes les vis et machines qui servent à diriger commodément le mural dans le plan du méridien, et à l'y fixer en le scellant dans un mur. Il ajoutera de même la boëte et tout ce qui est nécessaire pour transporter l'instrument sans danger à St. Pétersbourg.

Au reste, Monsieur, l'Acádemie Impériale remet le reste de la construction de cet instrument à vos soins, de sorte que si vous trouviez que quelques-uns des articles précédents furent trop difficiles dans la pratique, quoique je ne le croie pas, vous pourrez les changer comme vous le jugerez à propos. Mais si au contraire vous trouviez quelque chose à y ajouter, l'Académie vous fait prier de même d'en informer le S^r. Bird.

Mais comme l'Acádemie Impériale est intentionnée de faire faire ici l'hyver prochain les observations correspondantes à celles que M. de la Caille fera au Cap de Bonne Espérance, elle vous fait prier, Monsieur, de disposer le S^r. Bird afin que cet instrument puisse arriver ici au moins avec les derniers vaisseaux qui partiront cette année de Londres pour S^t. Pétersbourg. Pour cet effet, et pour être sûr de son fait de part et d'autre, l'Académie souhaite que le S^r. Bird fasse un contrat, moyennant lequel il s'oblige de délivrer le dit instrument dans un temps déterminé, pour le prix et sous les conditions que l'Académie Impériale vous fait prier de conclure en son nom avec le S^r. Bird, en ayant la bonté d'informer l'Académie le plutôt que votre commodité le permettra. L'Académie s'oblige aussi de son côté de payer à l'ordre que le S^r. Bird indiquera la somme dont vous serez convenu. Mais afin que l'ouvrage ne soit point rétardé par le temps qu'il faut pour attendre les réponses, vous aurez la bonté, Monsieur, de faire en sorte que le S^r. Bird commence à travailler à cet instrument incessamment après l'arrivée de cette lettre.

Au reste, Monsieur, je vous supplie d'être persuadé que vous ne rendrez pas seulement par-là un service à l'Académie Impériale, dont elle ne manquera assurément pas de vous témoigner sa vive reconnoissance, mais que vous donnerez encore par-là à moi en particulier un excellent moyen de mettre en pratique le zèle et l'envie que j'ai de produire des fruits dignes de la place que j'ai l'honneur d'occuper depuis quelques mois. Soyez enfin bien assuré, Monsieur, je vous en supplie, que je n'oublierai jamais les bontés et politesses que j'ai reçues de votre part, mais que je m'efforcerai au contraire de vous convaincre de plus en plus de ma parfaite reconnoissance dans toutes les occasions qui se présenteront, et que je serai toujours extrêmement charmé de recevoir de vos agréables nouvelles, comme une marque de la continuation

de votre chère amitié, que je vous supplie d'accorder à celui qui se fait un honneur d'être véritablement toute sa vie, avec toute l'estime et considération possibles, Monsieur,

Votre très-humble and très-obéissant serviteur,

A. N. GRISCHOW.

Quand vous aurez la bonté, Monsieur, de m'honorer d'une réponse, vous me ferez un très-grand plaisir de me marquer si le micromètre à quatre verres oculaires a réussi, et de me dire quelque chose des avantages que vous croyez qu'on peut tirer de cette façon de micromètre.

[To the Rev. Dr. Bradley.]

Greenwich, April 9, 1752.

HON. SIR,

* * * * *

Mr. Robertson called here about a fortnight since, by order of Mr. Hodgson, of Christ Hospital, who ordered him to let you know that he hath in his possession an original picture of Tycho Brahe, which Mr. Flamsteed left after the death of Mr. Hodgson, to be put in the Picture Gallery at Oxford. Mr. Robertson says, that if a proper person is sent to Mr. Hodgson from Oxford, that Mr. Hodgson will deliver the picture to him in his lifetime, and should be glad to have it sent for soon. Mr. Robertson says, that Mr. Hodgson don't expect he shall be able to live long, and would be glad to see the picture removed before his death.

I have sent the differences in right ascension between the principal stars that have been observed by the new transit instrument, reduced to the beginning of the year 1751. I have sent the particulars of the observations. and under each difference, the mean difference by the number of observations against it; beginning with the difference between Aldebaran and Capella, then Capella and Rigel, and so on to α Cygni, and then between α Cygni and Aldebaran; and I make the sum of the whole differences to be 360° 0′ 21″,1.* I have not examined the numbers yet, but took what care I could in adding them up to have the mean of each difference.

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* Bradley has drawn his pen through 21″,1 and written 4″,3 in the place.

I was very glad to hear that you and all friends were well at Oxford, as I hope you continue, sir; pray make my duty to my aunt, &c. and kind love and compliments to Miss Bradley, and all other friends.

I am, sir,

Your most dutiful nephew,

JOHN BRADLEY.

The foreign letter I have just received.

[à M. le Docteur Bradley.]

à Paris, le 15 Juillet 1752.

MONSIEUR,

VOUS avez été un des premiers et des principaux à qui l'Académie des Sciences de Paris, dont vous êtes membre, a souhaité de recommander les observations correspondantes à celles que M. de la Caille est allé faire au Cap de Bonne Espérance, par ordre du Roy, pour la recherche de la parallaxe de la lune et de celle du soleil par le moien de celles de Mars et de Vénus. Je vous ai fait tenir pour cet effet l'avertissement de M. de la Caille, dans lequel il avoit marqué toutes les étoiles, auxquelles il s'étoit proposé de comparer la lune, Mars et Vénus; et j'ay appris avec plaisir de M. Mortimer l'intérêt que la Société Royale, et vous en particulier, Monsieur, avez bien voulu prendre à un si utile projet. Je ne doute pas qu'il ne vous ait réussi à faire plusieurs des principales observations, dont votre habileté et la bonté des instrumens que vous y aurez emploiez nous garantiront l'exactitude. Comme M. de la Caille a fini ses observations, et que nous les avons reçues, je vous prie de vouloir bien nous envoier celles que vous avez pû faire qui sont indiquées dans son avertissement. J'ai déjà rassemblé toutes celles de Suède, de France, et d'Italie, et je suis occupé à les comparer avec celles de M. de la Caille, premièrement pour la recherche de la parallaxe du soleil, et ensuite pour celle de la lune, qui tiendra beaucoup plus de tems. S'il vous est trop pénible de m'envoier toutes ces observations, vous pouvez commencer par m'envoier celles de Mars et de Vénus, et ensuite celles de la lune.

J'avois recommandé, de la part de M. de la Caille, aux astronomes de ne pas obmettre la comparaison des hauteurs méridiennes du soleil et des principales étoiles fixes qui auroient passé le plus prez de son parallèle, pour en pouvoir déduire immédiatement la parallaxe du soleil. Je ne doute pas que

M. de la Caille n'ait aussi fait plusieurs de ces observations avec toute l'exactitude dont il est capable, et que peut comporter la grandeur des instrumens qu'il y a emploiez; mais nous n'avons point encore reçu ces sortes d'observations. Il les a apparemment reservées avec celles qui lui serviront à établir son catalogue des étoiles australes. Nous communiquerons à la Société Royale les observations de M. de la Caille, avec les résultats que l'on en aura tirez. Par les observations de deux éclipses de lune, et plusieurs observations du premier satellite de Jupiter, l'on a déduit la longitude du Cap de Bonne Espérance d'une heure cinq minutes à l'orient de Paris; et M. de la Caille a conclû la latitude du lieu où il observoit de 33° 55′ 12″ australe. Vous n'avez pas oublié, je crois, mon addresse, qui est au College Royal, Place de Cambray: vous pouvez m'y addresser directement vos lettres, ou me les envoier sous le couvert de M. Mildmay, commissaire nommé par l'Angleterre pour le réglement des limites, &c. Je suis avec le plus parfait attachement et une considération infinie, Monsieur,

Votre trez-humble et trez-obéissant serviteur,
DE L'ISLE.

[à M. le Docteur Bradley.]

à Paris, le 15-26 Juillet 1752.

M. DE CHABERT, officier de la marine du roy, me demande, Monsieur, que je vous prie de lui communiquer les observations de la lune correspondantes à celles qu'il vient de faire à Louisbourg en Amérique pendant les années 1750 et 1751: je lui ai déjà communiqué celles que j'ai faites; mais je n'ai pas toutes celles qu'il désire. En voici la liste (M. de Chabert s'est transporté au Cap de Sable, à l'Isle, au Détroit de Fronsac, &c.)

I. Le 16 Novembre 1750 n. st. au matin j'ai observé les passages au méridien de la lune et de ζ des gémeaux. Et M. de Chabert en a observé l'émersion à Louisbourg.

II. Le 8-19 Dec. j'ai eu encore le passage de la lune au méridien, que j'ai comparé aux étoiles *q* et *d* du Lion. Et M. de Chabert a mesuré ce jour-là les distances de la lune au soleil avec son quart de cercle de 2 pieds de raion, semblable à celui qui nous a servi en Lapponie.

III. Le 10 Janvier je n'ai point d'observation correspondante à une occultation que M. de Chabert a déterminée ce jour-là, ensorte qu'il ignore l'erreur des tables. Le P. Pezenàs, jésuite, de Marseille, nous en a communiqué une qu'il a faite, mais elle n'est pas exacte, s'étant servi d'un quart de cercle mural, dont les erreurs du plan ne sont peut-être connues qu'à 0h. 0′ 10″ près.

IV. J'ai bien observé les passages de la lune du 4 et 6 Avril; mais n'en aiant pas du 5, M. de Chabert désireroit en avoir une observation correspondante, à cause qu'il a mesuré ce jour-là les distances du bord de la lune à Procyon et α de l'Hydre.

V. Pareillement je n'ai d'observations que le 5-16 et 8-19 Avril, au lieu que le 6-17 au matin M. de Chabert a mesuré la distance des bords de la lune et du soleil et auparavant celle de la lune à ξ de Capricorne.

VI. Il reste une observation du 18 Juin, au quel jour j'ai observé les passages de la lune et d'Arcturus, et M. de Chabert a mesuré encore cette fois-là les distances de la lune au soleil.

Je suppose, Monsieur, que vous avés eu l'année passée le tome de l'acad. de 1746, avec un double exemplaire du 1er. cahier de nos observations de la lune depuis 1733-1736, que j'avois laissé à M. Short. Cette année je vous ai envoié par un de nos amateurs de Grandville le volume de l'acad. de 1747, et on ne tardera pas à distribuer 1748: vous pourrés m'addresser quelque Anglois, s'il s'en trouve, pour vous le remettre avec l'Almanac, ou Connoissance des Tems, que j'ai déjà.

L'on imprime ici les observations du Cap de Bonne Espérance, mais à vous dire le vrai, je ne vois pas de quel usage cela peut être jusqu'à ce que l'autheur ait vérifié ses divisions, ce qu'il a dessein d'exécuter pendant le mois d'Octobre prochain, suivant sa dernière lettre addressée à l'académie. Mon correspondant M. d'Après m'a envoié aussi de l'Isle de France une occultation d'étoile par la lune qu'il a observée le 13-24 Octobre 1751. J'étois justement de retour à Paris depuis trois jours, et j'ai moimême observé ce jour-là le passage de la lune au méridien et sa hauteur. Quand on aura distribué le feuillet volant des observations du Cap, je vous l'enverrai par la poste, si vous le désirés. J'ai l'honneur d'être, Monsieur, avec l'estime et le respect que je vous conserverai toujours,

Votre très-humble et très-obéissant serviteur,
LE MONNIER.

P.S. Je vous prie de me faire sçavoir comment et par quel artifice dans le niveau de M. Graham on s'assure que le fils d'argent effleure la règle horizontale de cuivre, et jusqu'à quel point (s'il n'y a pas quelque artifice que je ne connois pas) on peut s'assurer que le fil effleure le règle horizontale.

[To M. le Monnier.]

Sir,

I LATELY had the pleasure of your letter relating to some observations of the moon made at Louisbourg by M. de Chabert, and I have examined my journal in order to see whether we had made any here that would correspond with his, but am sorry to find that clouds prevented us from making such as would tally with his more properly than some that are inserted below; however, as these are nearest to the times you specified, I thought it might not be improper to send them, though some of them were not made on the days required. Although we seldom omit to observe the moon's passage over the meridian when she happens to be visible, yet the variable weather of this climate occasions many and long interruptions in the series of our observations, so that it must frequently happen (as in the present case) that there will be a deficiency of corresponding observations.

I suppose the day to begin always with the sun's passage over the meridian; but my clock being regulated so as to go sidereal time, shews the (horary) right ascension of the midheaven nearly; and the real error of it may be readily and exactly determined from the passage of the sun or stars over the meridian, which were observed (as likewise the moon) with a very good transit instrument well adjusted to the meridian, the telescope whereof is 8 feet long. I have sent you a copy of the observations as entered in my journal while they were making, without taking any notice of the error of the clock; except with regard to what it gained per diem, where that would not appear from these observations themselves; this being all that you will want to know in order to settle the moon's right asc. and the true time of her passage over the meridian.

The apparent zenith distances of the moon's limb were taken with my large mural quadrant, and want only to be corrected by the proper refractions; but it should be remarked, that I find the true latitude of this observatory 8″ or 10″ greater than what Mr. Flamsteed supposed it to be.

Mr. Short informed me that he had sent you an apparatus for examining the quantity of the arch of a mural quadrant, while it hangs upon the wall, by means of a spirit level, after the model of one contrived by Mr. Graham for my use; but I perceive by what you write in your postscript, that you meet with some difficulty with regard to the manner of using it. I wish you could have had the opportunity while you were in England of seeing the whole process of the experiment, and the manner of putting together the several parts of the apparatus; because that would have obviated the present doubt, which it may not perhaps be easy to remove by a bare description of the method of making the experiment, which was done by myself in the following manner.

One of the two pins that support the quadrant being moveable, I first of all (by the help of a plumbline) brought the centre to lie exactly over the beginning of the divisions on the limb, or the zenith point; (my divisions being numbered from thence;) then the sliding brass plates of the apparatus being fastened upon pieces of wood that grasped each end of the wall on which the quadrant was hung, I stretched a fine silver wire horizontally, and letting it bear upon the edges of the sliding pieces, and bringing it to be very near to the centre plate and the limb of the quadrant, (but not suffering it actually to touch either,) then by raising or depressing the sliding plates, on whose edges the wire rested, I moved it till it appeared to bisect the centre point of the quadrant, and the point of 90° on the limb. When the horizontal wire is thus placed, if the arc of the quadrant be exactly equal to 90 degrees, the wire itself must lie precisely horizontal, as being at right angles with the plumbline that passed through the centre, and therefore the edges of the brass plates that support this wire must also lie in an horizontal plane. Now whether these edges do really lie in a plane exactly parallel to the horizon or not, may be found by means of the level, by hanging the ends of the brass pieces (which are fastened to the board on which the level is to be placed) upon the edges of the sliding plates, as near as may be to the wire that bears on them, for if the bubble of the level stands at the same point when the board is turned end for end, the edges must lie exactly in an horizontal plane; but if it does not stand at the same point, then by turning the screw of the level till the bubble returns to the place it stood at before the board was changed end for end, and taking notice how many divisions the index of the screw was altered, half that number (reduced to minutes and seconds of a degree) will shew how much the arch of the instrument exceeds or falls short of 90°. N. B. The planes of the sliding brass plates must be placed nearly at right angles to the plane of the quadrant, and one plate ought to be about as far to the south of the centre of the instrument, as the other is to the north of the point on the limb; for then no error can arise from the bending of the wire into a curve by its own weight. Before the wire is laid to bear upon the brass sliding-plates, their edges must be brought to lie truly horizontal by help of the small level that is made for that purpose.

I know not whether I have guessed rightly with respect to the point you wanted to have explained, or whether what is now written will enable you better to understand the manner of trying this experiment; but if I have failed in either, I doubt not but a more particular examination of the several parts of the apparatus will help you to discover their respective uses; and I am persuaded (if your apparatus be properly executed) that you will be well pleased to find that this method will enable you to be assured of the true quantity of the arc of any quadrant, to as great a degree of exactness as can be desired. For in the year 1745, when I examined my large mural quadrant, and had repeated the experiment by new setting the plumbline, new adjusting the plates and horizontal wire, and turning the level near thirty times, the greatest difference that I found between the extremes of the several trials did not amount to five seconds; and therefore 'tis highly probable, that the mean of all the results would not err a single second from the truth.

I take this opportunity, sir, of returning you my thanks for the two printed copies of your observations of the moon, &c. made between 1733-1736, which I received in due time; but I know not where the tome de l'Acad. 1746 is left, neither have I been able to learn the gentleman's name who had promised you to take care that it should be delivered to me. The tome de l'Acad. 1747 I have lately received, the person who had taken the charge thereof having been so kind as to bring it to the observatory. But as it may perhaps be less trouble to yourself, and a more sure method of conveyance, I have desired Mr. Vaillant, a bookseller in London, to permit that any book, &c. which you or any other gentleman are so kind as to transmit to me, might be packed up among such books or papers as are sent to him by either of his correspondents;—Messrs. Hipp. Louis Guerrin, or Ant. Claude Briasson, libraires, ruë St. Jaques, à Paris.

I am, with great respect, sir,
Your most obedient humble servant,
[J. BRADLEY.]

⁂ Annexed to this letter is a paper containing a number of observations of the moon between Nov. 3, 1750, and Oct. 13, 1751, some of which M. de Chabert has made use of in his Voyage dans l'Amérique Septentrionale. The right ascensions are exactly the same as may be found in the first volume of the Greenwich observations; and the zenith distances are reduced according to the directions given to de l'Isle in p. 477.

[To M. Grischow.]

Greenwich, July 22, 1752.

Sir,

MR. BIRD having now finished the mural quadrant for the observatory at St. Petersbourg, I have (as you desired) carefully examined the several parts thereof; and they appear to me to be so well executed, that I cannot doubt but that this instrument will give you great satisfaction when it is fixed up in its proper place.

As the bulk and weight thereof render it cumbersome, and upon that account unfit to be moved oftener than is absolutely necessary; I was surprised to hear that you intended to hang it up in any other place than the observatory at St. Petersbourg, and am of opinion that it may be somewhat hazardous to attempt it, and perhaps you likewise will think so when you have seen the whole apparatus, and can better judge of the damage that some parts thereof may accidentally suffer by such an undertaking. It were to be wished therefore that so curious an instrument might not be exposed to unnecessary dangers, but be carried directly to the observatory which it was originally designed for.

As there are several improvements and additions made to this instrument, which were not in those that you had the opportunity of seeing when you were in England, Mr. Bird intends to send an account after what manner the several parts of the apparatus are to be put together, and will make such remarks as may enable you to distinguish their several connections; it may therefore be proper first carefully to inspect the whole, and attend well to those directions, in order to judge rightly of the use of each part, before you begin to put them together.

Permit me, sir, on this occasion, to conclude with my congratulations on your accession to the honourable station which you so worthily fill, and to assure you of my sincere wishes for the prosperity of the learned and illustrious members of the Imperial Academy of St. Petersbourg, and success to the laudable attempts they are daily making to promote the various arts and sciences that tend to the benefit of mankind.

I am, sir, with great respect and esteem,

Your most obedient humble servant,

[J. BRADLEY.]

[à M. de l'Isle.]

Greenwich, Aug. 22, 1752. o. s.

SIR,

I HAD not the honour of receiving your letter of July 15th n. s. till very lately; it having been delivered to a gentleman of my name that lives with the Sardinian ambassador in London, who without examining the superscription, had opened it before he perceived the mistake, and having occasion to go into the country for some days, left it to be sent to me; but on his return was surprised to find that through his servant's neglect, the letter had been detained from me much longer than it needed to have been. If it had been directed for me at Greenwich, instead of London, the postman could not so easily have made the mistake.

I now send you, sir, according to your desire, such observations as the weather would permit us to make, about the times specified in M. de la Caille's advertisement.

The enclosed is a copy of the journals wherein we enter the observations while we are making them. I have not corrected the apparent zenith distances observed by the quadrant, but you may readily do that, if it be necessary, by subtracting 4″ for the error of the line of collimation, and also 1″ for every 5° 42′ of the arch of the instrument, that having been found to be about 16″ – less than a true quadrant. Thus the correct apparent zenith distance of χ Aquarii, Oct. 4th, 1751, was 60° 31′ 16″.

You will perceive that my clocks are adjusted so as to go sidereal time, and to shew nearly the horary right ascension of the midheaven, that seeming to me the most convenient method when the transits of many different fixed stars are to be observed. The day is always supposed to begin with the sun's passage over the meridian.

The transits of the ☽ and stars, &c. were observed sometimes with a very good transit instrument, (the telescope of which is 8 feet long,) and at other times with the telescope of my large mural quadrant, whose limb is brought to lie so nearly in the plane of the meridian, that when objects do not differ much in declination, the observed times of their passage will give the true difference of right asc. without any sensible error: but the transit instrument is exactly adjusted to the meridian, and gives the true difference of right asc. in all cases. Two clocks were made use of, one which is near the quadrant, and the other in the room wherein the transit instrument is placed. When the time of the passage is observed by the transit instrument, 'tis denoted by the letter T, and when by the quadrant telescope, 'tis denoted by Q.

I hope, sir, that you will meet with few material mistakes in these observations; I have not examined that point myself, but if any should occur to you, I beg you will be pleased to favour me with your remarks; as also with the result of your comparison of them with M. de la Caille's; a copy of whose observations I shall be likewise glad to see, as soon as you have leisure and opportunity to transmit them to me; because we may perhaps have made others which will tally with some of his that have been made upon other days, besides those that he had fixed on in his advertisement.

I am, with great respect, sir,
Your most obedient humble servant.
[J. BRADLEY.]

⁂ This is the original from which the French translation was made that is inserted in the Mem. de l'Ac. Roy. des Sciences 1752. p. 424. One little oversight occurs, which, although not of much importance, may be worth correcting. The direction is simply given to subtract "une seconde pour chaque arc de 5° 42′ parce que cet instrument s'est "trouvé de 16 secondes plus petit que 90 degrés." It escaped the notice of the translator, (and it may have done so very easily,) that by "16″—less than a quadrant," Bradley meant to describe the arc as "less than a quadrant" by a quantity which was something smaller than 16″; 15″,8 would exactly answer to 5° 42′ for 1″, and it appears from the old quadrant books, that between the 18th and 28th of September, 1745, he "made several experiments in order to discover the true quantity of the arc of the outer "divisions of the mural quadrant, as it hung on a wall, by means of a curious level and "apparatus contrived by Mr. George Graham for that purpose." The trials were made in the manner which is explained in Bradley's letter to le Monnier, (p. 474): he repeated them "twenty-eight times, and found the greatest difference to be 17″,8 and the least "13″4 and the mean of all 15″¾," by which the arc of the quadrant was less than 90°.

The observations sent to de l'Isle are printed in the Mem. de l'Ac. p. 426—434, and are all (with the exception of the times by the quadrant clock) in the first vol. of the Greenwich observations.

[à M. le Docteur Bradley.]

à Paris, le 30 Novembre 1752.

MONSIEUR,

J'AY reçu la lettre que vous m'avez fait l'honneur de m'écrire le 22 Aoust de cette année v. st. en m'envoiant les observations que vous avez faites à Greenwich, correspondantes à celles de M. de la Caille au Cap de Bonne

Espérance pour la parallaxe de la lune, de Vénus, et de Mars. Je vous envoie, comme vous l'avez souhaité, la comparaison que j'en ai faite avec celles de M. de la Caille. Je n'ay encore comparé que celles de Mars, et quoy qu'il s'en trouve un assez grande nombre faites de part et d'autre, il ne s'en est cependant rencontré que six qui ont été véritablement correspondantes, c'est à dire faites à la mesme étoile et dans la mesme nuit, n'y aiant eu entr'elles que l'intervalle du tems, pour passer du méridien du Cap à celui de Greenwich. Par plusieurs observations de M. de la Caille comparées avec celles d'Europe, l'on est assuré que le Cap de Bonne Espérance est oriental à Greenwich de 1h. 14′ et c'est la différence de longitude que j'ay supposée pour réduire vos observations à celles du Cap.

Comme vous n'avez pas rapporté vos premières observations au bord supérieur ou boréal de Mars, ainsi que M. de la Caille avoit averti qu'il le feroit de son côté, j'ay été obligé de supposer le diamètre apparent de Mars connû ; et dans l'usage de vos premières observations je l'ai employé tel, ou un peu plus petit, que vous l'avez trouvé, lorsque vous avez commencé à l'observer.

La première observation que vous avez faite, correspondante à celle de M. de la Caille, est du 31 Aoust 1751, au matin, auquel jour vous avez trouvé que le centre de Mars dans le méridien étoit de 11′ 21″ austral à l'étoile 33 des Poissons. Si l'on en ôte 13″ pour le demi-diamètre apparent de Mars dans ce tems-là, il en résultera 11′ 8″ pour la différence de déclinaison du bord septentrional de Mars et de l'étoile au méridien de Greenwich ; la variation diurne de Mars en déclinaison étoit dans ce tems-là de 4′ 47″ ; d'où l'on la conclud pendant 1h. 14′ de 14″ 48‴ à soustraire de la distance observée à Greenwich pour la réduire à ce qu'elle auroit été au méridien du Cap ; ainsi par votre observation le bord septentrional de Mars auroit été austral à l'étoile de 10′ 53″ 12‴. M. de la Caille a trouvé cette étoile septentrionale au bord boréal de Mars de 10′ 18″ 24‴ : la différence est donc 34″ 48‴ pour la somme des parallaxes de hauteur de Mars ce jour-là au méridien de Greenwich et du Cap.

Voici ce qui j'ay trouvé pour les autres jours.

	′	″	‴
Sept. 14, matin. à Greenwich Rigel méridional au centre de Mars …………	7	50	30
demi-diamètre de ♂ …………………		13	
Rigel mérid. au bord sept. de Mars à Greenwich …………………………	8	3	30
Variat. diurn. en décl. 4′ 8″ répond pour 1h. 14′…………………………		12	47
Rigel méridional au bord sept. de ♂ par l'observ. de Greenwich réduite au Cap	8	16	17
La mesme distance observée au Cap …………………………………	8	51	30
Diff. ou la somme des parall. de haut. de ♂ …………………………	0	35	13

	′	″	‴
Oct. 3, soir. à Greenwich le bord sept. de ♂ étoit austral à l'ét. λ. du Verseau de	4	42	30
Variat. diurn. de ♂ en déclin. 0′ 59″ répond pour 1h. 14′		3	2
Le bord sept. de ♂ aust. à l'ét. réd. au mérid. du Cap	4	45	32
observée au Cap	4	9	54
Somme des parallaxes de hauteur de ♂	0	35	38
Oct. 4, soir. à Greenwich l'étoile λ étoit boréale au bord sept. de Mars	3	26	0
Variat. diurn. en décl. 1′ 18″ répond pour 1h. 14′		4	1
L'étoile bor. au b. sept. de ♂ red. au mérid. du Cap	3	30	1
observée au Cap	2	58	12
Somme des parallaxes de hauteur de ♂	0	31	49
Oct. 7, soir. à Greenwich l'et. λ austr. au bord sept. de ♂	2	17	0
Variat. journ. déclin. 2′ 10″ répond pour 1h. 14′	0	6	42
L'étoile austral. au b. sept. de ♂ réd. au Cap	2	10	18
observée au Cap	2	36	36
Somme des parallaxes de hauteur de Mars	0	26	18
Oct. 9, soir. à Greenwich l'ét. λ. aust. au b. de ♂	7	35	0
Var. journ. de déclin. 2′ 49″ répond à 1h. 14′		8	42
L'et. λ. aust. au b. bor. de ♂ réd. au Cap	7	26	18
observée au Cap	7	57	24
Somme des parallaxes de hauteur de ♂		31	6

Cette somme des parallaxes, que je viens de trouver pour chaque observation, est l'angle à Mars, formé par les deux raions visuels menez des deux observateurs à un mesme point de Mars; j'ay ensuite déduit de chacune de ces six observations, la parallaxe horizontale de Mars, en la prenant dans le mesme rapport avec le sinus total, que cet angle à Mars, est à la somme des sinus des distances apparentes de Mars au zénith de chaque observateur, et j'ay trouvé la parallaxe horizontale de Mars, comme vous voyez ici.

	″	‴		″	‴	
1751, Aoust 31.	26	42	Parallaxe horizontale de Mars pour chaque observation.	0	27	Corrections additives pour réduire les parallaxes à ce qu'elles auroient dû être dans l' ☍ de Mars au soleil.
Sept. 14.	27	10		0	1	
Octob. 3.	27	35		2	34	
4.	24	34		2	46	
7.	20	20		3	20	
9.	27	35		3	46	

Comme la parallaxe horizontale de Mars varie suivant la distance de Mars à la terre, j'ay calculé par les Tables de M. Halley les distances réelles de Mars à la terre pour le tems de vos six observations rapportées cy-dessus, et pour le tems de l'opposition de Mars au soleil; me servant ensuite du rapport de ces distances j'ay cherché de combien la parallaxe horizontale de Mars a dû être plus petite dans chacune de vos observations que dans le tems de l'opposition, ce sont ces quantitez qui composent la petite table que j'ay rapportée cy-dessus sous le titre de corrections, ces quantitez étant ajoutées à la parallaxe déduite simplement de chaque observation, ont donné la parallaxe horizontale telle qu'elle auroit dû être pour chaque observation réduite au tems de l'opposition. Les calculs étant faits, voici ce que j'ai trouvé pour la parallaxe horizontale de ♂ au tems de l'opposition:

			″	‴	
par l'observation du	31	Aoust	27	9	
	14	Sept.	27	11	
	3	Octobre	30	9	*
	4	———	27	20	
	7	———	23	40	*
	9	———	27	39	

En prenant un milieu, ou moien arithmétique entre ces six déterminations l'on conclud la parallaxe horizontale de Mars dans le tems de l'opposition de 27″ 11‴; mais comme il y a deux déterminations qui s'éloignent des autres d'environ 3″, qui sont celles du 3 et 7 Octobre, l'on pourroit les rejeter, et alors l'on trouveroit le milieu entre les quatre autres de 27″ 20‴, d'où vous voyez, Monsieur, que, soit que l'on rejette ces deux déterminations, ou que l'on les emploie, la parallaxe horizontale de Mars dans le tems de l'opposition en résulte tout prez de 27″$\frac{1}{4}$, et suivant le rapport de la distance du soleil et de Mars à la terre dans ce tems-là, l'on en conclud la parallaxe horizontale du soleil de 10″$\frac{1}{3}$ environ.

Voilà ce que j'ai pu conclure jusqu'ici de vos observations de Mars pour la parallaxe du soleil, aiant fait les mesmes calculs sur mes observations et celles des autres astronomes que j'ai pû recuellir jusqu'ici, j'ay trouvé à peu prez la mesme parallaxe du soleil, en prenant un milieu entre toutes les observations de chaque astronome; mais je n'ay pas toujours trouvé que les différentes observations des autres astronomes s'accordassent aussi bien entr'elles que les vôtres; c'est pourquoi j'ay été un peu plus incertain pour en conclure la véritable parallaxe du soleil, et je n'espere de la pouvoir déterminer plus précisément, que celle que j'ay déduite de vos observations, qu'aprez avoir vérifié les observations de tous les astronomes les unes par les autres, et rejeté celles qui seront manifestement défectueuses, aprez l'examen rigoureux que je me propose d'en faire.

Je n'ay pas encore comparé vos autres observations avec celles de M. de la Caille, c'est ce que je me propose de faire le plutôt que j'en aurai le loisir. Je vous envoie en attendant les siennes que l'Académie a fait imprimer pour donner aux autres astronomes la satisfaction de les pouvoir comparer avec les leurs propres.*

Je ne doute pas que vous n'aiez déjà pensé, Monsieur, au passage de Mercure sur le soleil, que nous attendons de 6 May de l'année prochaine; j'en ai fait le calcul sur les Tables de M. Halley, rectifiées non seulement aprèz les corrections qu'il y a fait lui-mesme ensuite du passage de 1723; mais encore sur le passage observé en 1740 à Cambrige dans la Nouvelle Angleterre; ensorte que j'espere être sur à peu de minutes prèz de l'entrée et de la sortie de Mercure du disque du soleil.

J'ay calculé que l'entrée se devoit faire à 2h. 44′ du matin à Paris, et la sortie à 10h. 37′, et par conséquent la durée de ce passage de 7h. 53′; et qu'outre cela Mercure devoit passer assèz prèz du centre du soleil; ce qui suffit pour avertir les astronomes du tems auquel ils doivent attendre ce passage; au lieu que ceux qui se serviront des Tables de M. Cassini, sur lesquelles sont calculées nos éphémérides et celles d'Italie, se tromperoient de quatre heures entières, dont le passage doit arriver plutôt par ces tables que par celles de M. Halley. Cette différence m'a parü assez grande pour mériter d'en informer les astronomes; et c'est ce que je vais faire par un avertissement incessament publié.

En attendant j'ay cru devoir en avertir aux Indes Orientales pour tâcher d'avoir des observations exactes de ce passage dans les endroits où l'on pourra en voir toute la durée. La fin se pourra observer dans une partie de l'Amérique Septentrionale peu aprèz le lever du soleil, et l'observation exacte que l'on en pourroit faire pouvant servir à déterminer la parallaxe du soleil, si ces observations sont comparées à celles qui se feront dans les Indes Orientales, c'est ce qui m'oblige de vous en écrire pour vous prier de recommander à tous ceux qui seroient en état de bien faire cette observation dans vos colonies Angloises, de vouloir bien s'y prêter, et d'y être sollicitez par la Société Royale.

J'ay trouvé qu'à Cambrige prez de Boston (ou M. Wintrop a déjà observé le passage de 1740) Mercure devoit sortir du soleil à 5h. 45′ du matin; ce qui est 51 min. aprèz le lever du soleil. Cette mesme sortie doit arriver à la Nouvelle York à 5h. 32′, ce qui est 34 min. aprèz le lever du soleil : dans les autres lieux du voisinage cette sortie se fera à proportion plus ou moins prèz du lever du soleil, comme il sera aisé d'en faire le calcul. Mais il seroit à souhaiter que l'observation s'en fît principalement dans des lieux dont on connût d'ailleurs la longitude le plus précisément qu'il seroit possible par d'autres observations ; ce qui serviroit à mieux déterminer la parallaxe du soleil, par la comparaison de ces observations avec celles qui seroient faites dans les Indes Orientales. L'on a déjà beaucoup d'observations pour la longitude à Cambrige, et à la Nouvelle York, sans les nouvelles que s'en y pourroient encore faire ; ainsi ces deux lieux seroient les plus propres pour y observer le prochain passage de Mercure sur le soleil, que je vous prie de vouloir bien recommander à la Société Royale, afin qu'elle s'intéresse de tout son crédit à nous procurer ces observations,

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* The beginning of this letter, as far as the end of this sentence, was published in the Mem. de l'Ac. Roy. des Sciences, 1752. p. 434—439; and a translation of the whole is printed in the Phil. Trans. vol. XLVIII. p. 512.

Je suis avec une trèz-parfaite estime, Monsieur,
Votre trèz-humble et trèz-obéissant serviteur,
De L'ISLE.

I received this, Jan. 13, 1753.

[To the Rev. Dr. Bradley.]

Jan. 5, 1753.

SIR,

* * * * *

I am very sorry to hear you have as yet met with so little satisfaction in your inquiry into the laws of refraction. I hope you will not find insuperable difficulties, though I must own I have some fears concerning it, especially any thing near the horizon, where the vapours from the earth must mix very variably and uncertainly with the atmosphere.

I am, sir, your most obliged humble servant,
J. HOWE.

[To the Rev. Dr. Bradley.]

Geneva, June 2, 1753.

SIR,

THE favourable opinion you have been pleased to express of the proposal which I had taken the liberty to lay before you, emboldens me to address

you once more for your sentiments concerning some other thoughts in optics that have occured to me since I did myself the honour of writing you; part of which have some relation to your own discovery of the aberration of the light of the fixed stars.

I do not find that it has been determined hitherto, or even so much as inquired into, whether the original velocity with which rays of a given colour are emitted, be the same in whatever medium the luminous body be placed. And yet, I think, we have sufficient data from the received doctrine of refraction, and one single experiment for deciding the question in the negative, viz. that it is emitted with greater velocity in denser mediums, and that in exact proportion to their refractive powers. Rays of any colour emitted by red-hot iron in the open air, or under an exhausted receiver, are found to be equally refracted by the prism at the same obliquity placed in the air; hence I infer that they have the same velocity at the time of their incidence on the prism, i. e. in air; now it is known by the theory of refraction, that the velocity of light in the exhausted receiver is less than its velocity after passing into the air in the proportion of their refractive powers. In that same proportion, therefore, are the velocities with which light of a given colour is emitted by the same body in the void and in the air. Rays from the sun, and a candle of the same colour, are likewise equally refracted by the same surface of glass or water; from whence I gather by the like reasoning, that the velocity with which the solar ray is emitted, is to that with which the candle emits its ray, as the density of the solar atmosphere contiguous to his surface is to the density of our lower air; exactly as the sine of refraction would be to the sine of incidence, if a ray pass immediately from the solar atmosphere into the air. Thus may we reason (I think certainly) from the common theory of refraction, which supposes the velocity of light in different mediums to be different according to the degree of their attractive force; but though the principles of this theory are drawn with great probability from the phenomena, several difficulties and objections that still remain have made me often wish, that as its velocity in the celestial spaces has been ascertained directly by the eclipses of Jupiter's satellites, some method could be found of determining likewise by experiment its velocity in some denser medium. This appears at first sight impossible, since the time of its passage through the greatest quantity of air, water, or other transparent bodies that can be any where found, is many thousand times less than sensible. But I am much mistaken if your observations of the aberration of light will not furnish us the very method sought after. For upon considering that affair with all the attention I am capable of, it appears to me, that it is not the proportion of the velocity of light in the celestial space, or in the air, but in the humours of the eye to that of the earth which is determined by your observations. In the same manner as you have shewn that a telescope is not directed to a star when it receives the rays along its axis, but obliquely according to the proportion which the velocity of light in the air bears to the earth's transverse velocity, it appears that the axis of the eye, when we look at the star directly, (i. e. when the rays which enter the centre of the cornea pass along that axis to the middle of the retina,) is directed to a certain distance from the star's true place, according to the proportion which the velocity of light in the humours of the eye bears to the velocity of the earth. Therefore, if the velocity of light in the eye be to its velocity in the heavens in a somewhat greater proportion than as 4 to 3, as follows from the theory of refraction, the aberration of the light of the fixed stars ought to be less than that which corresponds to the equation of the eclipses of Jupiter's satellites, in the same proportion. Consequently, if it be found by repeated observations that the former is not less than the latter, sir Isaac Newton's whole doctrine of refraction by an accelerating or retarding power must be given up, since it will then be a certain fact, that the velocity of light after entering a denser medium is not greater than its velocity before incidence. As therefore the decision of so curious and important a question in physical optics seems to depend upon the comparison of the observations, ought not astronomers to apply with new industry to fix precisely the quantity of the equation above-mentioned? I suppose it is impossible to determine so minute an affair as the aberration with greater accuracy than you have already done.

That trifling contrivance for illustrating the physical causes of the celestial motions, of which I sent you the description, has led me since to a new method of adapting the pendulum to clocks, which I flatter myself may not be entirely useless to the public. In seeking after the means of perpetuating the circular motion of the revolving bodies, I found that clocks may be regulated by the conical motion of a pendulum as well as the vibratory, and that with several advantageous circumstances, such as that the motions of the indices become uniform like the current of time; that hence we may distinguish to a 6th or 8th of a second; that the motions of the whole machine become more free and easy, and that there are no leaps or startings from tooth to tooth as in the ordinary clock, because the motion of the pendulum consents perfectly with that of the wheel work, and is animated by it, not by alternate strokes, but a gentle uninterrupted pressure; and, lastly, that the whole motion is carried on almost in silence. These advantages I foresaw clearly from the theory of the thing; yet I should not have ventured to affirm so much about it, had I not found by a little machine I have caused to be made in this place, (which goes well for five or six hours,) that the scheme is certainly practicable, and will not fail to answer expectation.

If you think a short description of this new pendulum, and the manner of adapting it to clocks, will be acceptable to the Royal Society, I shall send it soon to a friend in London who is of that body.

I was so justly diffident of the contents of my form[er] letter, that as soon as I thought of submitting the [above] to your examination, I resolved to lurk under a [bor]rowed name; but after my being so lucky as to [light] on what seemed worthy of the attention of Dr. Bra[dley,] I am bold enough to subscribe myself without [dis]guise, though still equally unknown to you,

Sir,

Your most obedient and obliged humble servant,

T. MELVILL.

[Ple]ase direct for me
[au]x trois Rois Genève.

Two considerations that may help, I believe, to recommend to the curious the new clock I propose describing are, 1°, That it exhibits a specimen of uniform uninterrupted motion, which, I believe, is no where else to be found in terrestrial bodies; 2^{do}, That the uniform motion of the pendulum is preserved by the very same principles as that of a planet revolving circularly; viz. by the equilibrium of its gravity and centrifugal force. The whole machinery serving only to remove the three circumstances which usually prevent the perpetuity of terrestrial motions; namely, the resistance of the medium, and friction of surfaces, and their gravity towards a remote centre. The first two are remedied by the continual pression of the wheel work animated by a constant weight, and the last by the suspension of the pendulum on a fixed point.

I cannot but observe here, that if my reasoning be just (which I humbly submit to your examination) and the ordinary theory of refraction true, the aberration of the fixed stars may be different to different observers; viz. if

the density of their ocular humours be sensibly different, which may, for any thing I know, be sometimes the case.

[To the Rev. Dr. Bradley.]

Parisiis, Idibus Decembris 1754.

SI quam laudem a nostratibus Gallis meritum est opus quod ineunte anno publici juris feceram, vir clarissime, eam totam Anglis me debere libenter profiteor. Libri titulus erat, Etat du Ciel calculé sur les tables de Mr. Halley, &c. pour l'an de grace 1754. Viam qua procedendum esset in hisce calculis præmonstraverat altissimi vir ingenii Newtonus. Celeberrimi Flamstedius, Stretius, et Halleius ex innumeris propemodum observationibus Newtoni principia redegerant in tabulas. Desiderabantur tamen in Galliis Ephemerides ex accuratissimis illis tabulis descriptæ. Serii laboris impatientes deterrebat scilicet quæ nimia videbatur calculorum prolixitas. Iis ineundis me non sum arbitratus imparem. Sed neque spem meam exitus fefellit. Mihi plene satisfactum fuerat, si operis unum et alterum exemplar ad te, vir doctissime, prout in votis erat, dirigere licuisset. Verum incuria typographi, hyemis inclementia, similibusque de causis factum est, ut ante veris medium hujusce mei voti compos esse non potuerim. In annum igitur alterum differendum esse duxi. Neque me dilationis ejus pœnitet. Prodit iterum opus suis numeris longe magis absolutum. Nihil in calculis, nisi summa religione limatum. Ad minuta secunda scrupulus eorum exigitur. Nullos omisi, unde currente anno 1755, possit vera lunæ sedes detegi facilius, unde locorum in longitudine discrepantia possit erui certius. Verum totius operis institutum ex ipso opere colliges accuratius, vir clarissime. Doctissimus D. du Hamel e regia scientiarum academia, simul et e vestra regia societate, eam in se provinciam suscipere dignatus est, ut ad te duo exemplaria libri transmitteret. Unam a te gratiam ausim deposcere, vir meritissime. Mihi pergratum feceris, si clarissimum virum D.D. Macclesfield, regiæ societatis præsidem induxeris, ut istorum exemplarium alterum dignetur ab humillimo servulo suo suscipere. Ea porro dirigenda esse mihi significavit D. du Hamel ad clarissimum virum (ni mea me fallit memoria) D. Miller e regia vestra societate, una cum seminibus quæ ad prædictum virum transmittit qui regium hic hortum colit et administrat. Ceterum quod ita sollicitus fuerim quî possent ad te laboris mei fructus pertingere, ne mireris, vir

eruditissime. Novi opera tua tuamque scientiam. Et quis in toto quantus est orbe litterario Bradleium non audivit? Desiit Flamstedios et Halleios Europa desiderare, ex quo te regio Grenovicensi observatorio præpositum cognovit. Te suum vestra regia societas lumen fulgentissimum, te meritissimum et præstantissimum academia nostra socium, te ætatis hujus astronomorum facile principem Urania tota suspicit et veneratur. Quid mirum itaque, si mihi subierit in mentem ei me meumque laborem commendare, vel potius ejus humiliter exposcere consilia, qui possit aut operis mei si quod est meritum æquius dijudicare, aut quid ejus perfectioni conducere possit aperire benignius? Tu scilicet attonito mundo luminis ipsius demonstrasti viam. Facies utique ut hujusce tuæ lucis radius aliquis ad me perveniat. Vale, vir omni laudi meæ superior, meque cum omni obsequio et veneratione tibi credas addictissimum.

Pingré, Sanctæ Genovefæ in Monte
Canonicus et Bibliothecarius.

[To Thomas Barker, Esq.]

[1755.]

I HAD the favour of your letter of December* 17th past, relating to the comet, whose return is expected three or four years hence, and I doubt not but the easy method you have pointed out for readily finding whereabouts it may appear at any given time of the year, will induce many persons to look out in hopes of finding it as soon as it becomes visible; by which means it will probably be discovered much sooner than if we were not apprised of the proper parts in the heavens where it may appear.

The inconvenience arising from the shortness of Dr. Halley's general table of the Parabola, is what every one must sometimes find that makes much use of it; and I am glad to hear that you have so effectually removed it, by constructing one that is so much longer, and which, in other respects likewise, as you observe, may be more convenient for practice. Such general tables being wholly designed for the computations in which they are to be used, the fuller they are the more effectually they answer the purpose for which they are framed; though therefore the difference of 5′ in the angle from the perihelion may seem less than is needful in some parts; yet a little increase of the length of the table need not, I think, be an objection to the making those differences run uniform throughout the whole Table, from the beginning to the end of it. Though the method you make use of to construct your Table may perhaps be looked upon as indirect; yet the facility and accuracy with which the numbers sought can be determined from the data, may give it a preference with any one that should attempt to construct the Table with an equal degree of exactness in the more direct way, as it may be thought, of finding the angle from the given mean motion. Dr. Halley has not told us how he extracted the cubic root of his general equation for that purpose, and I have not seen any way of doing it that would not be more tedious than the method you make use of; but probably there may be a shorter way of doing it than what I have sometimes practised; [it is as follows: Let $m=$ twice the given mean motion, (supposing, as you do, the mean motion corresponding to 90° from the perihelion to be expressed by unity,) then find $A=\sqrt{1+m^2}$ and take $A+m=B$ and $A-m=C$ and the log. $B-$log. $C=D$;] viz. of finding two mean proportionals between $\sqrt{1+n^2}+n$ and $\sqrt{1+n^2}-n$ where n denotes twice the area or mean motion from the perihelion, (the area when the angle from the perihelion is 90°, being, as you supposed it, $=1$,) the difference between which mean proportionals is the tangent of $\frac{1}{2}$ angle from the perihelion, if the radius is 1.

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* See Phil. Trans. vol. XLIX. p. 347.

As it cannot be any great addition to the trouble you have already had in computing your Table, to prepare a copy of it for the press, I hope you will favour the public with it; as every means of facilitating astronomical computations must contribute, in the end, to the improvement of science.

With this view I have taken the liberty you gave me of communicating your letter to my lord Macclesfield, in order to be laid before the Royal Society; who, I am persuaded, will be pleased with any further remarks upon this subject, from a correspondent capable of imparting to them what may so well deserve their attention. And, as it may be proper, before the time when the comet is expected, and thereby assist the practical astronomer, whose labours are now multiplied in such a manner, that he needs all the assistance that can be given him towards the facilitating his numerous calculations.

[J. BRADLEY.]

Viro insignis doctrinæ et pervagatæ orbem gloriæ Jacobo Bradley, astronomo regio, Londinensis Societatis decori; Jo. Matthias Gesnerus, Academiæ Georgiæ Augustæ S. T. Prof. et Societ. Gotting. Director S.

Gottingæ, prid. kal. Nov. 1755.

NON communi modo amore, quo devincti inter se esse debent optimarum artium studiosi, atque admiratione doctrinæ tuæ prope singularis impulsa universitas Georgia Augusta, sed tuis etiam in se excitata meritis, dulce mihi ad te scribendi mandatum dedit tibique significandi, tuum nomen sibi non modo tanquam viri doctissimi ac præclare de astronomica præsertim eruditione et naturali doctrina meriti carum esse, sed ob illam etiam officiosam efficacemque humanitatem amabile, quum nulli operæ tuæ pepercisti, ut nancisceretur specula nostra academica præclarum instrumentum locis et motibus cœlestibus exacte observandis oppido idoneum. Scribetur præclarum nomen tuum inter amicos optime de nobis meritos, et hujus ipsius epistolæ exemplum in tabulario nostro repositum, et tui beneficii et grati animi nostri memoriam servabit. Bene vale, et gloriæ tuæ optimis artibus partæ diu lætus interes.

[To the Rev. Dr. Bradley.]

Rome, Dec. 3, 1755.

REVEREND SIR,

I THOUGHT so much was proper to be said by way of preface, in case you judged the papers* worth presenting to the Royal Society, of which I have the honour to be a member; but if you judge otherwise, you are master to suppress them, as also to reform whatever you meet with therein that is erroneous. I shall further observe in this note, which is private to yourself, that I have chosen to write these papers in Latin rather than English, as theories of this nature have generally been delivered in that language. As I should be glad to know whether they be worth any attention or not, I must desire of you the favour to let me know what is your opinion of them. The liberty I have taken I hope will not be offensive, though coming from a person who is a stranger to you. I have lived for some years at Paris, though native of England, and left that city about a year ago to pass some time at Rome. As it is a little uncertain where I shall be at the time

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* See Phil. Trans. vol. XLIX. p. 700. The introductory letter, by a press error, is there dated 1756, instead of 1755.

you write, you'll please to direct your letter as follows: à M. Walmesley vis à vis la fontaine des Carmelites ruë St. Jacques, à Paris, and it will be immediately forwarded to me. I am glad to have this occasion of paying due respect to your distinguished merit, and am, with the highest esteem, reverend sir,

Your most obedient humble servant,

C. WALMESLEY.

[To the Rev. Dr. Bradley.]

Lyndon near Uppingham, Rutland,
Dec. 8, 1755.

REV. SIR,

I WAS last winter much obliged to you for so friendly answering me, a stranger, about my construction of the 1682 comet and example of my parabolick table, which you, seeming not to dislike, proposed my publishing. To save others the pains I took, I had indeed some thoughts of it, but thought it imperfect without some directions and examples of its use. I thought, therefore, to begin with sir Isaac Newton's method to find a comet's orbit from three observations, a transcript of which is on the other side; but my uncle, George Whiston, of whom I first learned the method, not having then seen how others reduced that problem to triangles, might not always hit on the nearest way. I have on reexamination altered and abridged several in his list of triangles; and being desirous to make it as correct as may be, thought it necessary to lay it before so skilful a person for your correction and hints of any thing not well explained, or what would be proper further to be added, and shall think it an honour to have your assistance, as well as your due publicly to own it. In adding a list of all the known comets' orbits, I thought to insert the sine and cosine of the inclination, because constantly used, and instead of perih. a nodo, to put perih. post nodum, as I proposed in my former letter; but as several of the later comets have been calculated by more than one person, I hardly know which of their orbits to insert, especially as I know not where to find the observations of the comets of 1706, 7, 29, 39, and some since. Having next given my parabolic table and method of computing it, I thought to conclude with examples of its several uses and the way to calculate a comet's place, with the improvements I last year proposed.

* * * * *

A sentence is here omitted, because it refers to a long illustration of his method, which Barker annexed to the end of the letter. This it was not necessary to print, because the substance of it, with additions and corrections, is already published in his book on Comets, p. 6—12.

I beg pardon, sir, for troubling you with this long, intricate letter, but having no friend in this country conversant in this problem, I hoped you would not take my intruding on you ill, and am, sir, with all due respect,

Your obliged and obedient humble servant,

THOS. BARKER.

[To the Rev. Dr. Bradley.]

Rev. Sir, April 17, 1756.

* * * * *

Mr. Mayer sends me word that he has computed all the observations of Jupiter that he could meet with, and when he has computed yours he will send me tables for Jupiter. I shall go on with computing more of your lunar observations, but have taken such liberties with the Tables as make the error of 22d July 1754 only + 0′ 34″; but have yet another error of 1′ 0″ + and one 0′ 59″ −, which are the two greatest that I yet know of. As you mentioned writing to Mr. Bird, I have not called there, but have been told that one of the instruments is almost finished, and that he is not well. Pray give my compliments to all friends, and I am, dear sir,

Your most humble servant,

GAEL MORRIS.

[To the Rev. Dr. Bradley.]

Sir, Rome, June 29, 1756.

LAST December I took the liberty to write to you, and to send you at the same time a couple of papers, one upon the precession of the equinoxes and the nutation of the axis of the earth, the other upon some irregularities in the annual motion of the earth. I was in hopes of receiving a line from you, by which I might know you had received them, and also what might be your opinion of them, but as nothing is yet come to me from your hand, it makes me fear they have been some way or other mislaid; though the person I employed to get them transmitted to you informed me he was pretty sure they got safe to your hands. I propose to stay some time longer at Rome, but if you'll please to favour me with an answer, you may direct it as follows: à M. Walmesley chez M. Walker vis à vis la fontaine des Carmelites ruë S^{t}. Jacques, à Paris; which by this means will securely come to me, and will greatly oblige, hon^{d}. sir,

Your very obedient humble servant,

C. WALMESLEY.

[To the Rev. Dr. Bradley.]

Essex, Spithead, March 30, 1757.

Dear Sir,

HAVING been ordered by the lords of the Admiralty to make trial of your new instrument at sea, I took several observations therewith in sight of Cape Finistère, which I have sent to the Admiralty in order to their being laid before you, and hope they will serve to shew what may be expected from observations made at sea, though they were not taken with all the advantages that might have attended them, had I been alone; for I was all the cruise in company with an admiral whose motions I was obliged to follow, which prevented me from laying the ship in the most advantageous position, or of continuing in the same place during a series of observations; to which may be added, my want of practice in observing with such a long telescope, &c. &c.

I herewith send you two of the observations worked as well as I could by the precepts Mr. Morris gave me, having forgot how to make an allowance you told me would be necessary when the moon had a great latitude. And as the errors are both the same way, and their difference not a third part of the least error, I hope some part of the error is owing to the want of that correction, for I cannot think the supposed longitude far wide of the truth. Indeed I was in hopes of settling the longitude of Cape Finistère to a tolerable degree of exactness by the moon's passage over Aldebaran on the twenty-fifth of February, being then in sight of the Cape, and having taken several altitudes of the sun in the afternoon, to find the watch's error; but, to my great mortification, the night proved so cloudy that neither moon or stars appeared. During all the observations, the new instrument remained adjusted as Mr. Bird sent it down, viz. the telescope index at 360, and the other at 315 degrees; and when you get the observations, you'll see the numbers given by the instrument put down with red ink in the column of altitudes, as a check on what is put down in the column of distances; and as I believe there is a difference in the dark glasses, I have marked which glass was used in each observation. There is a column marked Inst. in which N. stands for new instrument, B. and J. for Bird's and Jackson's quadrants; the height of the eye when the altitudes were taken was about twenty-three feet.

If I can obtain leave to come to town, I shall do myself the pleasure of paying you a visit, having many things to consult you about; but in case I cannot come up, if you will be pleased to let me know of any thing that I have omitted, either in taking or working the observations, you will confer an additional obligation on, dear sir,

Your most obliged and most obedient servant,

JOHN CAMPBELL.

[To the Rev. Dr. Bradley.]

SIR, Admiralty Office, 1st April, 1757.

MY lords commissioners of the Admiralty having directed captain Campbell, of his majesty's ship the Essex, to make trial of an instrument contrived by you for taking observations on shipboard for finding the longitude at sea, I am commanded to send you the observations he has made, and to signify their lordships' desire that you will take the trouble of examining and reporting to them your opinion thereupon, with which you will please to return the observations to, sir,

Your most humble servant,

J. CLEVELAND.

[Mem. of Bradley's]
Returned when I made a report;
viz, 14 April, 1760.

[To the Rev. Dr. Bradley.]

Sir, Admiralty Office, 27 April, 1757.

HAVING laid before my lords commissioners of the Admiralty your letter of the 22d instant, proposing to employ Mr. Bird in making another instrument for observing on shipboard the distances of the moon from the sun or stars, and that you apprehend the making of some small additions to and alterations in the form of the common Hadley's quadrant may render it very useful for that purpose, I am to desire you to employ Mr. Bird in making another instrument accordingly.

I am, sir,

Your very humble servant,

J. MILNES, D. S.

[Probably to M. de la Caille.]

Sir,

I HAD the pleasure of receiving your kind present of a work that must prove no less honourable to its author, than beneficial to astronomy. Such a series of exact observations justly entitle you to the united thanks of all the lovers of that science, and though, as one of them, I sincerely tender mine; yet I beg leave to add also my particular acknowledgments for the honour you have done me on many occasions.

A severe illness, which afflicted my family at the time I received your first letter, prevented me so long from attempting to send (as you desired) the observations of stars proper for determining the refractions, that I judged it would be too late for your purpose; you could not have received them before your work was printed off; but since I perceive you are still inclined to see them, as soon as my absence from hence, and other avocations would permit, I extracted from my journals the enclosed observations of some stars south of the equator, as you desired: (I have not had leisure to examine how they [agreed] with each other, or to deduce the refractions resulting from the observed z. distances compared with the declinations as settled by yourself; but) I apprehend that you will in general find the refractions here come out less than your Table gives for Paris. For I have found by my new mural quadrant with which these observations were made, that the z. distance of the pole here, as affected by the mean refraction, is 38° 30′ 35″, and the z. distance of the equator 51° 27′ 28″, which gives the sum of my polar and equatorial refractions only 1′ 57″, whereas your Table makes that sum 18″ more.

My observations of the circumpolar stars, and also of the sun near the solstices, (when they are corrected by the Table of refractions, which I had framed from observations of several stars above and below the pole,) give the latitude of this observatory the same that I should collect by assuming your latitude, and applying thereto the difference which the observations of stars near our zeniths give. Your observations of β and γ Draconis, compared with mine, give the difference between the parallels of our observatories 2° 37′ 10″ or 11″, which would make my latitude 51° 28′ 39″ or 40″, (that is, 9″ or 10″ greater than Mr. Flamstead supposed it to be,) but just the same as I had otherways collected it to be.

As we both proceed upon the same principle in order to discover the sum of the refractions, (at the height of our pole and equator,) I should suspect the difference in the result to arise from some defect in our instruments, rather than from a real difference in the quantity of the refraction, since the difference of one second in five or six degrees between the arcs of instruments may be more easily admitted, than that the refractions at Paris should be an eighth part greater than at Greenwich.

But although our refractions differ so much, yet, when they are applied to the observations made by our respective instruments, the result will be nearly the same; so that our declinations of the stars do in general agree very well; and your obliquity of the ecliptic exceeds by one second only, what I had deduced from my observations.

But notwithstanding this, it were to be wished that the true source of this apparent difference might be discovered; I first imagined that it might have been accounted for by a bending in the parts of your instrument, which it may be subject to from the manner of its suspension; but as the telescope, in that case, would be carried along with the centre, it did not appear that any error would arise from thence; so that if the whole arc of the instrument be of its due length, the difficulty would not be removed by any supposition of such yielding in the radii of it. If I had not already examined my new mural quadrant, I might have suspected that the arc thereof was too short, as I had formerly found that of my old instrument was, with which the observations were made which were sent to M. de Lisle; but from trial of this, as it hung on both sides the wall, I am fully satisfied that the difference does not proceed from thence.

⁂ As there is no address to this letter, it may be proper to state the reasons for supposing that it was written to Lacaille. He seems to be the person, who, in examining the question of refraction, would be most likely to wish for observations of stars south of the equator. In p. 214, of his Fundamenta Astronomiæ, (4to. Paris, 1757,) he gives a Table, in which the polar and equatorial refraction for the latitude of Paris make up 2′ 14″,2, which are nearly 18″ more than Bradley says that he found for the sum of these quantities at Greenwich; what is said in the beginning of the letter will also answer very well to this same work, and the editor happens to have a copy of it, on the fly leaf of which is inscribed, "à Monsieur Bradley, astronome de S. M. Britannique " par son très-humble serviteur, Lacaille." See also p. 501.

Astronomo ingeniosissimo ac celeberrimo Jacobo Bradley, soc. Lond. et Acad.
Paris. et Bononiensis socio, Paullus Frisius Acad. Petrop. et Bonon.
socius, et in Pisana universitate philosophiæ professor, S.P.D.

Dabam Pisis 10 kal. Mar. anni 1758.

POST cl. Walmesley in patriam reditum nihil aliud penes amicos, quos in Italia reliquit plurimos, vulgatum est, nisi ipsum Londini de variationibus obliquitatis eclipticæ et de nutatione terrestris axis, quod te inventum supra astronomos ætatis nostræ adeo extulit, plura tecum disseruisse. Ubi nunc temporis commoretur, et quo ad eum dandæ sint litteræ ignotum est, fortasse ob temporum bellorumque difficultatem. Quare cum aliqua haberem, quæ summo geometræ et amico honestissimo communicarem, epistolas ad te dirigendas censui, ut eas sigillo occlusas ad eumdem pervenire faceres. Atque hanc quidem libenti animo occasionem captavi, qua observantiam meam erga te, et præsidem amplissimum, et cl. Short, ac totam societatem regiam aliquo modo significarem. Tu me maximo beneficio cumulares si in arduis hisce problematis sententiam tuam aperire velles, atque insuper indicare utrum in societatem regiam nunc temporis cooptatus sim. Nam cum præsidi optimo testimonia trium sociorum in Italia degentium misissem, atque insuper dissertationem meam de motu diurno terræ, quæ a regia Berolinensi scientiarum atque humaniorum literarum academia præmium retulerat, nullum ad me hactenus responsum delatum est. Vale et te toti litterariæ reipublicæ diutius serva incolumem.

[To the Rev. Dr. Bradley.]

Bath, Oct. 21, 1758.

REVEREND SIR,

FROM the kind reception you gave me when I waited upon you last year at Greenwich, and the regard you seemed to have for the theory I had sent you sometime before on the precession of the equinoxes, &c. I have been induced to address to you the paper* here enclosed, presuming this liberty will not be offensive, as you are the best judge of all performances relating to astronomy. I have, indeed, been hindered of late by bad health from attending much to these studies; but I hope that what is here offered, though imperfect, will not be entirely unacceptable. If you please to favour me with a line, you may direct to me at the Belltree house in Bath.

I am, reverend sir,
Your very obedient humble servant,
C. WALMESLEY.

[To the Rev. Dr. Walmesley.]

Greenwich, Dec. 1, 1758.

SIR,

SOME time ago I received the enclosed papers, in a letter from the author, desiring me to transmit them to you. But not being able to learn where you then resided, I was unwilling to venture to send them as directed for you at Bath, because I was uncertain whether you were returned from the north, whither you told me you intended to go soon after I had the pleasure of seeing you here. I hope they will now come safe to your hands, as your curious dissertations lately did to mine; which I shall lay before the Royal Society, being highly sensible of the honour you do me by transmitting to the public through my hands such elegant theories on subjects so well deserving the attention of all lovers of astronomical and natural knowledge.

I am, sir,
Your most obedient humble servant,
J. B.

[To the Rev. Dr. Bradley.]

Nov. 9, 1758.

REV. SIR,

BEFORE the bishop of Clogher died, he fell under the censure of some astronomers, for having asserted that the moon kept the same face to the earth, without turning on its own centre. In vindication of himself, he sent me the enclosed letter to print, if I thought proper. As I would not publish any thing now under his name which should be thought a manifest absurdity, and as I am not a proper judge how far what he has here advanced is so, I would humbly beg the favour of you to let me know if you think it barely plausible. I do not presume to ask your decision on the question; but only to say whether what he hath produced hath the appearance of probability, which is the chief object of the press, and in general the utmost attainment of human inquiries. I have further to ask pardon, if my regard to his character hath made me exceed my own, when I subscribe myself, reverend sir,

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* See Phil. Trans. vol. L. p. 809.

Your most humble servant,

W. BOWYER.

⁂ This letter is taken from Nichols's Literary Anecdotes, vol. II. p. 246; he observes upon it, "I know not what answer was returned by Dr. Bradley, or whether "his advancing infirmities prevented his returning any; the bishop's letter, however, "did not appear in print."

[To the Rev. Dr. Bradley.]

DEAR SIR, March 6, 1759.

I HAVE computed captain Campbell's observations of the 9th of September 1758, and desire that you will be so kind as to acquaint me of any mistakes you may find in the computations. For that purpose I have sent you the heads of the first computation. Five out of the six observations agree with one another to a surprising degree of exactness, but I think they give the long. of the place of observation too much westerly.

As Mr. Brewer seems to think he can determine the times of the appulses of the moon to the meridian, I have desired he will send you the exact times of the transits on several days upon which you had the observations.

I hope you found all friends well in Glocestershire; and I am, reverend sir,

Your very humble servant,

GAEL MORRIS.

1758, Sept. the 9th, the watch 37′ too fast.

The equation of time subt. 3′ ∴ 40′ to be subtracted from the time by the watch, to give the equal time of the observation; and supposing the ship to have been 5° to the west of Greenwich, then the equal time of the first observation at Greenwich 4h. 32′ 20″.

To that time the true place of ⊙ 5^{s} 16° 51′ 1″ and of ☽ 8^{s} 5° 37′ 28″ lat. 3° 48′ 36″ N.

		°	′	″
R. A. M. H.		231	43	50
90°	♎	23	7	10
a Vertice		63	41	40
Parall^{c}. Angle		18	56	
App. dist. ☽ V.		68	12	
App. Long. ☽	♐	5	53	42
⊙	♍	16	52	54
Vis. Lat. ☽ N.		3	1	17
☽ a ⊙ in long.		79	0	48
App. dist. ☽ and ⊙		79	1	39
Sum semid^{rs}.		—	31	22
		78	30	17
Long. west		5	37	

By the watch.

	H.	′	″		°	′	′	
At	4	52	20	long. west.	5	37	2	22½
	4	59	53		5	26	1½	21½
	5	8	2		5	39	2½	23
	5	16	13		5	56	17½	38
	5	25	30		5	37	1½	22
	5	33	21		5	35	2	22½
				mean	5	25		
				for ☽	+	5		

⁂ The two last columns are added by Bradley.

[To the Rev. Dr. Bradley.]

SIR, Bath, Feb. 9, 1760.

I TAKE the liberty to present you with a small book containing some illustrations on sir Isaac Newton's theory of the moon's motion, which I composed some years ago; and having communicated it, when in Italy, to a friend there, he has thought fit to print it since I left that country. It is an imperfect work, but you'll please to accept it as a testimony of my sincere regard to your merit. I am, sir,

Your obedient humble servant,
CHARLES WALMESLEY.

[à M. le Docteur Bradley.]

MONSIEUR, de Paris, ce 18 Nov. 1760.

JE ne sçaurois jamais oublier les précieux moments, que j'ai eu l'honneur

de passer en compagnie avec un homme si célèbre que vous, Monsieur; mais l'accueil favorable et tant de politesse, que vous avés eu la bonté de me faire pendant mon séjour en Angleterre, m'en feront le souvenir cher pour tousjours, et demandent de ma part la réconnoissance la plus vive, dont je vous prie d'être persuadé. Messrs. Clairaut et l'Abbé de la Caille ont reçu vos compliments avec beaucoup de plaisir, s'intéressant beaucoup pour votre santé et pour tout ce qui vous peut faire du plaisir. Le premier disoit qu'il seroit charmé d'avoir de vos nouvelles, et particulièrement de sçavoir votre sentiment sur la théorie du mouvement des comètes, qu'il vous a envoyée il y a quelque tems. C'est un travail, qui lui a couté bien de peine, et qu'il a raison de croire doit remporter le prix de l'académie de Pétersbourg pour l'année **1761**. M. de la Caille paroissoit content de l'excuse que vous m'ordonniés de lui faire sur ce que vous n'avés pas pû répondre à sa lettre avant que ses Fundamenta Astronomiæ furent imprimés.

Messrs. Clairaut et d'Alembert travaillent incessamment à donner séparément de nouvelles tables de la lune selon des corrections qu'ils ont faites à leurs théories déjà publiées. C'est à l'avenir à juger de leur perfection; mais selon ce que M. Clairaut m'a dit lui-même, ses tables n'atteindront pas à une telle perfection que vous m'avés dit que celles de M. Mayer par vos corrections ont déjà.

M. Pingré, qui quitta Paris Lundi passé allant à l'isle Rodrigues pour y observer le fameux passage de Vénus, se plaignit beaucoup de ce qu'il n'a pas eû réponse à sa solicitation d'obtenir un passeport de la cour d'Angleterre pour la sureté de son passage. Il me nomma ce Monsieur Anglois qu'il avoit chargé du soin du passeport en question, mais je ne me souviens pas le nom. M. Gentil de Gallaifiere partit pour Pondicheri il y a quelques semaines, et M. l'Abbé de Chappe s'en ira demain, ou après demain, par Vienne à Tobolski. Ce dernier, après m'avoir demandé de quels instruments Monsieur Maskelyne devoit se servir pour observer le passage de Vénus à S^t. Helene, m'a remis une note dont voici le contenu: "Par la comparaison des observa-
"tions faites 1753 soit à Londres, soit à Paris, par les plus habiles astro-
"nomes, on a reconnu que les différences considérables, qu'on a trouvé dans
"l'entrée et la sortie de Mercure du disque du soleil, provenoient en partie
"de la différente longueur et bonté des lunettes dont on a fait usage. Cette
"même différence est d'autant plus à craindre dans l'observation prochaine
"de Vénus que son mouvement est plus lente que celui de Mercure. Ces
"raisons ont déterminé Messrs. Cassini, Pingré, et Chappe d'Auteroche de

" faire usage dans l'observation de Vénus de lunettes de même bonté et même " foyé de 19 pieds avec des oculaires de deux pouces $\frac{3}{4}$, et d'observer ensem- " ble les satellites de Jupiter pour reconnoître la différence de leurs vues."

Dans une lettre à M. le Docteur Birch j'ai rendu compte des mémoires qu'on a lus à l'ouverture de l'Académie des Sciences.

M. de Montucla a donné l'histoire des mathématiques depuis les tems les plus reculés jusqu'à la fin du siècle passé. C'est un ouvrage de deux volumes in-quarto, et si je ne me trompe, le meilleur dans ce genre qui a paru jusqu'ici. L'auteur est membre de l'Académie des Sciences et de Belles Lettres de Berlin; mais actuellement à Paris. J'ai l'honneur d'être, avec beaucoup de respect et une considération particulière, Monsieur,

Votre très-humble et très-obéissant serviteur,
FERNER.

P. S. Si je pourrois vous être utile à quelque chose ici, j'en serois charmé. Mon addresse est à M. le Profess. Ferner chez Messrs. Tourton et Baur, rue des Deux Portes vis-à-vis la rue Beaurepaire à Paris.

[To the Rev. Doctor Bradley.]

SIR, Bath, Feb. 11, 1761.

AS I am not entirely a stranger to you, I hope you'll not refuse me the favour I ask. Reflecting upon what you told me, when I waited upon you about three years and a half since, that you had observed some little irregularity in the earth's motion, occasioned by the action of Venus, I have undertaken to examine by theory what influence Venus and the earth have to disturb one another's motions. The inequalities of Venus's motion I have in a great measure determined; but those of the earth arising from the action of Venus, I can only settle by supposition, as the attractive force of Venus is unknown. For which reason I must desire you'll be pleased to communicate to me some account of these deviations in the earth's motion, as you have collected them from your accurate observations, that by them I may be enabled to determine the attractive power of Venus, and to compute its effects in all cases where they may be sensible, as also to see how near theory and observation agree.

As I find astronomers differ in assigning the proportion of the real diameters of Venus and the earth, I wish you would also let me know your estimate

of it. Any observations that may contribute to settle the derangements in the motions of Venus and the earth, I shall be greatly obliged to you if you please to impart them to me; assuring yourself I shall make no other use of them but for the improvement of the theory of astronomy, and for your honour and credit. The result of my labour I shall address, as I usually do, to yourself, and put it into your hands. I shall just mention at present that I find that by the action of the earth the nodes of Venus recede 24′ 12″ in the space of one hundred years, and 6′ 55″ by the actions of Jupiter and Saturn; the sum 31′ 7″, if subducted from 1° 23′ 50″ which is the retrogression of the equinoctial points in the same time, will leave 52′ 43″ for the quantity which the nodes of Venus appear to advance from the equinoctial points in 100 years. This perhaps may be of use for determining more exactly some particulars of the passage of Venus over the disc of the sun.

Hoping you'll grant my request, I remain, sir,

Your very humble servant,

CHARLES WALMESLEY.

Please to direct to me at the Bell-tree
house, in Bath.

[To the Rev. Dr. Bradley.]

Lyndon, near Uppingham, Rutland,
June 9, 1761.

REV. SIR,

IT was very clear here for seeing the end of the transit of Venus over the sun on Saturday morning, when the inner contact, which I could fix to 2″ or 3″ was at 8h. 16′ 7″, and with a 15 feet telescope judged the entire separation to be 34′ 40″; but that cannot be so nicely determined, for had not I kept my eye pretty constantly on the place, I could hardly have distinguished it for the last minute. 18′ 33″ for the time Venus was passing off, is less than the calculation, but as a light body on a dark place seems larger than it is, so I suppose a dark spot on a bright body will seem something less, as I think Mercury did when seen on the sun. My latitude is 52° 38′, and longitude about 2′ 30″ of time west from Greenwich. I could plainly see Venus on the sun through a smoked glass without a telescope, as did also some others who were here, though some could not.

It seems very strange to me, that if Venus has a satellite, which they describe as about the size of our moon, and at a like distance, M. Cassini and Mr. Short should never be able to see it more than once, nor any one else, till M. Montagne says he has lately seen it three nights at Limoges; whereas by the size they mention, one would expect to see it with the naked eye when Venus is near us, as having a considerably larger apparent diameter than Mercury or than Mars, except in the nearer part of its orbit. Its periodical time, as fixed by M. Montagne at 1d. 7h., supposes Venus far more * than the sun or other planets; for the moon does not move round our earth with $\frac{1}{20}$th of the velocity, nor do the satellites move round the vast body Jupiter with near that swiftness. On the whole, I still suspend my judgment about it till the affair is cleared up, as I have done ever since Mr. Short's account in 1740. Meanwhile, if you will give yourself the trouble to send me your opinion about that, as well as the transit of Venus, it will much oblige, sir,

Your very humble servant,

THOMAS BARKER.

* So in the MS.

[To the Rev. Dr. Bradley.]

Cambridge, New England,
21 Sept. 1761.

REV. SIR,

BY the advice of his excellency, Francis Bernard, Esq., governor of this province, I lay before you an observation of the transit of Venus, which by his Excellency's patronage I was enabled to make. I flatter myself it will meet with a candid reception, and prove of service in this affair.

I beg leave to subscribe, with the utmost respect, reverend sir,

Your most obedient humble servant,

JOHN WINTHROP.

⁂ This observation was afterwards communicated to Short, and will be found in the Phil. Trans. vol. LIV. p. 279. There is another letter from Winthrop to Bradley, dated May 19, 1760, communicating an observation which he had made (Oct. 1743) of the transit of Mercury, &c. which was afterwards printed in the Phil. Trans. vol. LIX. p. 505. The only additional circumstance to be collected from it is, that the egress is given in apparent time.

The following letter appears to have been written to Hadley in reference to the trials of his sextant, which are detailed in the Phil. Trans. vol. XXXVII. p. 341. The copy of it has neither date nor address, and was in consequence misplaced at the time when the part of the correspondence was printed, in which it ought to have been inserted. It belongs most probably to the latter part of 1732.

SIR,

I AM much obliged to you for the particular account you sent me of the observations we made with your instruments on shipboard; but could I return you thanks equal to your deserts, it must not be in my own name, but of the public, that will doubtless reap infinite advantage from your noble invention. I hope you have already in some measure shared, and will long live to enjoy, that pleasure and satisfaction which must attend actions and discoveries beneficial to so great a part of mankind; since the result of the experiment sufficiently proves, that your invention will be of singular use to all sailors, and instrumental in saving the lives of thousands; a reflection this that cannot but be very entertaining to one of an humane and generous disposition. As for my own part, though I was extremely pleased at the first sight of your instrument, yet I can't help receiving fresh and additional satisfaction every time I reflect on it or look into the results of our observations, since from them I am now convinced, that by means of this contrivance, we may observe at sea with a degree of exactness that 'twas before vain to expect, and which will undoubtedly contribute very much towards the perfection of navigation, not only in determining the latitude to great exactness, and thereby correcting the course as usually measured by the log; but if ever the moon's theory is completed, 'twill be of the utmost consequence in determining the longitude likewise. I could not have imagined it possible to observe with so much certainty on so unsteady an observatory, as I perceive I did, when we lay at anchor at Sheerness; and the observations made the next day, when under sail, make me inclined to think that a little practice will be sufficient to enable any one to take the latitude in tolerable weather, without an error of a single minute. As to the irregularities among the observations, the greatest, evidently, appear to be mistakes in reading the divisions, which by practice may soon be avoided: in the first series of observations that I made, Sept. 1, there seems so regular an alteration in the errors, that whatever the cause of them was, that regularity is sufficient proof of the exactness with which observations may in general be made; but the next series, made by your brother, seems to intimate that a practised seaman may yet observe more accurately than either of us. The circumstances, as you observe, of an imperfect horizon, may be one cause that contributes to the inconsistency of some of the observations; and the inaccuracy of noting the exact time may somewhat add to the errors; though I don't apprehend that the inequalities of the watch's motion during an hour or two will materially affect observations of this kind. I should rather think the rising and falling of the ship upon the waves might be a cause of some disagreement, because that must sensibly alter the visible horizon; for if we suppose the mean height of the eye to be about 11 feet above the still or level sea, agreeable to your allowed dip of $3'\frac{1}{2}$, and suppose a wave to raise the ship six or seven above the mean height, and likewise depress it as much below, there will be a difference of 2′ in the observations taken on the top and the bottom of the wave. But though this cause may occasion a greater apparent disagreement in a series of observations, yet I don't apprehend that the conclusion drawn from the mean of all will be much less certain than if the agreement had been greater, as 'twould undoubtedly be in smooth water.

Upon the whole, I think there is reason to conclude that your instruments will answer, or even exceed, the expectations of the most sanguine, and do honour to the inventor by the eminent service 'twill be of to mankind; which is the hearty wish of, sir,

Your most obliged humble servant,

J. BRADLEY.

[To Mr. Nash.]

Aug. 24. [no year.]

SIR,

THE small time I was with you yesterday not allowing me fully to explain what I thought would be the most proper method for your brother to make use of, in order to determine the situation of the axis of the sun's revolution, and its inclination to the plane of the ecliptic, I shall now beg leave to give you my thoughts on that subject.

From what you then told me, I understood that your brother's way of observing the spots was by transmitting the image of the sun through a telescope, and receiving the same at some distance behind the eyeglass upon a board or the like, on which he had drawn a circle of the same diameter that he would have the sun's image appear, and to which he made it exactly agree, by putting the circle nearer or farther from the telescope as occasion required; and when the image and circle exactly coincided, he noted the point on the board, &c. where a spot fell. But by what you then told me, he had no particular way of knowing thereby the position of the spot, in respect to the ecliptic or equator, which is necessary in order to find the situation of the pole of the sun's motion, and the time of its revolution. Indeed, if he noted the day, hour, and minute of his observations, and took care always to keep his board in such a position, with respect to the horizon, that if its under edge was straight, it might be always parallel to the horizon, which by the description you gave me was always the case in his method of observing: I say, if the observations he has already made are thus circumstantiated, he may from them determine the position of the spot with respect to the equator or ecliptic. For at the time of observation, he must by calculation determine the angle made between the ecliptic and the vertical circle passing through the sun's centre, and then on his board draw a line through the centre, making the same angle with the perpendicular line, and this line will represent the ecliptic on the sun's disc.

For instance, we will suppose RRSS the board on which the image is cast; the plane of this, I suppose, he always keeps at right angles to the axis of his telescope, and likewise the side SS always parallel to the horizon; then VCN drawn through C, the centre of the circle perpendicular to SS, will represent the vertical circle passing through the sun's centre, (Fig. 10.)

If then, for the time of his observation, he calculates the angle made between the vertical and ecliptic, (the method for doing of which he will find in Street's Astronomy, in the chapter about calculating the sun's eclipse,) and then makes the angle VCE equal thereto, the line lE will represent the ecliptic on the sun's disc, and CP drawn perpendicular to CE will represent the circle of longitude passing through the sun's centre, and P the pole of the ecliptic. Suppose now S′ was the place of a spot marked upon his paper or board; then, in order to find the real longitude and latitude of the spot, as seen from the sun's centre, it will be necessary to find the longitude and latitude of that point of the sun's disc represented by S′. In order to this, we will suppose the sun to be at an infinite distance, (for the error, when greatest, that will arise from this supposition, will not make an error of above 12′ in the spot's place; and 12′ on the sun's surface, when viewed from the earth, subtends an angle but of two or three seconds, which is a nicety that our best observations must not pretend to;) I say, we will suppose the sun at an infinite distance, and then VENP may be conceived to represent the sun's disc projected orthographically; in which projection, the circles of longitude will be represented by ellipses drawn through the poles P,w, and the circles of latitude by straight lines parallel to the ecliptic EC. If, therefore, through S′, the point representing the place of the spot, you draw the line parallel to the ecliptic EC; from the nature of the orthographic projection it is evident that the arch Eb will be the latitude of the spot, and making CE radius, Ca is the sine of the spot's latitude; and again, making ab radius, aS′ will be the sine of an angle equal to difference of longitude between the spot and sun's centre. And therefore that angle added to or subtracted from the sun's longitude, according as the spot is to the east or west of CP, will give the true longitude of the spot, as viewed from the sun's centre. But here, instead of taking the sun's longitude as seen from the earth, you are to take the point opposite thereto; because for determining the spot's place, you must suppose yourself in the centre of the sun. For instance, if the sun was in the beginning of Aries, then that point of the sun's disc which to us appears in the centre, if viewed from the sun, would be in the beginning of Libra; and so for the rest.

After this manner he may determine the longitude of a spot at the several times of observation; from which places, so determined, the inclination of the sun's equator to the ecliptic, as also the point of its intersection therewith, may be found. In order to this, three observations of the same spot are sufficient, if they are made with sufficient exactness, and at pretty good intervals from each other, the farther the better, (cæteris paribus.)

Let us now suppose, that the longitude and latitude of the same spot, as viewed from the sun's centre, is given in three different situations; (for example, I will suppose any thing,) the longitudes, for instance, at the three times to be

	S °	S. °	S. °
	0 10	3 18	6 19
The latitudes south...	16½	6¼	4 10′

Then upon a pasteboard, or the like, I draw a circle as big as I please (as Fig. 10.) ♈ ♋ ♎ ♑ representing the ecliptic upon a plane, of which I would now project the sphere stereographically, supposing the eye in the north pole of the ecliptic. Then C, the centre of the circle ♈ ♋ ♎ ♑, will represent the south pole thereof, and straight lines drawn from C will represent circles of longitude, and the circles of latitude will be also represented by circles parallel to the primitive circle; then from the nature of the projection, the distance of the circles of latitude from the pole C must be set off by taking the tangents of half the distance of those circles from the south pole. In order, therefore, to set down the places of the spot on the projection for the first observation, I take the arch ♈a = 0^s 10° and draw the line Ca, representing the longitude of the spot in the first observation, and from the centre C towards a, I set off C1 equal to the tangent of half the spot's distance from C, the south pole of the ecliptic: that is, equal to the tangent of 36° 52′½. In like manner, for the second observation, I take ♈B = 3^s 18°, and C2 = tangent of 41° 52′½; and for the third, I make the angle ♈CD = 6^s 19°, and set off C3 = tangent of 42° 55′. Then through the points 1, 2, 3, I draw a circle, whose centre I find to be in E. Then a line drawn from C through E will give the longitude of the pole of the spot's revolution, which in the present case will be ♈F, and CH will be the tangent of half of the spot's nearest distance, and CG the tangent of half its farthest distance from the south pole. Therefore half its least distance in degrees subtracted from half its greatest, gives the inclination of the ecliptic to the sun's equator; and half its greatest, added to half its least, gives the distance of the spot from the pole of its revolution. Thus in the present case, I found CH = tangent of 36°½, and CG = tangent of 43°½. Therefore the inclination is 7°, and the distance of the spot 80° from the pole of its revolution. All this follows from the nature of the stereographic projection, and upon supposition that the spot is fixed to the sun's surface. For the sun revolving about its axis, the spot will describe a circle parallel to the sun's equator, as I may call it; and all circles on the sphere are projected into circles likewise upon the plane of the stereographic projection: therefore the place of the spot, when laid down in this projection, will be in the circumference of a circle, whose centre may be determined from three given points, and not less, which is the reason why I require three places of the same spot to be observed. From the nature likewise of the stereographic projection, the centres of all the lesser circles are in the same line of measures, as it is by some called, with the centre of the great circle, to which upon the globe they are parallel; so that by determining the longitude circle of one, whether a greater or a lesser circle, you determine the longitude of all; and consequently, the centre of the circle representing the sun's equator, which in this case is the pole of the spot's revolution.

The longitude of the pole of the spot's revolution, and its distance from the pole of the ecliptic, being thus determined, you may then proceed to determine the time of the sun's revolution about its axis. In order to which you must calculate the angle at the pole of the spot's revolution, between the first and last observation, which may be done thus: Suppose C (Fig. 11.) the pole of the ecliptic, and E the pole of the spot's motion, CE the circle of longitude passing through them, C1 = to the complement of the spot's latitude at the observation = 73° 45′ and E1 = to the distance of the spot from the south pole of its equator = 80°, and the angle 1CE = to the difference between the longitude of the spot in the first observation, and the longitude of the pole E = 4^s 5°, then CE1 being considered as a spherical triangle, it will be as the sine of 1E to the sine of C1, so is the sine of 1CE to the sine of the angle CE1. In the same manner you may find the angle CE3 in the last. And having thus got the angle 1E3, say, as that angle in degrees is to 360, or the whole circumference, so is the time between the first and third observations to the whole time of its revolution.

JAMES BRADLEY.

⁂ This letter was published in the Gentleman's Magazine for 1780, vol. L. p. 605. It was communicated by an anonymous correspondent, who says, that it was an "original letter of the late Rev. Dr. Bradley, Astronomer Royal, to Mr. Nash," which had fallen into his hands by chance.

APPENDIX

CONTAINING

OBSERVATIONS OF HALLEY'S COMET IN THE YEAR 1607.

THE Baron de Zach found these observations in 1784 among Harriot's papers, which had descended to the Earl of Egremont from Henry, Earl of Northumberland. The Baron published them in 1793[a], but so very inaccurately, that it seemed right, with Lord Egremont's permission, to reprint them faithfully from the original manuscripts.

		° ′	
Die 15. Sept. 1607, hora 10ª½ p. m.	Long.	10 21	♌
	Lat.	33 18	
hora 2ª ejusdem noctis.	Lo.	12 27	♌
		33 24	

Mr. Standish at Oxford saw it and observed it[b].

1607. Sunday nighte, Sept. 13. When clarke was absente for smith. I saw it not.

thursday midnighte, 17. I saw it first at illford combe. it appeared under the greate beare in a line that might be imagined issuynge from the upper of the four starres of the bodie passing betweene the two lower and opposite, crossing the more westerlie of two of the 3 or 4 mag. that were then under the beare and parallell to the horison and so forth till itt cut the said blaser in a point of that line wher a perpendicle did fall from the utmost starre of his tayle. his traine did reach to the said more westerlie starre, but in lesse than an hower seemed to bee something declined from the said starre westward. the starre was of the second magnitude ferè. his traine was nubelous.

[a] In the first supplement to Bode's Jahrbuch.

[b] These two observations are written by Harriot on one of his memorandum-papers. In another place he has copied out the first of them, and noted, "Mr. Standish at Oxford saw it the 15th, at 10½ p. m."

friday 9. hora. at night. 18. I saw it att [c]kidwellie. wher I was told it was wondred att the night before. but itt was moued verie farre to the westward and was now come almost into the right line of the diagonall drawne from the former upper star of the greate beare, it was att that time in position with another starre parallell to the horison from whom it was distant as farre as ether of the two side starres of the greate beare are one from an other. and so much or more it was moued from the place I saw it in the night before. the traine was nothing so longe, whether it were because the moone now showne that the night before was sett, or because I now beheld it more earlie and it increased afterward in length and brightnesse, as wee thought during the litle time I looked uppon it, it appeared.

Saturday. Sunday. Munday clodie. tuesday 22d, 7 hora att mount [d]Martin with my crosse staffe, I observed it

from boötes, 6° 44′. from lyra, 53° 10′. from ultima caudæ, 31° 40′.

Wensday 23, from boötes. hora septima. octava ferè. octava et amplius
10° 53′ 12° 10′ 12° 56′
from ultima caudæ, 36 50
from lucida lyræ, 52 1 magnitudinis ultimæ caudæ.

Thursday, 24. hora, 7. from boötes, ...17° 0′
ultima caudæ 42 0
lucida lyræ 51 50

friday 25 clodie

[To his especiall good friend Mr. Thomas Harriotte att Sion[e] neere London.]

Sept: 30. Llanihengle[f] Abercowen, Cairmarthen.

1607, Sept. 13. Lookinge upon the starres, and especiallie above the greate beare, ther was no new phenomene.

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[c] Baron de Zach has substituted Illford Combe in this place for Kidwelly, the name of which town he has entirely suppressed: by this alteration, the place of observation has been removed into Devonshire, contrary to the distinct declaration of the writer.

[d] B. de Zach could not make out this name, but it is perfectly clear in the manuscript: and within half a mile from Kidwelly, on the side next to Carmarthen, there is an eminence with a house on it, which is still called Mount Martin.

[e] B. de Zach gives a long note ([c] p. 10.) to determine the precise place where Harriot made his observations, and argues that "Sion, near London," must be understood to mean, not the well known mansion of the Earl of Northumberland, but Sion

17. Passing over the sea into Wales aboute midnighte going abord I saw a Comete. it appeared under the greate beare in a line that mighte bee imagined issyinge from the upper of the foure starres of the bodie, passinge betweene the two lower and opposite crossing the more westerlie of the two that were then under the beare and parallell to the horison and so forth till it cutt the said blasing starre in a point of that line, wher a perpendicle did fall from the utmost starre of his tayle.

his traine did reach to the saied more westerlie starre, but in lesse than an hower seemed to be something declined from the saied starre westward. the starre was of the magnitude of thos of the greate beare and his traine was nubulous.

18. it was moued farre to the westward, and was now come almost into a righte line of the diagonall drawen from the former upper starre of the greate beare. but thes observations were as I iournied without instruments. now I am come to my owne house and crosse staffe that I sent downe for measuring of land. so the observations that follow are more artificiall.

Distantia Cometæ.

Septemb.	à boote.	ab ultima caudæ U maj.	à lucida lyræ.	hora 7^{ma} vespertina.
22	6° 44′	31° 40′	53° 10′	
23	12 10	36 50	52 0	
24	17 0	42 0	51 50	
25		me observa	nte	Cloudie.
26	24 32	50 10	50 48	
27	28 15	53 20	51 54	
28		Willm Low	ër [g]	I was a gossipinge.
29	33 30	58 20	53 30	longitudo caudæ 5° 15′.

ita ut motus ejus est ab oriente per septentrionem in occidentem.

---

college, in London, which in truth had no existence till several years after Harriot's death. Harriot was described in his epitaph as being "de Syon ad flumen Thamesin," and there can be no doubt that he resided and observed in Sion House, on the banks of the Thames, between Brentford and Isleworth.

The letter is evidently drawn up from the preceding notes, which are in the same handwriting with it.

[f] B. de Zach reads this Slani Eegle Aberrcoroen, and thinks that it may have been a seat of Lord Abercorn. All that he deduces from this must of course be erroneous. Llanfihangel Abercowen is a town lying a few miles to the south-west of Carmarthen.

[g] B. de Zach entirely omits this name, as well as "me observante," which are con-

* * * * *

distantia Cometæ, 1607. à[h]

hora septima.	boötes.	Ultima caudæ ursæ majoris.	lucida lyræ.	vespertina.
Sept. tuesday 29	33° 30′	58° 20′	53° 30′	nox serenissima ubi certior et accuratior observatio longitudo caudæ 5° 15′
wensday 30				cloudie.
Oct. tuesday 1		64 10		
friday 2				cloudie.
saturday......... 3				cloudie.
sunday 4	40 50	66 20	55 30	caudatissima } tendebat versus meridiem et zenith. ipsa stella iam posita in Ophiucho.
munday 5	41 40	67 2	56 20	
tuesday 6	42 20	68 30	57 20	

HARRIOT'S OBSERVATIONS[i].

1607, Sept. 21[k]. ☽ hora 8. 30′ p. m.—Seen on (18) fryday before, by Sir Allen p. and two or three days before that by others.

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nected with it, and without the slightest authority, says, that the letter was from Nathaniel Torporley, who certainly never wrote it. There are manuscripts of his in the library of Sion College, which are in quite a different hand. The Earl of Egremont gave a number of Harriot's papers to the British Museum, and among them there is a letter, the writing of which is precisely the same with that which is here printed. It is signed " William Lowër," and is dated from " Ira' kenti, on Mount Martin, 6 Feb. " 1610." The Lowers were a Cornish family, (see Gilbert's Survey of Cornwall, vol. II. p. 192.) and the individual in question made his first observations at Ilfracombe, and in crossing from thence to the opposite coast of South Wales.

[h] Lower, in his letter of the 30th of Sept., gave the observations previous to that day. We have now a separate account of all which he made from Sept. 22, to Oct. 6; but it was thought unnecessary to reprint the first of them. The entry of September 29 is, however, inserted, because it contains some additional notices in the last column. B. de Zach has mixed up together the contents of Lower's three papers, without marking what belonged to each.

[i] These are written on four half-sheets of foolscap, and consist of the notes which

1607, Sept. 21.

ca	5° 40′
bc	5 16
cd	5 30

Corpus æquale arcturo, cauda ad oppositum ⊙ nam extendebatur paulum infra[l] d et versus occ. long. 30′ obscura propter lumen Lunæ.

Corpus ad partes orientales rectæ ad, parum circiter[m] $\frac{1}{4}$g

Sept. 22 ♂ hora 8^a. p. m.

	°	′
bf	6	40
af	6	46
ab	10	0
ad	11	20
bd	4	30
ed	14	35
ae	5	0
fg	12	40
bg	11	0

Sept. 23. ☿ hora, 7^a. 0′ p. m.

	°	′
bh	11	30
ah	11	45

Sept. 24. hora $6\frac{1}{2}$

bk	16	45	14, 45[n], + $\overline{na}$ ut potui propter nubes.
ak	16	40	

mk	8	8	hora 8^a 0′	bk	17° 0′
gm	20	23		ak	16 50
ma	25	10			

Crosse changed and newe and no paralaxis considered.

Harriot made with a pencil. It is much to be regretted that some one has taken the unwarrantable liberty of tracing them over with ink, and has by that means substituted his own unauthorized version for the original documents. This is the more unpardonable, because the person, whoever he may have been, was unable to make out correctly either the English or the Latin.

[k] The observations of Sept. 21, 2, 3, 4, are contained in the first half-sheet, together with a drawing illustrative of them, which is copied in Fig. 12. In it, as well as in Fig. 13 and 14, the small Italic letters are those which were found in the manuscript, and Bayer's references with the names of the constellations have been added (between brackets) to each, that the explanation might be more perspicuous.

[l] B. de Zach, implicitly following the penman, has printed "extendebatur paulum "infra, dispersus occ." If these words were supposed to imply that the tail extended below the comet, they would contradict the other part of the sentence as well as the fact; for at the time of Harriot's observation the sun had set little more than two hours, and the comet was near the western horizon. The difficulties, however, do not belong to the original MS.—the word immediately preceding occ. is versus, and before that, it is easy to see in the pencil marks, which still remain, that Harriot had written &, his way of forming which contraction has been mistaken for a *p*; *i* and *s* have been interpolated to make out the word; no pencil marks appear of these letters, and certainly there is no dot above the line, to indicate that *i* had been written there.

[m] B. de Zach has printed this as belonging to the 23d, instead of 21st of Sept.: "*ad*," which refers to the line in Fig. 12. drawn from *a* to *d*, has been mistaken by him for the word "*at*," otherwise he must have seen his error.

[n] B. de Zach omits the numbers, and reads "plus non potui." Now 14, 45 are very

1607, Sept. ☽ 28. hor. 7^a (30′)[o]

kb 26° 30′ (25° 0′)
ka 26 30
$*^2$ 28 10
at rectangles with the line q^r. g.

per $\overline{\cup}$

5° 50′	kl	5° 43′	l, m, p, in recta
6 25	lm	6 37	l infra rectam mk
	mn	1 35	parum circa $\frac{1}{4}$ g.
12 10	km	12 21	
	lo	8 30	
	lq	5 0	

Cauda longior ista nocte ob tenebras $1\frac{1}{2}$ circiter vel amplior, obscura[p]. Corpus lucidum 2æ magt. i.

clear, and so is the word "ut," which he has taken the liberty of omitting. It is very improbable that the symbol of addition should have been substituted for the word "plus;" and what immediately follows has been so marred by the penman, as to make it difficult to ascertain what it originally expressed. Nothing therefore remained, but to print faithfully what seemed to be indicated by the MS. On Sept. 29, the same cross-mark is prefixed to an observation which is designated as "dubia." To radius 100, 14 is the tangent of 7° 58′ and 45 is the tangent of 24° 14′. If, therefore, we suppose that Harriot after observing *bk* and *ak* had endeavoured, as he did afterwards at 8 o'clock, to take *mk* and *ma*, these numbers would not be far wide of the measures: they would both be too small, and it must be acknowledged that the tangents on his cross are afterwards entered to radius 1000; but this is a rough observation on which he placed no dependence. It is possible, therefore, that the memorandum may mean "14, 45 were "the tangents, but this is uncertain, for I took them as I could on account of the "clouds."

[o] The second half-sheet contains the observations of Sept. 28, with the drawing from which Fig. 13. is copied. This is afterwards referred to, wherever the comet's place is designated by the letter *l*; *kb* and *ka* connect them with Fig. 12; this does not appear to have occurred to B. de Zach, who, possibly from not seeing their meaning, has omitted these quantities altogether. He has likewise omitted the mark over the second column, which may indicate that the object was brought into contact with the rod: 28° 10′ may be intended to give the distance of *k* in Fig. 13, from *d* the second star from *b* in Fig. 12.

[p] B. de Zach has omitted these three last words; he possibly had a difficulty in making them out, but fortunately they have been unmarked by the penman, and there can be no doubt about them. It would have been well if he had equally abstained from touching what follows *corpus lucidum*. The comet on the 21st appeared equal to Arcturus, it had now diminished and come down to the 2d magnitude; the words in pencil may yet be clearly made out, although they have been written over in ink. To complete his blunder, the penman has interpolated an *h*, and led B. de Zach into the mistake of printing "hora 2 mane," for which there is not the slightest authority.

[q] Sept. 29. ♂ ho. 7^a per intervalla observat.
kl 138 7° 52′
+lm 78 4 28 dubia
mo=lo visu, 120 6° 50′

Cauda respiciebat borealiorem humeri dextri ophiuchi et longiorem vidimus ista nocte quam antea, usque ad (o) circiter, quasi 7° 0′ (rara). (o) orientalior parum.

Corpus $2^{\text{æ}}$ magt.[r] i. et satis clarum.

l Cometa[s].

Oct. 3. ♄ hora 7^a.

lm	37	$\overset{\circ}{2}$	$\overset{\prime}{7}$
lp	$116\frac{1}{2}$	6	38
lo	$143\frac{1}{2}$	8	12
lq	128	7	18
ln	65 backe 16 veré 0° 55′		

+ Cauda ad australem humeri dextri et inferius rara[t] et obscura.

Corpus minus $2^{\text{æ}}$ magt.

kn $\frac{2}{3}$ as nm[u]

Cometa infra rectam mp circiter $\frac{1}{4}$ g, m supra rectam lk.

$$\begin{array}{r} 7\ 52 \\ 5\ 43 \\ \hline 2\ \ \ 9 \end{array}$$

---

[q] The third half-sheet contains the observations for Sept. 29, Oct. 3 and 5; the numbers which now precede the degrees and minutes are the tangents on the cross-staff which belong to them, the radius being taken as 1000.

[r] B. de Zach has here again printed "$2_{\text{æ}}$. mane," where the true reading is still more clear than before.

[s] This notification of the letter *l* now being generally used to denote the place of the comet is omitted by the B. de Zach.

[t] This word has been left in pencil, and therefore no doubt can be entertained about it. B. de Zach reads "cauda...inferius visu, &c."—for the next line he has printed "corpus minus $2^{\text{æ}}$ hora mane."

[u] There is annexed to this, a drawing which is copied in Fig. 14. It is possible that *kn* may not mean the distance of *n* from the point *k* in the figure, but from the place of the comet. This supposes Harriot to have returned to the notation used on the 24th of Sept. which is liable to objection; but no better explanation has occurred. B. de Zach has omitted this part.

Oct. 5. ♂ 6ª.

lm			
lo	181	10° 17′	
lq	175	9 57	dubia[x]
lp	66	3 47	
ln			

Oct. 6. ☿ h. 7ª.[y]

lm	110 dub. 6° 18′
lo	
lq	
lp	
ln	

Oct. 13. ♂ hora 6ª.

		° ′
lm	162	9 14
lo	260	14 39
lp	42	1 24
lr	181	10 18
[z]ml	171	9 45

lp, et borealis humeri dextri ophiuchi in recta linea.

Corpus = p. fere.

Cauda obscurissima.

Oct. 22. ♃

It a was reasonable clear[a] inough to have sene the comet, but for the light of the mone, and for hauzines of the horizontal ayre. for the two knees wold scarce be sene sometimes. and at the first they wold seme but as starres of the 4th magt.

---

[x] Dubia belongs only to *lq*. B. de Zach has printed it so as to connect it likewise with the two other observations.

[y] The fourth half-sheet contains the observations of Oct. 6, 13, and 22.

[z] The penman has here again been doing mischief. B. de Zach, in consequence, has not been able to make out any thing; but in the pencil marks, which yet remain unobliterated, the letters *m* and *l* may be imperfectly traced; it appears, therefore, that Harriot gives a second measure of the distance of the comet from the star *m*, from which it was now receding.

[a] B. de Zach reads this: "it was a reasonable cloud * * to have sene." But the "a" between "it" and "was" is certainly redundant, and as certainly there is no such word as "cloud" in the MS.; the fourth letter in it is an "a," and the last is not carried up above the line, or at all similar to Harriot's final "d." Although the penman has been here at work, there can be no difficulty with respect to the two first letters, and very little about the word as it is now printed. Zach could not make out that which follows, but as the two first letters have only been inked, there can be no doubt of what has been written in pencil.

I thought I saw him with much a do betwixt the knees, but lower and according to his proportion of motion as he sholbe in, as gathered by conference with my papers.

[It was in this place that Harriot had inserted the memorandum of Standish's observations. See P. 511.]

×175×
×135×

Harriot also left some observations of the comet of 1618, but they are of less importance, and not particularly connected with the present publication: some few remarks, therefore, on the manner in which B. de Zach has given them will be sufficient. His text contains all Harriot's measures, and with the following corrections may be applied to any useful purpose.

The originals consist of two parts: the rough entries in pencil, which, like those of the comet of 1607, have been disfigured by being traced over in ink, and a collection of the observations written out by Harriot's own pen. These have not been kept separate, as they ought to have been, that the reader might judge for himself of what was the first impression on Harriot's mind, and what might be considered to be his final opinion. The following corrections must likewise be made.

Nov. 20. "Cometa visus hic a quibusdam die 9 nocte" should be
"a quibusdam 9. di. ante"
"Tycho 33° $1'\frac{1}{2}$" should be "Tycho 33° $1'\frac{2}{3}$"

Dec. 1. "hor. $5\frac{3}{4}$ by the clock of Thistlworth" should be "by the clock of "Thistleworth."

This is the way in which about the time of Q. Elizabeth (Lysons's Environs of London, vol. III. p. 79.) it was usual to call the village of Isleworth. Sion House is situated in that parish; if therefore it were possible to entertain any doubt of the place where Harriot made his observations, it must be completely removed by this reference to the town clock.

There is a rough drawing annexed which exhibits the tail of the comet extending 38°, and reaching a little beyond "$4^{æ}$ m. χ in Bayero."

Dec. 4. Harriot says, "ho. $5^{a}\frac{1}{2}$ p.m. I saw the comet N.W. almost in a "right line with the two uttermost stars of the tayle of the beare, and $19°\frac{1}{2}$ "distant from the top of the tayle" (of the bear) "or nere. The tayle" (of the comet) "dim and pointing to the lowest and nearest of □."

B. de Zach has altered the time to "5½ mane:" probably because the other observations were all taken in the morning; but it escaped his recollection that, at this season of the year, the tail of the Great Bear is in the western hemisphere at 5½ o'clock in the evening.

In the latter part likewise he has made some mistakes, where he reads "19°½ distant from the top of the tayle near the tayle and pointing to, &c." There is a part with a dotted line of red ink drawn under it, and B. de Zach has written also in red ink, on the side of the observation paper, "N.B. Ceruso scripta atramento expinxi, quædam non sunt legenda, difficileque intellectu, tamen nullius momenti." Now, in the part so marked, the original pencil-writing has fortunately been left untouched, and it is neither illegible nor difficult to be understood: it likewise completes the sense, which is rather obscure from the omissions which the Baron has made in the text. Harriot had written, in the place referred to, "or feré nere," and he drew his pencil through the Latin when he added the English word in the place of it.

The last distance is said to be taken "platice," a word which the Baron mentions as unintelligible: it is not indeed classical Latin, but it was used commonly in the middle ages when any thing was done roughly or without minute accuracy.

In the MS, "12° 15°" are not annexed to the comet in the way in which they are printed. They are taken from the fair copy; in the rough entry we find

> Cometa obscurus ϱ = cauda aliquando 8° 11°.
> Cauda versus infimam et proximam □ Ursæ.

This, however, is the penman's version; the ϱ probably is only Harriot's contracted way of writing "et" which connected the two first words with what was to follow. He did not make the sign of equality exactly as we do, but drew two upright lines in the middle between those which were horizontal and parallel. What we have therefore after ϱ was probably nothing but a mark to fill up the space which had been left in writing; there is something similar in the MS. between "cauda" and "aliquando."

Dec. 8. Intervallum ought to be intervalla. There are also some little variations of expression between the rough entry and the fair copy, but nothing that affects the sense. The same would apply to some other days.

Dec. 11. This day's observations have not been written out in ink by Harriot, and it would have been well if that office had not been attempted by some later hand; there might then have been a chance of recovering parts of them which have been omitted in the printed copy: but written over as they have been in ink, it requires more conjectural emendations to elicit sense, than it might be right or useful to apply to them.

In Harriot's Table, as printed at p. 34, there are some corrections which are necessary.

N°. 1. the hour of observation ($5\frac{1}{2}$) is omitted, and in the last column $0'$ should be $6'$.

5. ♎ belongs to this observation, and not to N°. 4.

8. ♍ belongs to this, and not to N°. 7.

extreme difference in Lat. for $15''$ read $25''$:

At the end, "Summa" belongs to $66° 52' 22''$; and "Distantia, &c." to $66° 35' 50''$: a distinction, which is lost by the arrangement used in printing the quantities.

"Some notes concerning this comet 1618" (p. 36.) have been mislaid, and the MS. could not be found in order to compare what has been printed from it with the original.

So much has been said about Harriot's papers, that the subject cannot be dismissed without some further notice. After B. de Zach had found them, he entertained the wish of preparing a certain portion for the press. In 1786 he accordingly applied to the University of Oxford to print the work for him, which, upon his undertaking to provide the necessary apparatus and illustrations, was immediately agreed to. Instead however of fulfilling this engagement, he at last transmitted, in 1794, merely a certain number of the papers, which he had selected from the rest. These were put into the hands of the late Dr. Robertson, whose report on them may be seen in Dr. Brewster's Edinburgh Philosophical Journal, vol. VI. p. 316. It was decidedly against any publication being undertaken; and the papers were accordingly restored to the Earl of Egremont, with whom they have ever since remained.

B. de Zach had previously published, in 1793, what he had prepared on the comets of 1607 and 1618, and his having in this manner preoccupied the ground, made it now necessary to enter into many verbal remarks, which might otherwise have been avoided. Among the other papers, the two most remarkable collections are the observations of the spots on the sun, and of the satellites of Jupiter. These are certainly very curious as original records of early observations; but the curiosity depends chiefly on this particular circumstance. We should learn nothing new from facsimiles of Harriot's drawings of the solar spots, and of the configurations which Jupiter's satellites presented to him, especially as none of these last are so early as Galileo's. For the rest of the astronomical papers, it is painful, but, in due regard for the truth, it is necessary to state, broadly and unequivocally, that no dependence is to be placed on the account which was published of them in the Berlin Ephemeris for 1788, and of which a translation has been so widely circulated in this country.

ADDITIONS AND CORRECTIONS.

P. iv. was printed before Sir J. Herschell had made his valuable communication, on double stars, to the Astronomical Society in 1831.

P. vii, lxx, lxxii, lxxv, for "Foulkes" read "Folkes."

P. vii, note [y], add, "MSS. in Bodleian Library."

P. xiv, note [x], "Smith drawing of it," add, "(see P. 96. note [c].")

P. xv, line 23, "1802," read "1803."

P. xxiv, line 12, "for he says," read "and he says."

P. xxxviii, line 29, "Stawwell," read "Stawell."

P. li, note [e], line 3, "for fifteen years; but probably not so long, as he went, &c." read "for fifteen years, but probably not so long; he went, &c."

P. lxviii, note [t], "Lemonnier," read "Le Monnier."

P. xc. The latter part of note [i] ought to have been omitted. Unequal pressure would alter the position of the frame.

The half title prefixed to p. 93. ought to have been dated MDCCCXXXI.

P. 134, line 5, from bottom; "It should be north," ought to have been added, as it has been written on the opposite page of the MS. by Molyneux. The observation of the 19th of December raised some suspicion of the accuracy, with which the mark had been placed on the limb of the instrument; it is to this doubt, that we are most probably indebted for the important observation of the 21st. There can be no mistake about the direction, in which the star was found to be moving; but Bradley and Molyneux seem to have differed respecting the point, to which they referred the relative terms of north and south.—See P. 133, and again P. 173, 174.

P. 217, line 6, from bottom,	1 29,5	read	1 24,5
233, line 17,	5 0,4	—	5 0,5
320, last line,	192,6	—	162,6

341, note [b].

	°	′	″
The latitude of Greenwich in Halley's Tables is	51	28	30
The north declination of 4 Geminorum	23	0	5
	28	28	25

It appears therefore that the mistake consists in "declination" having been written for "zenith distance" at Greenwich.

P. 360, note [k]. The following memorandum was found in a handwriting which appeared to be Mason's.

"Sept. 30, and Oct. 8, 1726, by 60 observations, Dr. Bradley, by a micrometer fitted "to 15f. telescope, and also by many observations made with a chronometer or timepiece "which he could stop at quarter of seconds, by which he took the time of the sun's passing a wire, and then found his diameter by turning the time into motion; he says,

"set ⊙ mean diameter at (apparent)	32′ 2″
"greatest	32 35
"least	31 30

“ in the Connaissance, 1760, it is said, Dr. B. fixed the least at 31′ 28″; but it is a
“ mistake.”

P. 416. The portion of Dr. Bevis's letter[e] which is omitted, has been alluded to in P. xliii. of the Memoirs; it is now therefore subjoined, that the reader may be enabled to form his own judgment on it.

“ . . . . . . You may remember I spoke to you of this when I had the pleasure of
“ seeing you at Newington: and because I hope that your sagacity or future trial may
“ solve this difficulty, which I take to be a matter of some importance; and that by what
“ observations I have likewise made with the telescope of the mural quadrant, which
“ you know is hung on a separate wall in a quite different manner from the transitory,
“ it obtains there too, and probably may do so in other fixed instruments, if not in all;
“ I will take leave to describe the manner of it as succinctly as I can.

“ Our mark is of this form ⊙, fixed about 600 yards southward, and on a level with the
“ instrument, and very nearly in the plane of the meridian. It is about a yard above the
“ ground on the remaining trunk of a large dead oak, and so scarce liable to any local
“ motion. However, not to be deceived in this essential particular, I fixed another mark
“ on the ground, just under it, and found that both were at all times affected alike.
“ If the cross hairs be brought at noon to bisect the mark, thus ⊕, the cross will soon
“ begin, and continue to advance apparently westward, (the image being inverted,) but
“ really eastward on the mark till sunset, when it usually was seen thus ⊕; and if it
“ be let alone without farther rectification till next morning at sunrise, it will appear
“ thus ⊕: but had it been rectified in the evening to the middle of the mark, it would
“ then be seen next morning still farther eastward apparently, but really westward[f].
“ This, sir, is the constant fact, saving that the deflection is always least when
“ there is least sunshine. And if the sun shines not at all for a whole day, and more
“ especially if it be a rainy day, it is scarce sensible. Whether the cause operates at all
“ in the night time I cannot say, having found it very difficult to illuminate the mark so
“ as to make it distinct at such a distance. Only this I am sure of, that when the instrument
“ has been carefully rectified at noon, without any new rectification in the evening,
“ this sort of observations which I now send you have, upon the whole, agreed much
“ the best with one another. All that I am able to guess about it, with any shadow of
“ probability, is, that the sun's heat, by rarefying the air more in that quarter from
“ whence it shines than elsewhere, may perhaps occasion an azimuthal refraction, whereby
“ an object seen on the meridian when the sun is in that vertical, may, when it is removed
“ from thence, appear out of it the contrary way, as the nature of refraction
“ should seem to require. But this I propose only as a conjecture, to be examined by
“ farther trial.”

P. 412, 496, “de Lisle,” read “de l'Isle.”

P. 496, “Flamstead,” read Flamsteed.”

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[e] “ If the instrument be rectified at noon, “ and not rectified again before the next noon, “ it will be then found true to the mark in “ any weather.”

INDEX.

ABERRATION, p. xii, 1.
—— discovery, p. xvi.
—— cause, p. xix, xxx, lxi, 6.
—— laws, p. xxii, xxx, xxxiii, 5, 9, 287, 484.
—— maximum, p. xxxv, xxxvii, 10, 14, 29.
Academies, Royal, of Paris, Berlin, Petersburgh, Bologna, p. lxxi.
Airy, p. lvi.
Ashmolean Museum, p. xxxviii, xxxix.
Assistants at Greenwich, p. li, xcii, xcix.
Atwell, p. vii, lxi.
Aurora Borealis, p. lix.
Bacon, p. xxxi.
Barker on comet of 1682, p. 491.
Bessel, p. xci, xcii.
Bevis, p. xxxiii. Halley's tables, p. v, 431, 456, errors of transit, p. 416, 418, 524. stars for comet of 1737, p. xliii, 416, 417. solar eclipse 1738, p. 415. comet of 1744, p. lviii, 425. Uranographia, p. 455, 456, 457. transit of Venus, p. xv.
Bird, p. lxxiv, lxxvi, lxxvii, lxxviii, lxxxiii, 75, 466.
Bliss, observation of transit of Venus. p. c. Jupiter's satellites, p. 422. comet of 1744, p. 426.
Bradley family, p. i, xi, ci, cvi.
—— John, p. li, xcix.
—— Richard, p. vii.
Brahe's, T. portrait, p. 469.
Bishop Brinkley, p. xxxv, lxvi.
Brice, p. i.
Bouguer, p. 430. heliometre, p. 439.
Burnet, p. x, lx.
La Caille, p. lxv, lxviii. Ephemerides, p. 429, 441. orbit of comets, p. 436, 440. solar tables, p. 454. observations at the Cape of Good Hope, p. 464, 470, 477, 478.
Calendar reformed in 1751, p. lxxx, 461.
Campbell Colin, p. xl, 62.
Campbell, John, p. xcviii, 87, 493.
Cassini, p. iv, vi, xiii, xxxvi.
Cavendish, Lord Charles, p. xxix, 237.
Celsius, p. xlii.
Chabert, p. 471, 473.
Chesterfield, Lord, p. lxxx.
Clairaut, p. xxxiii. laws of gravitation, p. 435, 439, 451. mutual action of Jupiter and Saturn, p. 436.
Comets, 1607, p. 511.
—— 1618, p. 519.
—— 1707, p. x, xciii.
—— 1723, p. x, 42.
—— 1729, p. 437.
—— 1737, p. xlii, 49.
—— 1742, p. xliii, lxii, 371.
—— 1743-4, p. lvii, 373, 423, 425, 426, 427.
—— 1748, p. lix, 375, 444.
—— 1757, p. xciii, 53.
—— 1759, p. xciv, 379, 491.
—— calculation of orbits, p. x, xliv, 436, 440, 488, 491.
Copernican system, p. xii.
Copley medal, p. lxx.
Costard, p. xcix, c.
Crewe, Lord, Bishop of Durham, p. xxxviii.
Daguesseau, p. lxxxi.
Dalembert, p. lxvi.
Daval, p. lxxxii.
Degree in Lapland, p. xlii, 20, 404—412.
—— Peru, p. 430, 439.
Delambre, p. x, xvi, xxxvi, xxxvii, xlii, xliv, lxx, lxxxvi, xcii.
Dereham, p. xxxiii, lxi.
Derham, p. ix.
Desaguliers, p. xxxvii.
Double stars, p. iv, 221, 244, 342, 346, 354.
Earth's figure, p. 67, 399, 400, 404, 430, 439.
Eclipses, lunar, p. 357, 362, 363, 371.
—— solar, p. lix, 342, 359, 361, 365, 366, 367, 369, 391, 415, 443, 446, 447.
Euler's Lunar Tables, p. xcvi, 432, 448.
Experimental Philosophy Lectures at Oxford, p. xxxvii, xcviii.
Ferner, p. xcvii.
Flamsteed, p. v, xiii, xviii, l, lxiv, 16, 75, 469.
—— Cat. p. liv.
Folkes, p. vii, xlvi, lxx, lxxv.
Frisi, p. 497.
Galileo, p. xii, xxxi.

Godfrey, p. xl.
Graham, p. xiii, xxvi, xxvii, xl, xlii. l, li, lii, lvi, lvii, lxviii, lxxii, lxxiv—lxxvi, lxxxiii, 2, 20, 128, 201, 273, 387. pendulum experiments in Jamaica, p. 62, 395, 399.
Greenwich observations, p. liii, xcii.
——— vicarage, p. viii, xci.
Groombridge, p. lxxxvi.
Hadley, p. ix, xl, 505.
Halley, p. iii, xi, xxix, xxxix, xliv, lv, lxi, xci, xcix, 42, 208, 248. instruments, p. lxxii. tables, p. v, vi, xcvi, 431, 436, 441, 459, 461.
Harriot, observations of comet of 1607, p. 514, of 1618, p. 519. papers, p. 521.
Harris, p. xli, 64, 398.
Hearn, p. xxvii, lxxv.
Hearne, p. ix, xxix, xxxvii, xxxviii, xliv.
Heath, p. xcvi, xcix.
Herschell, p. iv. xxxv. 523.
——— Sir W., p. lxix.
Hipparchus, p. xxxvii.
Hoadly, Bishop, p. vi, viii.
——— Dr., p. xxix, 254, 257.
Hodgson, p. v, 469.
Hooke, p. xii, xvii, xxiii, 1, 16.
Hornsby, p. lxxxiv, xc, xcii, 78.
Huygenian telescopes, p. ix, xi, lx.
Iles, p. i.
Illumination of wires of telescopes, p. xlix, li, 107, 420.
Instruments at Greenwich, 1742, p. 381.
Jones's library and manuscripts, p. xlv.
Jupiter observed. p. 339—344, 347—349, 354, 355, 358, 365, 368, 380, 401.
——— Satellites observed, p. 462, 463.
——— ——— first, p. 342—345, 347—349, 354—362, 364, 365, 368—371, 412—415, 423.
——— ——— second, p. 343—348, 354—358, 360—362, 364, 368—371, 413, 414.
——— ——— third, p. 342, 344, 345, 347—349, 354, 357, 358, 360, 362, 364, 367—371, 380, 414.
——— ——— fourth, p. 343—349, 354—356, 364, 370, 412—414.
——— tables, p. v, lx, 81, 434.
Kater, p. lvi.
Keill, p. vi, xxxvii.
Kepler, p, xii, xxxvii.
Kew palace, p. xiv, xv, ciii, 2.
King George II, p. lxxiv, xci.
——— III, p. xv, xcii.
—— William IV, p. ciii.
Lalande, p. xiii, xxxii, li, xciv.
Latitude of Greenwich, p. xc, 75.

LETTERS.

From Bradley to Barker, p. 488.
——— Bevis, p. 417.
——— La Caille, p. 495.
——— Graham, p. 395, 406.
——— Grischow, p. 476.
——— Hadley, p. 505.
——— de l'Isle, p. 462, 477.
——— G. Earl of Macclesfield, p. 419. 422, 429, 461.
——— Maupertuis, p. 408, 410.
——— C. de Maurepas, p. 455.
——— Le Monnier, p. 473.
——— Mortimer, p. 462.
——— Nash, p. 506.
——— Smith, p. 401.
——— Stirling, p. 400.
——— Walmesley, p. 498.
To Bradley from Barker, p. 491, 503.
——— Bevis, p. 412, 416, 417, 425, 431, 456, 524.
——— Bliss, p. 422, 426.
——— Bowyer, p. 499.
——— John Bradley, p. 469.
——— La Caille, p. 429, 438, 454.
——— Campbell, p. 493.
——— Clairaut, p. 451.
——— Cleveland, p. 494.
——— Ferguson, p. 446.
——— Ferner, p. 500.
——— Frisi, p. 497.
——— Gesner, p. 490.
——— Graham, p. 395, 396.
——— Grischow, p. 448, 466.
——— Harris, p. 398.
——— Howe, p. 483.
——— de l'Isle, p. 433, 442, 443, 463, 470, 478.
——— G. Earl of Macclesfield, 421, 423.
——— Maupertuis, p. 404.
——— Melvill, p. 483.
——— Milnes, p. 495.
——— Molyneux, p. 391.
——— le Monnier, p. 427, 471.
——— Morris, p. 492, 499.
——— Pingré, p. 487.
——— Raper, p. 459, 460.
——— Schumacher, p. 465.
——— Stirling, p. 398.
——— Suarez, p. 444.
——— Walmesley, p. 490, 492, 498, 500, 502.
——— Wetstein, p. 432.
——— Winthrop, p. 504.
From Folkes to Archbishop Wake, p. vii.

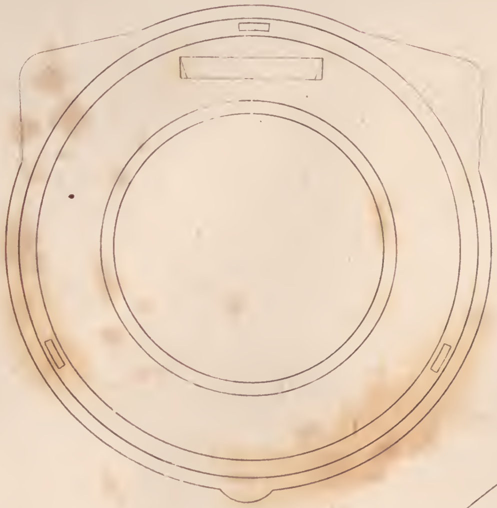
LETTERS.
From Halley to Pound, p. iii.
—— Irving to Ferguson, p. 447.
—— G. Earl of Macclesfield to Jones, p. xlvi.
——————————— Lord Hardwicke, p. xlvii.

Light's progressive motion, p. vi, xxxi, xxxvi, 11, 483. velocity, p. xxxvi.
Llanfihangel Abercowen, p. 514.
Longitude of Lisbon and New York by Jupiter's first sat. p. xi, 58.
———— by lunar distances, see Moon.
Lower, p. 514.
Lowth, p. lxxi.
Macclesfield George earl of, p. xlv—xlvii, liii, liv, lviii, lxxxi—lxxxv.
——— on comet of 1744, p. 423.
——— his instruments, p. 19, 41, 422.
Macclesfield Thomas Earl of, p. vii, lxxii, lxxxii.
Machin, p. xliii, liii, lxvi, 25, 436, 454.
Manfredi, p. xxxiii, lxi.
Mars, p. iii, iv, lx, 339, 342, 344, 350—353, 357, 367, 418, 479.
Maraldi, p. xliv, lix.
Maskelyne, p. liv, lv, lxviii, lxxvi, lxxviii, lxxix, lxxxv, xcix, 75, 197.
Mason, p. lxxxix, xcii, xcviii, xcix, 388.
Maupertuis, p. xlii, lxv. Degree in Lapland, p. 404—410.
Maurepas C. de, p. 455.
Mayer, p. 492. Lunar tables, p. xcvii, xcviii, 84, 86, 492, 501.
Melvill, p. xxxii, 483.
Mercury, transit, p. ix, 355, 371, 433, 482.
Micrometer, p. xcv, 70.
Micrometer screw at Wansted, p. 211, 272, 273, 274, 282, 283.
Mirrors, remarks on grinding them, p. 401.
Molyneux, S. p. vi, viii, xiii, xiv, xvi, xviii, xxviii, xxix, 201.
——— W. p. xvii.
Le Monnier, p. xvii, xliv, lxv, lxviii. 440, 443.
Moon, appulses, p. lix, 342, 380.
—— computation, p. x, xcviii.
—— distances from stars, p. xl, xcvi, xcvii, xcviii, 84, 87, 493, 494, 495, 499, 505.
—— occultations, p. 357, 367, 369.
—— tables, p. xcvi, 84, 86, 432, 448, 459, 501.
Morris, p. xliii, xcvi, xcix, 459, 492.
Mortimer, p. lxx, 408.
Mount Martin, p. 514.
Nebulous stars, p. iii, 361.
Newton, p. iii, vii, xi, xiii, xxix, xl, lxiv, 17, 46, 435, 453.
Nutation, p. xix, lxii, 17, 411, 454, 457.
———— cause, p. lxiv, lxix, 23.
———— laws, p. lxvii.
———— maximum, p. lxvi, 26, 409.
———— tables, p. 37, 457.
Observation with quadrant, p. lv.
——————— transit, p. liv.
Parallax, p. iv, xii, xv, xvii, xviii, lxi, 15, 353, 418, 478.
Pelham, Rt. Hon. H., p. viii, lxxx, lxxxi, xci.
Pendulum experiments, Greenwich, p. lvi, 384.
——————— Jamaica, p. xl, 62, 395—400.
Phelps, p. lxxxiv.
Picard, observations of pole star, &c. p. xvi—xviii.
Pingré, p. lix, xciv, Etat du Ciel pour 1754, p. 487.
Pond, p. lxxvii, xci.
Pound, p. ii—ix, xi, xviii, xlix.
——— Mrs. p. ii, xxxix.
———— her house at Wansted, p. xxiii—xxvi.
Precession, quantity, p. lxiii, 28.
Quadrant apparatus for examining, p. 473, 478.
———— for Gottingen, p. 490.
———— Greenwich, Bird's, p. lxxvi, lxxxv.
——————— Graham's, p. xlix, l, lii, lv, lxxiii, lxxvii. 381, 478.
———— Lord Macclesfield's, p. lxxxiii, 421, 422, 466.
———— for Petersburgh, p. 466, 476.
Queen Caroline, p. xci.
Ramsden, p. lxxvii.
Raper, p. xcvi, 459, 460.
Reflecting telescopes, p. ix, lx, 401.
Refraction, p. xx, lxxxv, 483, 495.
Richardson, p. xxxv.
Richer, p. 62.
Robins, p. lxxv.
Roëmer, p. xxxi, xxxvi, l, lii, lxiv, lxxvi, 11.
Rowley, p. xiii.
Royal Society, p. vi, lxx, c.
Rushout, Hon. Mrs. A., p. xxiv, xxvi.
Saturn, p. 340, 343, 346—350, 354, 355, 364.
Savilian Professorship of Astronomy, p. vi.
Shelton, p. lxxvi.
Shirburn castle, longitude, p. 420.
————— observations made there, p. liii, lxxxiii.
Short, p. lxxiv, lxxix, 473.
Simpson, p. xxxiii, lxvi, lxxvi, 76.
Sion, p. 512.
Sisson, p. li, lxxiv, lxxvi, lxxxiii.
Smith, p. 401.
Stars' parallax, p. xii, xv, xvii, xviii, lxi, 15.
—— proper motion, p. v, lxix, 40.

Stirling on figure of the earth, &c. p. xli, 398.
Svanberg, p. xlii.
Sun's diameter, p. 358—361, 433, 484, 485, 523.
—— parallax, p. iv, 353, 418, 478.
—— spots, p. 506.
Tables for liturgy, p. lxxxi, 461.
Transit instrument, p. xxxix, l, li, lii, lv, lxxii, lxxviii, lxxxiii, 382.
—————— illumination, p. 420.
—————— method of observing with it, p. liv.
Venus, gravitation, p. 502.
—— observations, p. ix, 341, 343, 354.
—— transit, p. xv, xcix, c, 388, 501, 503, 504.
Wake, Abp. p. vii.
Wallis, p. iv, xiii, lxix.
Walmesley, p. lxxxii, 490, 492, 498, 500, 502.
Walpole, Sir R. p. xlviii.
Wansted observations, where made, p. xxiii—xxvi, xxix.
Whiston, p. xlvii, 423.
Whitehurst, p. lvii.
Whiteside, p. vii, xxxvii.
Wilson, p. xxxii.
Wollaston, p. liv.
Wymondesold, p. ii, xxiii, xxvi, xxxix, 18, 403.
Zach, p. 511—521.
Zenith sector, Bradley's, p. xxvi, xxvii, lxxv, 4, 20, 194, 201.
———— Hooke's, p. xiii, xxiii, 1, 16.
———— Maupertuis's, p. xxvii.
———— Molyneux's, p. xiv, xviii, 1, 93.

Plate 1. *at the end of the Volume.*

A



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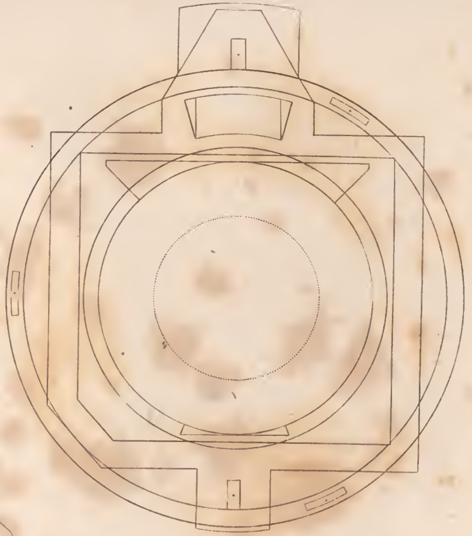
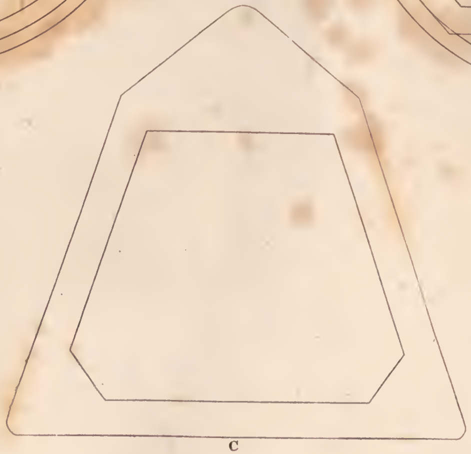
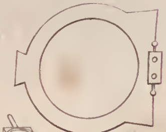




C

Fig. 2.



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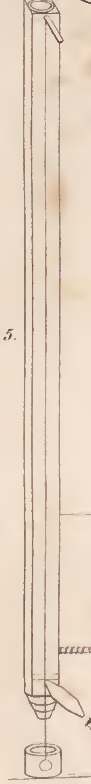


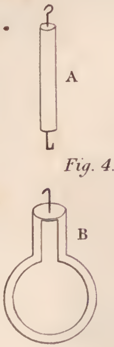
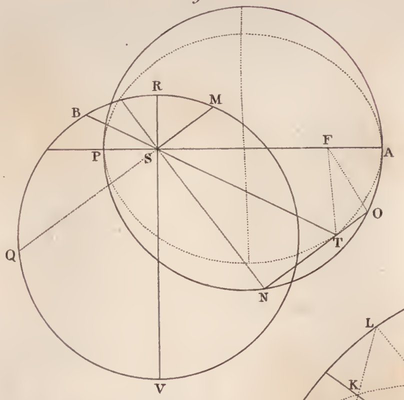


Fig. 4.

Fig. 6.



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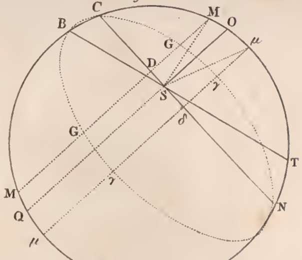

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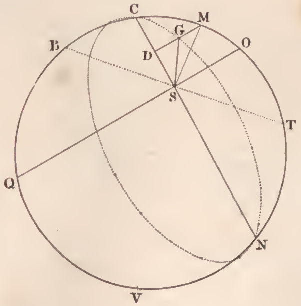


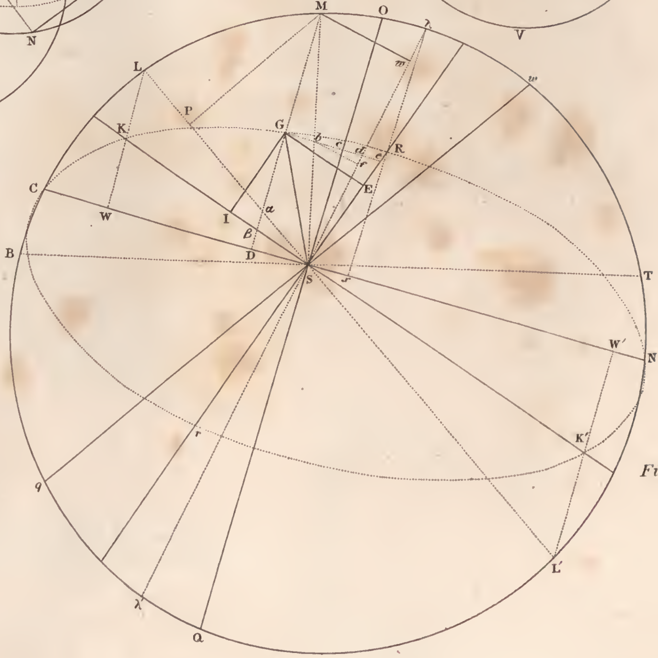


Fig. 9.

Plate III. at the end of the volume.

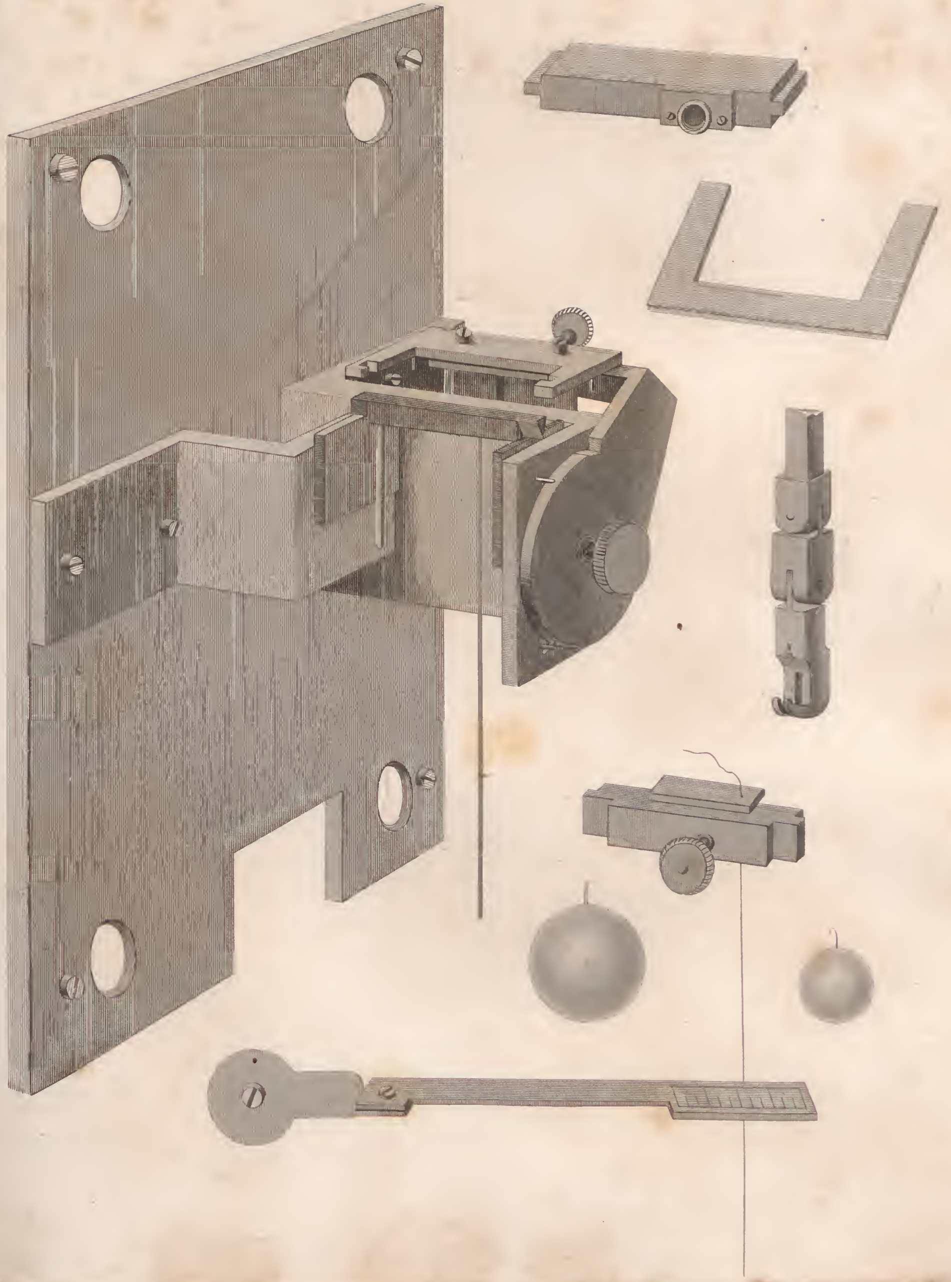


Plate IV at the end of the volume.

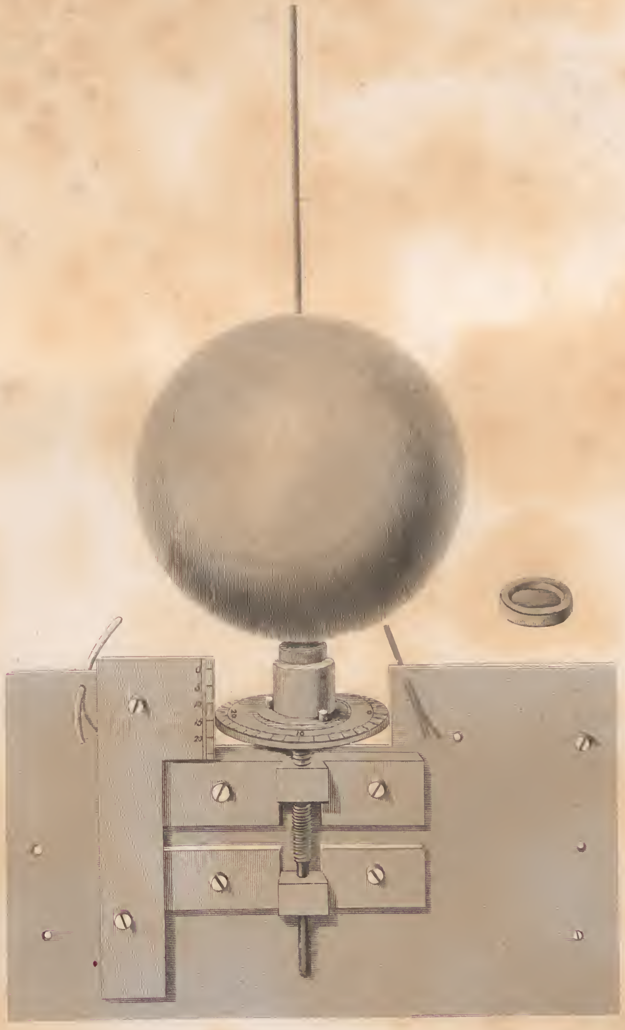
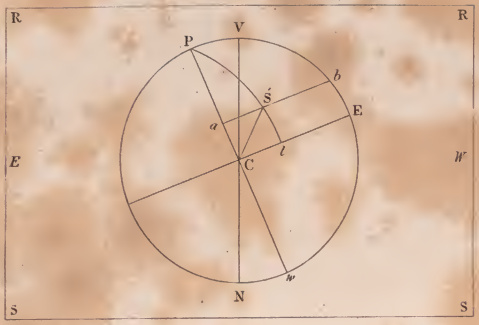


Fig. 9.



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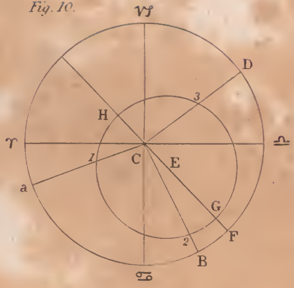


Fig. 11.

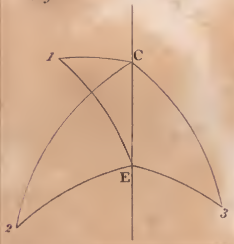


Fig. 12.

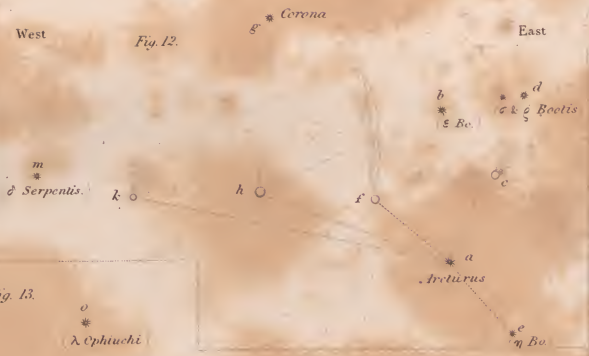
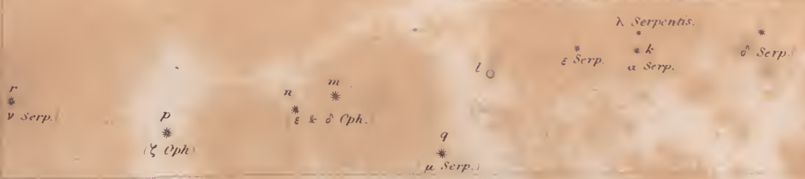


Fig. 13.



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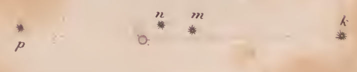

SUPPLEMENT

TO

DR. BRADLEY'S MISCELLANEOUS WORKS:

WITH AN ACCOUNT OF

HARRIOT'S ASTRONOMICAL PAPERS.

OXFORD,
AT THE UNIVERSITY PRESS.
MDCCCXXXIII.

ADVERTISEMENT.

In the Appendix to Bradley's Miscellaneous Works (p. 522) I have ventured to say that no dependence was to be placed on the common account of Harriot's papers. That account, however, was given to the world by one, whose name has now been associated with the cause of astronomy for nearly half a century; it has in some quarters been very readily admitted, and unfounded accusations have been repeatedly derived from it against those, by whose fault it was supposed that the papers were withheld from the public. It was necessary therefore not to leave the contradiction to rest on my bare assertion; and as the discussion had begun in connexion with the publication of Dr. Bradley's works, this supplement appeared to offer no inappropriate opportunity for a full statement of the truth. I have to regret that the scientific world has been deprived of the Baron de Zach before it was possible for me to complete the inquiry: but this has made me doubly careful not to overstate any thing. I have endeavoured also to keep to facts, on which the reader will be well able to form his own opinion, and on the accuracy of which he may, in many parts, easily satisfy himself without requiring any assistance or further explanation. If I have been led on to a greater length than at first sight may appear necessary, it must be recollected that much more detail is requisite to efface a false impression than to satisfy unprejudiced minds. Upon the whole, I trust that I have proved the erroneous and exaggerated view which has been taken of the subject; that the papers sent by the Baron de Zach in 1794 to Oxford did not supply materials for a new edition of Harriot's works, which, it must be remembered, was the original and real design; and, that if such a publication had been unwisely attempted with this imperfect selection, much that was really interesting, and which had never been communicated, must of necessity have been left entirely unnoticed.

S. P. RIGAUD.

March 1833.

Analysis of Calculations made by Bradley when investigating a Rule for Refraction.

WHEN engaged in the publication of Dr. Bradley's Miscellaneous Works, I had the good fortune to meet at Greenwich with a paper book, in which he was in the habit of writing occasional memoranda, and of making the calculations on which he was employed. It is the same to which Dr. Maskelyne has referred in his Dissertation on the Latitude of Greenwich[a]: it furnished me with several curious notices for the Memoirs of their author[b], but it also contained much more, of which I was not able at the same time to avail myself. Dr. Maskelyne, although he first published Bradley's rule for refraction, has given us no particulars of the specific manner in which it was investigated. Now the Greenwich book contained a large number of computations made for that purpose. This was clear from sentences, which were occasionally inserted in them; but the quantities which were used, had for the most part no descriptions annexed to them to indicate the objects, for which they were introduced; natural numbers were mixed with logarithms, the logarithms were most usually without indices, and (what at first created considerable difficulty) in some cases the five first figures were used, and in others the five last of the seven decimals, which are given in common tables. These obstacles have probably contributed to deter others from examining the book, although it has now remained for so many years in the library of the Royal Observatory. I thought it best not to stop the publication of the other papers by dwelling at the time on an examination, the result of which was attended with so much uncertainty; but being allowed to retain the manuscript till I was able to apply more undivided attention to its contents, I gradually became better acquainted with its several parts. In this I had an advantage, from being able to refer to Bradley's book at Oxford, in which he entered his "Observed Refractions," and, although we might wish for more detailed information on some points connected with so interesting a subject, it is hoped that the following particulars, combined with what has been already given in the Memoirs[c], will enable the reader to form a fair idea of the manner, in which Bradley conducted his inquiry.

Descriptions must be tedious, and could not be easily made intelligible; a facsimile is therefore given (in the upper part of plate I. Sup.) of the first numbers which occur in Bradley's calculations. He began, as Dr. Maskelyne has explained[d], by determining the apparent zenith distances of the equator and of the pole, which he found together to be 89° 58′ 6″. This was deficient of 90°, by 1′ 54″, which quantity being the sum of the refractions for these two altitudes, he divided in the ratio of the tangents of the respective zenith distances. His method of calculation for this purpose is too obvious to call for any thing like an explanation of it; but some little time and trouble may be saved by an enumeration of the quantities to which his numbers refer; and for that purpose the following particulars may not be wholly useless.

---

[a] Phil. Trans. vol. lxxvii. p. 156, 160.

[b] P. xxiv. lxxxvi. lxxxvii. xc.

[c] P. lxxxv—xc.

[d] Bradley's Misc. Works, P. 75.

He took tang. Z. D. of equator + tang. Z. D. of pole : tang. Z. D. of pole :: sum of the refractions : refraction for the Z. D. of the pole;

$$\begin{array}{rl}
\text{for } 125530 & \text{is the nat. tang. of } 51^\circ\, 27'\, 30'' \\
\underline{79575} & \qquad\qquad\quad 38\ \ 30\ \ 40 \\
205105 &
\end{array}$$

$$\begin{array}{rl}
9.900778 & = \text{log. tang. } 38^\circ\ 30'\ 40'' \\
-1.311976 & = \text{log. } 20.5105 \text{ the sum of the tangents.} \\
\underline{2.060698} & = \text{log. } 115'' \\
1.649500 & = \text{log. } 44''.6
\end{array}$$

10.098747 (the tangent of 51° 27′ 30″) was originally 9.898293, (the sine of the same quantity which was possibly taken out by mistake,) but is only used to find the natural tang. From the sum of the refraction he separately calculated the difference of them, and the half of the difference would be 12″.8, if the sum is taken as 114″; and 12″.9, if it is 115″. Bradley appears at first to have written $12\frac{1}{2}$, and then to have altered it to 13″, which was nearer to the truth and avoided the fraction of seconds. By this means he got his latitude sufficiently near for his first approximation, and then proceeded to investigate more particularly the effects of refraction.

The refractions, with which he began his calculations, were deduced from the observations of γ Persei for 75° 57′ 30″, γ Draconis for 76° 55′ 20″, and Capella for 82° 41′ 50″. His method of proceeding will be evident from an examination of some of his numbers in the plate; for ease of reference letters have been annexed to each row of figures, and the numbers, which will be used between brackets are intended to mark the distance from the left hand, at which the small square may be in which the quantities under consideration are situated.

The quantities in A are repeated; and it will be better therefore to begin with those in the succeeding line.

$$\begin{array}{rrl}
\text{B (1)} & 9.9992080 & \\
 & +\underline{9.9964628} & \text{sine } 82^\circ\, 41'\, 50'' \\
 & 9.9956708 & \text{sine } 81\ \ 55\ \ 24 \\
\multicolumn{3}{r}{46'\, 26'' = 82^\circ\ 41'\ 50'' - 81\ \ 55\ \ 26}
\end{array}$$

$$\begin{array}{rrl}
\text{B (2)} & 7.2983593 & \text{tang. } 0\ \ \ 6\ \ 50 \\
 & -\underline{8.1305824} & \text{tang. } 0\ \ 46\ \ 26 \\
 & 9.1677769 & \\
 & +\underline{7.1612464} & \text{tang. } 0\ \ \ 4\ \ 59 \\
 & 6.3290233 & \text{tang. } 0\ \ \ 0\ \ 44
\end{array}$$

$$\begin{array}{rrl}
\text{B (3)} & 9.9992080 & \\
 & +\underline{9.7942554} & \text{sine } 38\ \ 30\ \ 40 \\
 & 9.7934634 & \text{sine } 38\ \ 25\ \ 41 \\
\multicolumn{3}{r}{4'.\ 59'' = 38^\circ\ 30'\ 40'' - 38\ \ 25\ \ 41}
\end{array}$$

$$\begin{array}{rll} \text{B (4)} & 9.9992080 & \\ & +\underline{9.8932930} & \text{sine } 51^{\circ}\ 27'\ 30'' \\ & \underline{9.8925010} & \text{sine } 51\ \ 19\ \ 39 \\ \multicolumn{3}{r}{7^{\circ}\ 51' = 51^{\circ}\ 27'\ 30'' - 51\ \ 19\ \ 39} \end{array}$$

$$\begin{array}{rll} \text{B (5)} & 9.1677769 & \\ & +\underline{7.3585965} & \text{tang. } 0\ \ 7\ \ 51 \\ & 6.5263734 & \text{tang. } 0\ \ 1\ \ 9\tfrac{1}{3} \end{array}$$

These numbers are evidently derived from the method of calculation described by Thomas Simpson in his Dissertation on Refraction. At p. 59 he gives the following rule: "As 1 to .9986, &c. or as radius to the sine of 86° 58′½ so is the sine of any given "zenith distance to the sine of an arc, $\frac{2}{11}$ of the difference of which arc and the zenith "distance is the refraction sought." Now the logarithm of 0.99861 is 9.9993959, which clearly answers to Bradley's repeated logarithm 9.9992080. Why he altered it to this particular quantity cannot be determined; nor would the inquiry be of much importance, for it will be found that his whole method was tentative.

Simpson assumes an horizontal refraction, and from it calculates the quantities for different altitudes by a formula which he had determined from theory. Now Bradley knew the great uncertainty of all refractions deduced from observations near the horizon; and the difficulty of reconciling them with Simpson's coefficients evidently determined the first object to which he turned his attention. He therefore took a quantity, which (no doubt for some good reason) he had a little altered from Simpson's, and with it found an auxiliary angle (46′ 26″ in B (1)). Simpson gives a constant multiplier for his angle which is to determine the refraction; but Bradley, taking not the angles themselves, but their tangents, tries, by a mutual comparison of them, to discover what this constant multiplier ought to be. For this purpose he subtracts (in B (2)) the tangent of his auxiliary angle from that of the observed refraction at the same zenith distance, and by that means gets the logarithm 9.16782. As a test of its accuracy, he adds this quantity to the tangent of the auxiliary angle, which he found (B (3)) for the zenith distance of the pole, and the sum gives the tangent of 44″, which was the observed refraction at that elevation. He repeats the same (B (4) and B (5)) for the height of the equator, and again brings out a quantity which is not exactly, but very nearly the same as that which had been deduced for it from observation[e].

This first trial seems tolerably successful; but he was induced to examine the effects of varying the quantity, by which he multiplied the sines of the zenith distances, and at last increased its log. in D (8) from 9.9992080 to 9.9999200. These alterations are evidently arbitrary; and as the same process is repeated through nearly the whole of the remaining part of the calculation, there is only one additional particular which it can be useful to explain. From some other observation the refraction for Capella S. P. appears

[e] By some oversight the height of the equator is written in the little table 51° 27′ 30″, instead of 51° 28′ 30″; but this makes no difference in the auxiliary angle or in the refraction.

to have come out 7′ 10″ instead of 6′ 50″, and the number 3190, which occurs in C (6) and afterwards, was introduced to try the consequence of it, 7.3190440 being the tang. of 0° 7′ 10.″

The numbers at the end of the line E give the refraction from observation. On the fifth of June 1751 it may be seen in the printed observations that Bradley found the zenith distance of Capella S. P. to be 82° 41′ 52″. In the book of "Observed "Refractions," there is a memorandum that, for the error of collimation, 2″ were to be subtracted from zenith distances observed at this time to the north, which will give the quantity here used. To the south, the following observations occur of the same star:

1751	June 4.	5° 45′ 57″½	5° 45′ 53″
	5.	5 45 57½	5 45 54
	6.	5 45 58	5 45 54

The first are from the divisions of 96 reduced to degrees and minutes; and as this was the more to be depended on, it is probable that it was used; and then 82° 41′ 50″ − 5° 45′ 58″ = 76° 55′ 52″. Again, the apparent zenith distance of the pole was 38° 30′ 38″, and twice this quantity is 77° 1′ 16″; 5′ 24″, which is the excess of 77° 1′ 16″ above 76° 55′ 52″, leaves a portion which is to be accounted for by refractions. Now, if to this difference of 5′ 24″ be added the refraction at Z. D. 5° 45′ 58″ + twice the refraction at the altitude of the pole, the sum will be equal to the refraction at Z. D. 82° 41′ 50″. Taking therefore 44″ for the polar refraction, and calculating the other from it (as Bradley most probably in this instance would have done) by the simple ratio of the tangents of zenith distances, we shall have 44″ × 2 + 5″.6 = 93″.6. This is larger than the number (1′ 26″) actually added; but then it is noted in the memorandum with respect to collimation, that 6″ were to be subtracted on the south for that and the error of the arc of the old quadrant: if this correction had been applied to 5° 45′ 58″, the difference would have been 5′ 18″ instead of 5′ 24″. Now 5′ 18″ + 1′ 33″.6 = 6′ 51″.6, which is sufficiently near to 6′ 50″ to make it probable that Bradley introduced the correction for his instrument in this sum of 1′ 26″, which he has used in the calculation.

As the opportunity occurred from observations of circumpolar stars, Bradley continued to apply the same method to them. In the first instance he had varied his logarithm through rather a wide range; but this appears to have been more to try its effect than for any other purpose. He generally began with 9.9990000, a little more or a little less. With this he found an auxiliary angle from one of his lowest stars, and by a comparison of its tangent with that of the observed refraction, arrived at a multiplier, which he tried by its results when applied to his other contemporaneous observations. According as these came out nearer or further from the truth, he varied the quantity which he had applied to the sines of the zenith distances; and with reference to this process we find the following memorandum: "N.B. When the lesser refraction "comes out too small, the log. of the ratio must be diminished; and the contrary, when "it comes out too big."

The repetition of arithmetical computations soon becomes tedious, and he therefore naturally endeavoured to diminish his labour. A logarithm which began with 9.999 would not alter the sine to which it was to be added, either in the index or in the two first places of decimals; he contented himself therefore with writing down the five last decimals, well knowing what he must prefix to the result of them. This could occasion no difficulty to him, and he was only working for himself; but the omission would not obviously occur to others, and it therefore may save trouble to point it out particularly. Thus in the book of "Observed Refractions" there are repeated entries like "$91535=z$, $.1507=x$." This means, that for the observations, to which this memorandum was annexed, 9.9991535 was found to be the best number for determining the auxiliary angles, and 9.1507 was the logarithm which, when added to the tangents of these angles, brought out the refraction so as to answer best, upon the whole, to those which resulted from immediate observation.

In this manner Bradley continued to work till he probably felt the wish of having some more direct method of finding the quantities of refraction. He knew that the simple ratio of the tangents was insufficient for large zenith distances; and the first means, by which he tried to introduce a distinct correction may be traced in the following numbers:

" 92860	25100	92860	25100	92860	25100
" 94850	21517	15494	51517	68358	81208
" $\overline{87710}$	$\overline{46617}$	$\overline{08354}$	$\overline{76617}$	$\overline{61218}$	$\overline{06308}$
" 54 21½	D	63 14 54½	E	75 35 32	F
" 5 38½	2.9253	11 15½	5.8367	22 18	11.5633

"$a + b + c = \text{D}$ then $a = \frac{8}{3}\text{D} - \text{E} + \frac{1}{12}\text{F} = 2.9277$ log. $= 6.46653$
"$2a + 4b + 8c = \text{E}$ $b = -2\text{D} + \frac{5}{4}\text{E} - \frac{1}{8}\text{F} = 0.0001$ log. $= 2.0000$
"$4a + 16b + 64c = \text{F}$ $c = \frac{1}{3}\text{D} - \frac{1}{4}\text{E} + \frac{1}{24}\text{F} = -0.0023$ log. $= 3.30173$."

The tangent of 45° being 1, the tangent 63° 26′ 5″.8 will be 2, and that of 75° 57′ 49″.5 will be 4. Now for the observations of the 18th of November 1750 (the examination of which came immediately before the preceding extract) Bradley had found 9.9992860 to be the best constant for finding the auxiliary angles, and 9.25100 the quantity which was to be applied to their tangents. He therefore uses these numbers to calculate the refraction for the zenith distances 45°, 63° 26′ 10″, 75° 57′ 50″. Accordingly he finds for 45°

9.25100
7.21517 tang. 5′ 38″½
$\overline{6.46617}$ log. 0.00029253 (tang. 1′ 0″.3)

In the same manner for 63° 26′ 10″

6.76617 log. 0.00058367 (tang. 2′ 0″.4)

And for 75°.57′.50″.

7.06308 log. 0.0011563 (tang. 3′ 58″.5.)

The first and second do not differ so much from the ratio of 1 to 2 as the second and third. This must be so from the nature of the case; and Bradley therefore was induced to try whether the second and third powers of the tangents of zenith distances did not enter, as well as the first, into the required formula. To determine this, he assumed the expression that the tang. of refraction $=a$. tang. Z. D. $+b$. tang.^{2} Z. D. $+c$. tang.^{3} Z. D. He then took the three cases in which 1st tang. Z. D. $=1$ and the expression $=\mathrm{D}$; 2nd tang. Z. D. $=2$, and the expression $=\mathrm{E}$; and 3rd tang. Z. D. $=4$, and the expression $=\mathrm{F}$. From these three equations he brought out a rule, thus:

$$\text{tang. of refraction}=0.00029277.\ \text{tang. Z. D.}+0.00000001.\ \text{tang.}^2\ \text{Z. D.}-0.00000023.\ \text{tang.}^3\ \text{Z. D.}$$ [f]

In this result the coefficient of tang.^{2} Z. D. came out so very small, that he was induced to neglect it altogether, and by that means his formula became a. tang. Z. D. $-c$. tang.^{3} Z. D. The subject having occupied some of the first mathematicians of the present time, it is very remarkable that their investigations lead to a series depending on the odd powers of the tangents of zenith distance, and M. Laplace [g] shews that his term, involving tang.^{5} is almost insensible even at 88° dec. or 79° 12′ sexag.

Bradley examined his coefficients by trial on observed refractions, and found that they were not sufficiently accurate. This may naturally have been expected. The tangents for large arcs increase so rapidly, that no dependence could be placed on the use of their cubes, unless the quantity assumed for a multiplier had been determined with greater precision than the first inquiry could attain to. By further modification, however, he satisfied himself better on this point, and the result of his inquiry may be seen from the following memorandum:

> "Suppose the constant number to be added to the thermometer to be 450, then add log. 2.9853 to the log. of the height of the barometer, and from the sum subtract the log. of 450 increased by the degrees of the thermometer: the remaining log. will be the log. of the refraction at 45°, (in seconds,) and if to this log. be added the log. tang. of any other Z. Dist. there will be given the log. of the refraction (in seconds): this will be a little too big below 70°; but if to the log. last found be added the constant log. 6.8352, and twice the log. tang. of the Z. Dist. the sum will be the log. of seconds to be subtracted from the refraction before found. This rule will give the true refraction within two seconds, as far as 85° from the zenith."

2,9853 and 6,8352 are the logarithms of 966,72 and 0.00068423; the substance therefore of the rule may be more distinctly understood when expressed in the following algebraical formula:

$$\text{Refraction}=\frac{b}{450+t}.966''.72\ \text{tang. Z. D.}\ (1-0.00068423\ \text{tang.}^2\ \text{Z. D.})$$

$$\text{or}\ \frac{b}{450+t}.\ (966''.72\ \text{tang. Z. D.}-0''.66145\ \text{tang.}^3\ \text{Z. D.})$$

If in this expression we take, according to Bradley's standard, $b=29.6$, and $t=50$

$$\text{Refraction}=57''.23\ \text{tang. Z. D}-0''.03916\ \text{tang.}^3\ \text{Z. D.}$$

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[f] This at first sight may seem to differ from Bradley's numbers; but as the relative results would be the same, he omitted all the noughts which strictly belong to the beginning of his values of the tangents of refraction. This was only to save unnecessary trouble in working out his equations. The indices of the logarithms which he has annexed to a, b, c, will shew that for making use of his formula he gave the real value to his quantities.

[g] Mec. Cel. tom. IV. p. 269.

M. Delambre[h] brought out the analogous quantity with Bradley's coefficients,

$$=56''.65 \text{ tang. Z. D} - 0''.04665 \text{ tang.}^3 \text{ Z. D.} + 0''.0000778 \text{ tang.}^5 \text{ Z. D.} - \&\text{c.}$$

It may, however, be reasonably doubted whether Bradley, who was always willing to avoid long decimals, would have taken his mean refraction at 45° to the hundredth parts of seconds; but to avoid this, we must suppose the standard height either of the barometer or of the thermometer to be altered. After a number of trials, therefore, for this purpose, it was found that log. $58'' \times \frac{500}{30}$ is 2.9852767. No other numbers brought out so close a correspondence without introducing fractions; and if these are applied to the coefficient of tang.^{3}, the rule will become

$$\text{Refraction} = \frac{b}{30} \cdot \frac{500}{450+t} \left(58'' \text{ tang. Z. D} - 0''.03967 \text{ tang.}^3 \text{ Z. D.}\right)$$

or (without involving the corrections for the barometer and thermometer)

$$\text{Refraction} = 58'' \text{ tang. Z. D.} - 0''.03967 \text{ tang.}^3 \text{ Z. D.}$$

There are two examples of the application of this rule: one is for the refraction at Z. D. 85°, and is most probably compared with the refraction of Nov. 17, 1750, with which it seems to agree: the other is for the zenith distance of 82° 42′, which Capella was observed to have on the 10th of July, 1750; and as the heights of the barometer and thermometer answer to what they were at that time, there can be little doubt of the star referred to. In this case the result of the computation exceeds that from observation by 1″.3. Bradley however was not yet satisfied without endeavouring to arrive at greater accuracy by a further modification.

In the beginning of the very next page we find

> 98965
> "To the log. A=2.98966, add the log. of b=the height of the barometer, and the log. tangent of the given zenith distance, and from the sum subtract the log. of $450+t$, (t being the degrees of the thermometer in Fahrenheit's scale) then the difference will be the log. of the refraction required, if the zenith distance be less than 60°.
>
> 7.21527
> "But if the zenith distance be greater, then to the given log. C=7.21534 add log. of the height of the barometer, and three times the logarithmic tang. of the given zenith distance. The sum will be the log. of the seconds to be subtracted from the refraction before found. This last part is supposed to be proportional to the elasticity of the air, (or the height of the barometer,) and the first part is supposed proportional to the density of the air."

2.98966 and 7.21534 are the logarithms of 976.47 and 0.001642; the rule, therefore, if reduced to a formula, becomes

$$\text{Refraction} = \frac{b}{450+t}\, 976''.47 \text{ tang. Z. D.} - 0''.001642\, b. \text{ tang.}^3 \text{ Z. D.}$$

If the standard quantities are again taken as 29.6 and 50, the expression becomes

$$\frac{b}{29.6} \cdot \frac{500}{450+t} \cdot 57''.806 \text{ tang. Z. D.} - \frac{b}{29.6} \cdot 0.04860 \text{ tang.}^3 \text{ Z. D.}$$

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[h] Astron. vol. I. p. 303.

but here again is the objection from decimals, and as the constant logarithm 2.98966 only exceeds 2.9853, which was adopted in the previous rule, by 0.00436, it is probable that a further modification of some of the elements has been introduced. There is written immediately under it "$45^{\circ}=61.0''$," but this probably refers to the example rather than to the rule. After varying the quantities in every way, which he was likely to have used, the nearest expression was found from $58''\frac{500}{29.7}$, the logarithm of which is 2.9896416.

These quantities, brought out without any previous view to such a result, happen to have an analogy, which tends much to confirm the probability of their really containing Bradley's numbers; for the first $\left(58.\frac{500}{30}\right)$ only differs from the second $\left(58\frac{500}{29.7}\right)$ by an excess of 0.3 in the standard of the barometer. Assuming this to be the case, the rule will become

$$\text{Refraction}=\frac{b}{29.7}\times\frac{500}{450+t}58''\text{ tang. Z. D.}-\frac{b}{29.7}.0''.048756\text{ tang.}^{3}\text{ Z. D.}$$

In this form there is not only an alteration of the coefficients, but there is an essential variation in another respect. It has been found that there is a part of the refraction, which requires correction for the state of the barometer, while it is quite independent of the thermometer. Bradley has contemplated this very distinction. He has not indeed connected it with the proper function of the zenith distance, $\left(\text{which is }\frac{\text{tang.}}{\text{cos.}^{2}}\right)$ nor does his coefficient agree with that (0.0739) which Dr. Brinkley [i] arrived at from later and more recondite investigations; but in the earlier stage of the inquiry there are not many, to whom *à priori* the possibility of such a distinction could have occurred.

The next step was to the form in which Bradley finally embodied his rule [k].

> "If to the log. 7.5686 be added the log. of the height of the barom. and log. tang. of the apparent zenith distance lessened by three times the refraction [l], (which is given near enough in the common tables of refraction,) and from that sum be subtracted the logarithm of 348, increased by the degree of the thermometer, the residue will be the log. of the correct refraction in minutes, &c. &c."

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[i] Trans. of the Royal Irish Acadamy, vol. XII. p. 99.

[k] On a loose paper at Oxford there is the following memorandum:

$y=393$ Refraction in seconds
Log. $n=8.9820$ $=$log. $m.+$log. $b.+$log. $t.-$log. $(393+a)$
Lon. $m=2.9311$
$b=$barometer subtract. log. $n.\ t^{8}$
$a=$thermometer
$t=$tang. of App. Z. Dist.

It is unnecessary to enter into an analysis of this formula, of which Bradley does not appear to have made any use: it only shews that he was willing to try whether $\frac{8}{3}$ or a somewhat lower power than 3 of his tangent might not give him a more accurate correction.

[l] It is on a vacant space, which appears to have been left over the rule, that we find the remark (quoted, Memoirs, P. lxxxvii.) of the subtracted arc being possibly "in summer $3\frac{1}{2}$, "in winter $2\frac{1}{2}$," and not always three times the refraction. We find, probably upon the same ground, that he at first separated his "morn-"ing" from his "evening refractions."

There is a little omission in wording the latter part of the rule, which might easily have occurred, and was of small importance in a private memorandum of one, who well knew the quantities he had to work with: and this will appear evident from the manner in which the number (7.5686) is found.

Bradley had been calculating refractions for different zenith distances, and among others the observed quantity for 69° is estimated at 2′ 23″. At the end of this part of his work he has fortunately added (though he has not done so in other places) the following memorandum. "These observed refractions are collected from the observa-"tions made July 9th, 1750, (between 17^{h} and $18^{h}\frac{1}{2}$ sid. T.); the latitude being sup-"posed 51° 28′ 39″, and the error of collimation in the zenith − 2″, but at the pole − 3″ + "(and 4″ near the horizon to allow for the motion of the quadrant as the telescope "approaches the zenith.)" Now he did not begin to make regular entries of his observations with Bird's Quadrant till August 1750; but in the book of Observed Refractions there is an entry of several for this 9th of July, 1750; and it is stated that the barometer was at 30.04, and the thermometer 70°. Hence

tang. 0° 2′ 23″	6.8409110	
tang. 69	9.4158226	
	6.4250884	tang. of 54″.89 ref. at 45°
log. 30.04	1.4776999	
	4.9473885	
log. 348 + 70 = 418	2.6211763	
	7.5685648	

This last logarithm is exactly what Bradley has inserted in his rule, and there are numbers on the same page of his paper which leave no doubt, not only of his having arrived at it in this particular manner, but precisely from the refraction here taken of 69°. The process, when reduced to a formula, gives

$$\text{tang. } 54''.89\frac{b}{30.04}\times\frac{418}{348+t}\text{tang. (Z.D.} - 3\,r) = \text{tang. of refraction.}$$

It appears therefore that in the rule we are to read "log. tangent," instead of "log." of refraction. In the same manner over log. 7.5686 he has written "12′ 44″ = 764" of which quantity his number is the logarithmic tangent, although that circumstance is not mentioned. It is not necessary to shew that Bradley understood his own rule, but it may be satisfactory to shew that we have not mistaken it by annexing the following example of his application of it.

" B. 29.9	7.5686 (constant logarithm)
Th. 31	1.4757 (log. 29.9)
75. 30	2.5786 (log. 379 = 348 + 31)
− 12 (three times the refraction)	6.4657
75. 18	0.5811 (tang. 75° 18′)
	7.0468 (tang. 3′ 49″.7)
	3 50 −"

The intimate connexion between this rule and the result of Simpson's investigation most probably did not escape Bradley's observation, but there are no traces of the particular steps by which the transition was made; and it is remarkable, that, with the exception of a few examples to each of the preceding rules, he continues to work on for many subsequent pages by the method, which he had first adopted. He probably trusted most to it for finding the quantities, by which he might establish the coefficients.

The correction for the variation of the barometer created no difficulty, but that for the alteration of temperature seems to have occasioned considerable perplexity. In the first place in which it is mentioned, the denominator has originally been $400+t$, which has been subsequently altered to $450+t$, which is the very quantity which Dr. Brinkley[m] found to answer best at low altitudes for Bradley's form of calculation, and this change was possibly connected with the memorandum which occurs at the top of the next page: "Machin's density is as $\frac{29.25}{432.8}$, and his elasticity as 432.8." This probably was the result of some actual experiments, for in another place we find

$$\text{“ Mr. Mach. greatest}\begin{cases}\text{diff. dens. as}\\ \text{elasticity as”}\end{cases}$$

Unfortunately the numbers have been worn off the corner of the paper, or they might have afforded some means of estimating the care with which the experiments were made. Bradley however in this, as well as in the other parts of the inquiry, did not trust to any reasoning *à priori*. He referred every thing to his observations; and the manner in which he deduced the numbers for the thermometer, as well as the multiple of refraction to be subtracted from the zenith distances, may be seen in the Memoirs[n] prefixed to his Miscellaneous Works.

When calculating from the observations which were made nearly at the same time, he did not at first introduce the corrections for pressure and temperature into each, as all must have been nearly affected by them in the same manner: and it was from a comparison of the results, combined with a mean estimate of the barometer and thermometer for each set, that he calculated the modifications which it was necessary to introduce. This of course produced variations both in the logarithms, which at different times he had to apply to the sine of the zenith distances, as well as to the tangent of the observed refractions. These however he afterward endeavoured to remedy, as will be seen from the numbers, which are copied at the bottom of plate I. They serve likewise to shew (although further evidence is hardly requisite) how much Bradley's calculations were originally founded upon Simpson's method. It is shewn in the Mathematical Dissertations[o], that the sine of the observed distance is to be multiplied by $1-(h-k)$ to find the sine of an angle, the difference between which and the given zenith distance will be to the corresponding refraction as $1:\frac{k}{h-k}$. Now k, as determined in p. 58, is 0.000253 or the refraction at 45°, expressed in parts of the circumference to radius 1; and h, which depends on the height of the atmosphere, is brought out by Simpson (in p. 59) to be 0.001643. Bradley assumes analogous quantities, though not exactly of the same value. His calculation is applied to the observation of α Cygni, which, on the 26th of

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[m] Trans. R. of Irish Acad. vol. XII. p. 104.

[n] P. lxxxviii.

[o] p. 58.

Oct. 1750, will be found to have had the apparent Z. D. 83° 58′ 5″; the correction for collimation would take off 4″, and the odd 1″ is neglected. 7.5825 is some modification of 7.5686, the exact elements of which it may not be easy to determine; but tang. $59''\frac{400}{30}$ will have for its logarithm 7.5814, and probably is not far from the truth. Hence

7.5825
+1.4694 log. 29.47 (barometer)
−2.5807 log. 380.8 (for thermometer 22)
6.4712 log. 0.00029594 tang. 61″ and log. k, which is written against it, evidently means the log. of Simpson's quantity.

4.7015
−2.5739 log. 374.9
7.2754 log. 0.00188539.

The first quantity (4.7015) is the log. of Simpson's h, which is probably taken for some standard number, which differs by 6 from that, for which the logarithm of k was adjusted.

Hence $0.00188539 = h$
$0.00029594 = k$
$0.00158945 = h - k$
$0.99841055 = 1 - (h - k)$ which is Simpson's multiplier.

With these numbers an auxiliary angle is calculated precisely according to Simpson's method:

9.9993091 log. $0.99841 = 1 - (h - k)$
9.9975877 sine 83° 58′ 0″
9.9968968 sine 83 9 31½
48 28¼

The whole may now be completed:

6.4712 was found for the log. of k
−7.2012 log. $0.00158945 = h - k$
9.2700 log. $\frac{k}{h-k}$
8.1492 tang. of the auxiliary angle 48′ 28″
7.4192 tang. 9′ 1″½ = refraction at Z. D. 83° 58′

The use, which Bradley therefore made of what he called z and x[p], are in this instance brought out exactly similar to the logarithms of Simpson's $(1-(h-k))$ and $\frac{k}{h-k}$. Though no instance has been observed among the earlier calculations of his making specific allowances for the barometer and thermometer in settling the logarithm, which was to be added to sines of his zenith distances, it is by no means improbable that some rough estimate determined him to increase or diminish the quantities with which he began his trials[q].

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[p] P. 7.

[q] For the observed refraction we have the following notice annexed to the second determination of the latitude, as quoted by Dr. Mas-

He was not a man to employ himself without consideration of the means most useful for the end which he had in view.

In estimating his temperature, Bradley at first appears to have depended on his thermometer, which was in the open air. This is evident from a memorandum in the beginning of his calculations. It says, " that in the hot weather, about the middle of July, " the thermometer by the clock stood at 75;" and it goes on to speak of " one out of " doors," which was considered to " have been about 80;" the barometer was about 30.00 at that time: hence it adds, " the density of the atmosphere was as $\frac{30.00}{450+80}$." In the same manner the height of the thermometer in doors is not annexed to any of the observations with Bird's quadrant before the 11th of Dec. 1750, although the instrument had been expressly hung on the western side of the pier with a view to making observations with it for determining the latitude and the laws of refraction. From what Mason says[r], it seems, however, probable, that Bradley afterwards used the mean of the thermometers within and without; and this agrees with the entries in the book of " Observed Refractions." To each of the observations in 1750, several of which are used, there is but one notice of a thermometrical height; and by a comparison with the observation-book, it is clear that this was the height out of doors. But to the observations of 1751 and 1752 there are annexed the heights of the mercury both within and without. In one instance the calculated refractions are set down opposite to those which were observed; but this only occurs for Nov. 17, 1750, and consequently gives no means of examining the share, to which the heights within doors were afterwards admitted, in framing the calculation.

Bradley's thermometers have often been inquired for; but they are most probably no longer in existence. All that are now to be found at Greenwich are of a later date. And this very circumstance makes it desirable to correct a mistake (notwithstanding its small amount) which has occurred with respect to one of those which he used.

When Dr. Hornsby published the Greenwich Observations, he introduced two separate columns into each page for the quadrant, and inserted in them the heights of the thermometers within and without. The manner, as will be presently explained, in which the entries were originally made was somewhat different; and to accommodate the text to the alteration, he has varied the arrangement of a notice which occurs in 1750. This in itself would hardly be worth mentioning, if he had not made a little oversight in reading the numbers which it contains.

Between the 21st and 24th of October, Bradley has entered the following memorandum in the north quadrant book: " The thermometer here noted was made by Mr. " Sisson. Oct. 22, I immersed it in snow, and found the mercury sunk only to 33—or " 33, so that it stands near 1° too high. That made by Bewley sunk exactly at the " same time and place to 32. Subtract 1° from Sisson's to have the true number."...This was originally " 33— or 32½, so that it stands near ¾° too high;" but 32 has been evi-

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kelyne: " Mem. Assume a mean refraction, and " correct the observations thereby from the bar. " and ther. both at the pole and equator, and " thence may be collected the error of the as- " sumption."

[r] Memoirs, P. lxxxix.

dently altered to 33, and the fraction ½ has been scratched out, although imperfectly. Now Bradley's own description is very clear. The mercury sunk a little below 33, and he at first thought that the heights which it indicated might be considered at 0,75° too great; but he afterwards saw reason to think that the depression below 33 did not amount even to 0°.25; and as it seemed not worth while to take so small a difference into account, he was induced to call the reduction an even quantity of 1°. On the other hand, " 33— or 33½," as it stands in the printed text, will infer a doubt to the extent of more than 0°.5, which could hardly have occurred to so careful an observer[r]. Accordingly M. Bessel[s] very justly concludes, that the thermometer " temperaturam nivis liquentis in 33°.25 indicavit," whereas the 33° was the very utmost height for Bradley's thermometer out of doors; and there appears to have been neither excess nor deficiency in that which he kept within.

On the 11th of Dec. 1750, we find in the printed text a note that " the thermometer placed within was made by Bewley," with a reference to the memorandum in the preceding October. This is all very clear, and suits the form given to the publication better than the exact entry as it stood in the original book. In the thermometer column we there find on the day which has been stated, "Th. 45 S.;" and after the name of the pole star is added, "Th. 46. Bewley." Under the whole Bradley has written, "Th. "S. is without doors, and Th. B. is within doors." In the MS. "Th. S." continues constantly to occupy the thermometer-column, which corroborates the idea of Bradley's having originally considered it to be the fundamental quantity; and "Th. B." is almost always annexed as an addition after the name of the star to which it refers. The designations of "Th. S." and "Th. B." are continued for the subsequent quadrant-observations, and are used likewise for the "Observed Refractions." It would have been very inconvenient to have retained this form in the printed text, and much trouble was saved by the alteration to readers, who must otherwise have been continually under the necessity of recollecting the position of each instrument; but the entries in the manuscript lead us to some particulars of which we might otherwise have found no indication. In the calculation book we had the mention of a thermometer by the clock; and it is not improbable that Bewley's is here referred to: it is possible that it was fixed in the clock-case. Again, the same descriptions of "Th. S." and "Th. B." are used in the south as well as in the north quadrant book. It is clear, therefore, that the temperatures were entered from the very same thermometers for the observations, which were made with both the instruments.

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[r] M. Deluc (Mod. de l'Atm. vol. ii. §. 438. h.) points out particularly that melting snow is an accurate measure of the freezing point, and, although Bradley does not specifically mention it, we may fairly infer, with M. Bessel, that the snow was in that state. If it had been cooled down below that temperature, the mercury could not be supposed to have stood so high in it, without Sisson's having erred, more than can well be imagined, in his graduation. But there is no occasion for recourse to any original error. If it was not new, it may have undergone a material alteration. M. Flaugergues detected that the freezing point in mercurial thermometers is subject to a rise, which gradually advances for some considerable time after their original construction. Bradley had a thermometer of Bird's, (Memoirs, P. lxxiv.) and it is remarkable that he did not use it for his refractions. There can be no doubt of its having been constructed with all the peculiar care and accuracy of that great artist, and he seems to have paid particular attention to his graduation. Lord Macclesfield made the following entry in his observation-book at Shirburn: " 1756, March 23. 0. 15. Clouds with much snow all the morning till just now. I this morning tried Bewley's thermometers in thawing snow. That which stands without doors, and is Fahrenheit's scale, stood ¼ of a degree above the freezing point. That which stands just within the door, and is D'Lisle's scale, stood ⅓ a degree above the freezing point. I afterwards tried Mr. Bird's large thermometer, which hangs near the clock, and in the thawing snow it stood precisely at the freezing point." In this case, however, the thermometer might not have been made long enough to have undergone any alteration. That which was constructed by the same hand for Greenwich was broken a short time after Dr. Maskelyne's death. There is, however, one of Bird's thermometers in the Observatory at Oxford. This has been immersed to the top of the mercury in pounded ice, which was suffered to melt slowly in a room where the temperature of the air was between 34° and 35°. The vessel of ice was sufficiently large, and the experiment was continued for a considerable interval; but the mercury stood steadily, and as precisely as possible, at 32° of the scale, which is about 0.1 or 0.2 of a division below the mark which the maker had filed on the tube for the fixed point. From a letter of Bird's to Dr. Hornsby it is probable that this thermometer was made in 1772; but as Bird died in 1776, it must have been constructed more than half a century. M. Flaugergues attributes the alteration to a contraction of the bulb from the pressure of the external air. How far the strength of the Oxford thermometer may have been calculated to resist such an effect, may be estimated from its dimensions: the tube on the outside is 0.17 of an inch in diameter, and the end is blown out into a bulb whose diameter is 0.52.

[s] Fund. Ast. p. 28.

ACCOUNT

OF

HARRIOT'S ASTRONOMICAL PAPERS.

AT the death of the celebrated Thomas Harriot in 1621, his manuscripts became the property of Henry, earl of Northumberland, in whom he had found a most munificent patron during the latter part of his life. The male line of the ancient Percies ended with the earl's grandson Joceline, from whose daughter the earls of Egremont are descended. By this means most of the manuscripts passed into their possession [a]. The value and curiosity of these papers were known, and Count de Bruhl pointed [b] them out particularly, in 1784, to the notice of the Baron de Zach, who immediately called public attention to the subject [c], and formed a plan for collecting and printing a new edition of Harriot's works. In the astronomical ephemeris of the Royal Academy of Berlin for 1788 [d], he inserted a general account of the papers, dwelling more particularly on the astronomical observations which they contained. This, having been translated, was separately printed for circulation in England, and a copy has been subjoined to these remarks [e]. I shall have such constant occasion to consider the several parts of it, that it seemed endless to note them: when any expression therefore of the Baron's is quoted, without a distinct reference, it may be generally understood to be taken from this document. It seems to have been drawn up while he was yet uncertain of finding any one who would engage in the heavy expense of his intended publication; but in 1786 he applied to the university of Oxford for assistance in printing it. This was immediately acceded to, and in April of the same year he wrote a long letter, containing details of his plan, to the delegates of the press[f]. He promised a life of Harriot after the manner of Gassendi, which, with the original observations of comets that had been seen in 1607 and 1618[g], was to make up the first volume of his intended work. On the receipt of this specification an order was made for the printing to proceed as soon as the editor was ready, but there is no record of his having advanced any further in the undertaking. He was indeed appointed in 1786 to the care of the duke of Saxe Gotha's observatory at Seeberg; but this had taken place previous to his application to the university. However, in 1793 he inserted in the Supp. to Bode's Jahrbuch the observations of the comets of 1607 and 1618. These were accompanied by notes, which seem to indicate that they had been drawn up with a view to his intended publication at Oxford; and if so, they fix the time when he had abandoned this intention. I am inclined to believe, though I have no direct proof it, that by lord Egremont's indulgence a portion of the papers were all this time in his keeping; for in May 1794 he transmitted to Oxford, by bishop Cleaver, then principal of Brazennose college, a certain number of the manuscripts, though without any of the apparatus, which he was to have drawn up. It is now clear, though it does not appear to have been then known, that this was only a small part of the whole; and as it will be seen that such a selection had not been made as to render the several parts complete in themselves, it seems most probable that they had been taken out at first for use in the conduct of his work. It may easily be understood that the Baron might not be very scrupulous in troubling himself at once to take every thing, which he might require, when a further revision, as he went on, would enable him to complete what he might find to have been omitted: but it can hardly be conceived, that with the whole collection before him, he would have selected what he intended for Oxford in such a manner as to leave several essential parts either imperfect or unnoticed.

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[a] In the Savilian Library there is a copy of the Artis Analyticæ Praxis, which once belonged to Sir Charles Cavendish, of whose conversation with Roberval, Wallis (Opera, vol. II. p. 204) tells the remarkable anecdote which has so often been repeated. The book was Wallis's, and he has written several particulars on the fly leaf, which he concludes by saying that Harriot had left many other "worthy " pieces; . . .some of them I have seen. But in " whose hands they now are, or whether they " be since perished, I cannot tell (March 27, " 1677.)" The papers here alluded to may have been those which were in Dr. Pell's possession; for there are some MSS. of Harriot's in the British Museum, which seem formerly to have belonged to him, (6001, 2, 6083 Harl.). Lord chancellor Clarendon in 1662 had " some " considerable papers of Mr. Harriot," which are said to contain, among other things, " considerable observations on the weather." Birch's Hist. of the Royal Society, vol. I. pp. 120. 126. 309.

[b] See note A at the end of this account.

[c] The Baron says (Corr. Astron. vol. VII. p. 105) that he printed a little dissertation at Oxford about 1784, which gave an account of the contents of these papers, but I have never been fortunate enough to meet with it.

[d] Printed about 1785.

[e] See note C. This translation has not the appearance of having been printed at Oxford, and therefore is probably not the dissertation mentioned in note [c].

However this may be, the business had now assumed a totally different character. The delegates of the press had no longer to deal with an editor whom they had undertaken to assist in the publication of his work, but whatever was done with the papers must have been executed entirely on their own responsibility. It became necessary to inquire further, and ascertain the nature of the materials which had been put into their hands. It was therefore determined to obtain the opinion of some competent judge; and Dr. Robertson, then Savilian professor of geometry, having reported[h] that it would not be advisable to proceed with the proposed publication, the papers were, after some accidental delays, restored in 1799 to the earl of Egremont.

This is the plain state of the case; but a very different version of it has been circulated for many years. Dr. Hutton in his Dictionary (art. Harriot) has reprinted the translation of the baron's communication to the Berlin Ephemeris, and has added[i] [1796] that the papers were in a train for publication, having been given on that condition to the university of Oxford; the baron repeats this story of the gift[k], and Dr. Hutton retained it in the second edition of his Dictionary, which was published in 1815. The general impression has therefore been, that the baron having discovered these papers, lord Egremont made at once a present of them to the university, in order that they might be printed, and that to the injury of science they have been kept at Oxford in obscurity, without any regard to the condition on which they were accepted. This certainly is most erroneous. There was no call on the noble owner to give up his property in the papers, and it was most liberal in him to suffer the baron de Zach to transfer what he had kept under his own consideration for ten years, and submit any of the manuscripts to the university. There can likewise be no doubt that in the same spirit of liberality he would immediately have permitted the whole collection to have been used for the same purpose; but it is equally certain that only a small part was ever at Oxford. This portion, after it was returned to his lordship, was carefully preserved at Petworth, and answers so exactly to the description which is given in Dr. Robertson's reports, that no doubt can exist of its identity. The larger quantity, which has before been alluded to, (on a rough estimate about seven parts in eight of the whole,) remained at lord Egremont's house in London, and were given by him in 1810 to the British Museum. These of course have been examined. Several important documents have been found among them, but in occasional and short visits to the metropolis it was impossible for me to study the contents of all with minuteness. To follow out such an inquiry it is necessary to compare the mutual bearing of the several parts, when the particular topics that they embrace are each fresh in the recollection; and this could only be done by one, who could possess the uninterrupted opportunity of referring to every thing exactly as the doubts or their explanations happened to occur. Lord Egremont's indulgence has however enabled me to follow out one part of the subject with tolerable completeness. The baron de Zach's first object seems to have been the exhibition of Harriot in the new light of an astronomer; this was probably the reason for his having taken out and sent to Oxford the most important papers relative to that point. It will be seen that he omitted some which are necessary to the right understanding of Harriot's claims; but there seems to be no other obvious way of accounting for his having communicated so very little comparatively upon other subjects.

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[f] See note B.

[g] Whenever this date is mentioned, it must be understood with reference to the third comet of that year: Pingré Cometographie, vol. II. p. 8. Halley's comet is in the same manner intended to be designated when that of 1607 is referred to.

[h] See note D for these reports.

The baron, in his account of Harriot's observations, has not arranged them chronologically, which is of the first importance for the consideration of claims to discovery, and with this view they must be referred to five distinct objects. I. Halley's comet, when it appeared in 1607; II. the moon; III. the satellites of Jupiter; IV. the spots on the sun; and V. the comet of 1618.

Of I. and V. an accurate opinion may be formed from what has been printed in the appendix to Bradley's works[l], combined with the further remarks which will be hereafter submitted to the reader, when we come to consider Harriot's view of the orbits of comets.

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[i] See note E.

[k] Corr. Astron. vol. VII. p. 105.

[l] It may be right to correct a mistake of "ceruso" for "cerusa," in P. 520. "Cerusa scripta" is indeed not a very precise expression for what it was intended to describe; but the

II. The observations of the moon contain one as early as July 1609. According to professor Moll's curious and valuable account [m], telescopes had been made in Holland in the latter part of the preceding year; but Galileo did not construct one before May 1609 [n]. It is probable, therefore, that Harriot had his directly from the first inventor. No object was more likely to have drawn early attention than the moon; but the drawing made of what presented itself was extremely rough, the moon was "5 days old," and the edge of its illuminated disc is serrated at random, with only an attempt in one place to make some little of an outline. Subsequent observations made between July 17 and Sept. 15, 1610, present drawings of the moon at different ages; but the Sidereus Nuncius had then been published, and it was probably in pursuing Galileo's discoveries that the following memorandum was subsequently entered on the 23rd of October:—"I "observed that the line of division betwixt the light and the shadows passed by the "outward side of Caspian, and it shewed mountenous. By this observation it may "appeare how farre it is from the outmost perifery of the moone."

One of the objects to which Harriot seems to have paid particular attention, was the endeavour to determine by observation the time when the moon was dichotomised. This was possibly with a view to the method suggested by Aristarchus for finding the distance of the sun from the earth; but nothing like precise certainty was obtained in the result. Thus we find on April 9, 1611:

"10^h 5′ not yet a right line.
10 20 very nere, but yet not perfect.
10 45 yet doubtful to be perfect.
10 52 as I judge now unsensibly a right line, one accidentall rag or two at and nere the lower corner being abstracted. but others say not yet perfect.
11 15 yet a right line and not contrary. but if not right rather wanting by the lower corner.
11 30 yet continuing. others say wanting.
12 0 } all say it is a right line. At 12^h.0 I described the appearance as soon as I could
12 15 } by my instrument of $\frac{32}{1}$."

The observation is dated at Syon, and Harriot annexes the following memorandum: "Sir Nicholas Sanders [o] and Christopher were with me, and also observed in my "garret."

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baron has used it again in another place. Harriot's observations of both the comets were originally noted down with a black lead pencil, and the writing has been traced over with ink. This is what is alluded to in the memorandum where the correction has just been pointed out, and at the end of the whole we find again, in the baron's handwriting, "N.B. Hæc cerusa "notata erant, ego attramento παντογραφῇ· punc"tulis notata difficile erant lectu."

[m] Journal of the Royal Institution, vol. I. p. 319.

[n] "Mensibus abhinc decem"—he was writing in March 1610.

[o] Sir Nicholas Sanders was member for Maidstone in 1585, and for the boroughs of Penryn, St. Ives, Helston, and Lestwithiel, in the four last parliaments of queen Elizabeth. He appears to have been knighted on the accession of James I. and was member for Gat-

There are two drawings of the full moon, with a table of references noting the relative situations of the principal points on them. They do not differ from each other so as to shew any effect of libration, and the figure given to the less brilliant parts is of course only a first approximation; but it must have required considerable time and attention to construct what possibly were the very first maps of the kind which were ever attempted to be drawn.

The baron de Zach mentions "some eclipses," and of course a careful search was made for them. There was one half sheet of paper, on which circles had been drawn to represent the phenomenon, when it was either annular or extending to a certain number of digits; and there was another, on which was inscribed "anno 1601, Dec. 14, eclipsis "⊙;" but there is also written on it "Tycho, pag. 132;" and in that very page of the Progymnasmata the calculation of it will be found; but there was no observation even of T. Brahe himself, for he died in October 1601. Harriot has examined this calculation; but no record whatever was found of any eclipse either of the sun or moon which he had observed himself.

III. By far the most curious and interesting part of the astronomical papers are connected with the early observations of Jupiter's satellites. The records which we have of Harriot's labours on this point consist in his drawings of the configurations which he observed, and the calculations which he made to determine the revolutions of these newly discovered bodies.

There are fourteen half sheets of foolscap filled with the representation of the appearances which he noticed. One of these contains the rough entry of a few of the earlier observations, and the other thirteen are filled with more regular delineations. The whole are clearly written, and a fair estimate of them may be formed from the facsimile (in plate II. Supp.) The upper part contains the rough entry of his first observation, and the lower part of the plate represents the beginning of the fair copies which he afterwards wrote out. Such a specimen as this seemed on many accounts to be more satisfactory than any attempt to describe them; but as the handwriting may present some little difficulty, it will not be superfluous to add in this place a copy of the entries which it expresses.

"My first observation & others following of the new found planets about Jupiter.
1610 Syon.
1. Octob. 17 ☿ ho. 12^a, 1^a, 2^a. I saw but one, & that alone.
Blackfriers, London.
2. November 16 ♀ ho. 9^a, 10^a. I saw one fayre 9′ or 10′ above, and sometimes I thought I saw an other very small betwixt them 3′ or 4′ a ♃.
London.
3. November 19 ☽ ho. 9^a one under fayre.
Syon.
4. November 28 ☿ ho. 9^a one under fayre.

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ton in 1603. From the frequent appearance of his name in committees he must have taken a very active part in the business of the house.

5. November 30 ♀ ho. 9^a one above fayre.
6. December 4 ☿ ho. 9^a one under fayre.
7. Decemb. 7 ho. 9^a, $9^a\frac{1}{2}$, I saw but one, & above.
8. Mane ho. 17^a. Two seen on the west side, a little under. Sir W. Lower also saw them here. The nerest fayrest. The farther not well seen within the reach of my instrument of $\frac{20}{1}$ of 14′ dyameter."

Harriot speaks of "the new found planets," and says that in his first observation he "saw but one:" it is evident, therefore, that he knew of there being more, and had looked for them in vain. It is indisputable, likewise, that he did not even succeed so far till October, 1610, nine months after Galileo. The cause of this delay, and the other circumstances connected with it, will be completely made out hereafter. For the present it will be best to confine our attention to the observations themselves. They were made, for the most part, at Syon house, the seat of the earl of Northumberland, on the banks of the Thames near Isleworth; and we find accordingly, that he notes distinctly on Nov. 16, his having removed to London, and on Nov. 28 his having returned to his usual residence. The baron de Zach was clearly mistaken in his opinion of Harriot's having observed at Sion college; but he might naturally be led to this conclusion by finding it said in Antony Wood, that Harriot "for some time lived in Sion coll. near to "London[p]:" the thing however was impossible: as Wood was an antiquary, he must have known that Sion college was not founded till after Harriot's death, and that it was not near, but in London. He was in the habit of writing in abbreviations, and "Sion "ho." was probably mistaken by the printers for "Sion Co." Harriot, from what he says in his observations of the moon, appears to have had an upper room of the mansion especially assigned to him for his astronomical pursuits.

The several observations of Jupiter's satellites are regularly numbered up to 90, which are in a uniform series on to May 28, 1611; there are also three made Oct. 1, Oct. 6, and Dec. 11, of the same year, and six made in January and February 1612. These make together ninety-nine. There are likewise nine from April 25 to May 5, 1611, which are described as having been made "in a gutter of a house" in London, by Christopher, who appears to have been Harriot's regular assistant. These are not numbered in the series, and Harriot shews himself particularly careful to distinguish what he had seen himself, and whether his observations were made alone, or in company with others. Thus in all his first descriptions of Jupiter's satellites he uniformly says, "I saw," till the 14th of March, 1611, when he possibly began to have the assistance of Christopher; and from that time, with comparatively few exceptions, he speaks of what "we saw." Nor is this accidental, for in entering the observations of the solar spots, he had originally written, in 1612, Jan. 6, 19, "I saw," &c. but he afterwards inserted "we" over the lines.

The first time, on which he was able to see the four satellites, was on the night of Dec. 14, 1610, "all clere and seen at once:" he has written over this entry ("omnes

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[p] Athenæ, Oxon. vol. II. p. 303.

"in recta linea cum asello australi. ut proxime)[q]." He has inserted an additional memorandum, in the same place, and says, "hora 8^a circiter the same night ♀ "being ¾ gr. high did eclipse ♄. 2 houres before I found them by my trunks "$\frac{10}{1}$ $\frac{20}{1}$ about 6′ or 7′ asunder. So that I think about ½ an houre after they were "nere centrally conjoined." The square wooden spouts which are used to convey water on the outside of houses are still called by the workmen "trunks," and Junius says in his Etymologicum Anglicanum, "Trunk, sipho, siphon, similiter a Theot "trunc, quod nihil aliud est quam stipes equali foratu in longum excavatus." The name may have obtained at first from the glasses being set in square tubes. Galileo used a leaden pipe (tubum plumbeum)': Sirturus complains (cap. XIX.) that "artifices "& imperiti,...usi sunt tubis ex lamina ferrea & cartaceis," and that "angustum & con- "tinuo æqualem producebant ignorantes visionum & radiorum naturam." This will account for the name of "cylinders," by which we shall also find the instruments designated. The print of Simon Marius in his Mundus Jovialis (1614) represents a telescope lying on the table before him, which is not cylindrical, but larger at the end in which the object glass was inserted than at the other. The perspective is not to be depended on, but the instrument appears to be about 18 or 20 inches in length, and the larger end about an inch in diameter[r]. Of the size of Harriot's we have no precise description, but he mentions $\frac{20}{1}$ and $\frac{10}{1}$, for the magnifying powers of twenty and ten, which belonged to the two, which he refers to as being used for the observations of Jupiter's satellites. Telescopes from the first were applied to terrestrial objects, and therefore most probably were then fitted with concave eye-glasses, analogous to what is commonly called the Galilean construction: Harriot's appear to have been of this kind, for the images which he represents are not inverted. Jupiter was near its opposition in December 1610, it therefore was in the east at night and in the west in the morning. Now on the 17th of December we find the two satellites which were on the west, not only drawn on the right hand of the planet, but declining also downwards to the right, in their really apparent position; whereas, all the preceding observations taken at night exhibit the line of the satellites inclining downwards to the left. By describing them to be "fair," he means of a good size and distinct: thus on Jan. 26, 1611, he says, "I saw three. Two occi- "dentall fayre. The nerest to ♃ the fayrest. That orientall small, but well seen." And the spots on the sun are constantly spoken of as "fayre and black." His great criterion of the clearness of the night and of the effect of his telescope, was the nebula in Cancer. He is constantly referring to it. Thus 1610, Dec. 27, a satellite, which had been marked as fayrest of the whole four in the preceding morning, was not visible; "yet præsepe was wel seen, & by the instrument his starres." On the morning of Jan. 21, 1611, he saw two satellites, one "fayre. The other small, & but sometimes

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[q] Another similar notice occurs in 1611, "Oct. 6. I saw sometimes two, and certainly. "The ayer was variable. one occidentall the "fayrest about 5′ of ♃. The other orientall "about 3′ of. All were in a right line with "cor ♌ and Asellus Australis."

[r] In the engraved title to Hevelius's Selenographia (1647) there is the representation of a telescope, which, when compared with the height of the man who holds it, must be between five and six feet in length, and of a proportionate diameter: it consists of five cylindrical joints, which slide, the smaller into the larger, exactly as they are now constructed.

"seen. præsepe not seen, & the moone not farre off, shining bright;" and on the night of the same day he could distinguish none of the satellites, "starres cleare, præsepe hardly "seen. I saw nothing, though I looked diligently." The same disappointment occurred to him on the 30th of March, "starres cleare, ♃, other starres and præsepe wel seen, "yet no Joviall by any of our instruments."

Estimates are annexed of the apparent distances of the satellites from Jupiter; these are generally given in minutes and half minutes, in one or two instances we find $\frac{3}{4}$ of a minute, and on the morning of Dec. 18, 1610, he has entered that he saw one as he "thought 10″ or 15″ à ♃, hardly seen." He gives his measures therefore from the limb and not from the centre of the planet; and accordingly there is a rough entry of the observation of Nov. 28, 1610, to which is added "$1'\frac{1}{2}$ à ♃, or diameter Jovis "à ♃." This is the same observation, which he has written out as the fourth in the fair copy, and he has there made the distance 2′. In fact his manner of estimating these distances did not admit of certainty to very small quantities. Galileo, in the beginning of his Sidereus Nuncius, explains, that by covering his object glass with plates, which were perforated with circular apertures of different dimensions, "angulos alios atque "alios pluribus paucioribusque minutis subtendentes pro libito constituemus, quorum "ope stellarum intercapedines per aliquot minuta ad invicem dissitarum, citra unius aut "alterius minuti peccatum commode demetiri poterimus." By this means he took his distances to $\frac{1}{2}$ of a minute: he says indeed on the 8th of February, "stella Jovi proxima "illum fere tangebat, distabat enim ab eo min. o. sec. 10 tantum," but this must have been an estimate, not a measure. Harriot's quantities were more roughly taken. He gives no reason to think that he had made any previous experiments like Galileo, who determined the value of his apertures by the length which they included of a line at a given distance. One method which Harriot used has been already mentioned, which was to take the diameter of the planet itself for the standard; and so on the 16th of March 1611 he describes one of the satellites as being "about 5 diameters of," which, on the configuration is called "9′ or 10′." But his most general way depended on the relative distances when compared with the places which the bodies occupied in the field of his telescope. In the observation of the 17th of December, 1610[s], the satellite was said to be "not well seen within the reach of his instrument," the field of which he describes as being 14′ in diameter; and on the 25th of January, 1611, a satellite under similar circumstances is set down at 12′ or 13′ from Jupiter. In both these cases he used his telescope $\frac{20}{1}$, but the diameter of its field was not very precisely determined, for among the observations of the spots on the sun, the following memorandum occurs; 1612, July 16th, "with $\frac{20}{1}$ I see little more than $\frac{1}{3}$ of the diameter of the sonne. That is 11′ or 12′." Such estimates must of course be very loose: in one of the observations of the dichotomised moon we find "anno 1612, October 21 ☿ ho. 6°. post M. The division then "betwixt the light & darknes was in a right line observed by my trunke of $\frac{20}{1}$.... At "that time the rounde valley *a* (fig. 1. plate IV. Sup.) was $\frac{1}{3}$ from the lower point, & "$\frac{2}{3}$ from the higher, because *ab* was the diameter of my instrument, & *ac* semi-"diameter."

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[s] P. 22.

It is clear that such quantities are not to be depended upon. Harriot estimated the diameter of Jupiter at something less than 2′. This is not to be wondered at; neither is it to be attributed to him as a fault. His telescopes probably did not represent the objects with sufficient sharpness for their outlines to be properly seen, and the blaze of light would naturally have increased their apparent magnitudes[t]. It may be said that the distances of the satellites deduced from this standard are relative, and therefore may be corrected accordingly; but we have no very definite estimate of the standard, and there are comparatively few instances in which there is a note of its having been used. When the field of view has been taken for the micrometer, we have no means of making allowance for the errors in it, or to correct the quantities which Harriot has in this manner deduced from his observations. The roughness with which he observed is clear in another circumstance: Galileo discovered the satellites on the 7th of January, 1610, and on the 13th he says, "primum a me quatuor conspectæ fuerunt stellulæ.... tres occi-"dentales & una orientalis: lineam proxime rectam constituebant; media enim occi-"dentalium paululum a recta ad septentrionem versus deflectebat." He afterwards notices repeatedly that a satellite was out of the line; but there is no indication of any thing of the kind in Harriot's text or drawings. On the contrary, the only passage in which any allusion is made to the fact is quoted above[u], and he there mentions them as being "omnes in recta linea."

To have published a collection of such observations could have been of no use, and the expense of engraving all these configurations would have been a waste of money. The papers, however, are of singular curiosity, from containing, in all probability, the very earliest original records in existence of any observations of Jupiter's satellites. They furnish us likewise with several particulars connected with the history of Harriot's observations, which are remarkably interesting; but the account would be very deficient without the explanations that are found in that part of the collection which has been deposited in the British Museum. Much light, for instance, is derived from the following letter:

"To his espesial good friend Mr. Thomas Harriot, at Sion neere London.

"I gave your letter a double welcome both because it came from you and contained newes of that strange nature; although that which I craved, you have deferred till another time. me thinkes my diligent Galileus hath done more in his threefold discoverie than Magellane in openinge the streights to the South Sea or the dutchmen that weare eaten by beares in Nova Zembla. I am sure with more ease and saftie to him selfe & more pleasure to mee. I am so affected with his newes as I wish sommer were past that I mighte observe the phenomenes also. in the moone I had formerlie observed a strange spottednesse al over, but had no conceite that anie parte therof mighte be shadowes; since I have observed three degrees in the darke partes, of which the lighter sorte hath some resemblance of shadinesse but that they grow shorter or longer I cannot yet perceive. ther are three starres in orion below the three in his girdle, so neere togeather as they appeared unto me alwayes like a longe starre, in so much as aboute

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[t] Jupiter's "apparent diameter (measured "equatorially) at his mean distance from the "earth is 36″.74. At its conjunction it is "sometimes only 30″.0; but it increases as "the planet approaches its opposition, when it "sometimes amounts to 45″.88." Mr. Baily's Astronomical Tables and Formulæ, p. 36.

[u] P. 23.

4 yeares since I was a writing you newes out of Cornwall of a new a strange phænomenon but asking some that had better eyes then my selfe they told me, they were three starres lying close togeather in a right line. thes Starres with my Cylinder this last winter I often observed, and it was longe er I beleved that I saw them, they appearinge through the Cylinder so farre & distinctlie asunder that without I can not yet dissever. the discoverie of thes made me then observe the 7 starres also in ♉, which before I alwayes rather beleved to be 7 then ever could number them. through my Cylinder I saw thes also plainelie and far asunder, and more then 7 to, but because I was prejudgd with that number, I beleved not myne eyes nor was carefull to observe hou manie; the next winter now that you have opened mine eyes you shall heare much from me of this argument. of the third and greatest (that I confesse pleased me most) I have least to say; saving that just at the instante that I receaved your letters we Traventane Philosophers were a consideringe of Kepler's[x] reasons by which he indeavers to overthrow Nolanus & Gilberts opinions concerninge the immensitie of the spheare of the starres and that opinion particularlie of Nolanus by which he affirmed that the eye beinge placed in anie parte of the univers the appearance would be still all one as unto us here. when I was a sayinge that although Kepler had sayd somethinge to most that mighte be urged for that opinion of Nolanus, yet of one principall thinge he had not thought; for although it may be true that to the ey placed in anie starre of ♋ the starres in Capricorne will vanish, yet he hath not therfore so soundlie concluded (as he thinkes) that therfore towards that parte of the world ther wilbee a voidnesse or thin scattering of little starres wheras els round about ther will appeare huge starres close thruste togeather: for said I (havinge heard you say often as much) what is in that huge space betweene the starres and Saturne, ther remaine ever fixed infinite numbers which by reason of ther lesser magnitudes doe flie our sighte, which may supplie the appearance to the eye that shalbe placed in ♋. what if aboute ♄.♃.♂ &c. ther move other planets also which appear not. just as I was a saying this comes your letter. which when I had redd, loe, q^{d}. I what I spoke probablie, experience hath made good; so that we both with wonder and delighte fell a consideringe your letter. we are here so on fire with thes things that I must render my request & your promise to send more. of all sortes of thes Cylinders. my man shal deliver you monie for anie charge requisite, and contente your man for his paines and skill. send me so manie as you thinke needfull unto thes observations. in requitall I will send you store of observations. send me also one of Galileus bookes if anie yet be come over and you can get them. Concerning my doubte in Kepler, you see what it is to be so far from you. what troubled me a month you satisfyed in a minute. I have supplied verie fitlie my wante of a spheare, in the desolution of a hogshead, the hopes therof have framed me a verie fine one. I pray also of your leasure answere the other pointes of my last letter concerning Vieta Kepler & your selfe. I have nothinge to presente you in counter, but gratitude with a will in act to be usefull unto you and a power in proxima potentia; which I will not leave also till I have broughte ad actum. if you in the meane time can further it, tell wher in I may doe you service, and see how wholie you shall dispose of me.

"Your most assured and loving
"friend,
"Willm Lowër."

"Tra' venti the longest
day of, 1610."

We shall hereafter have a better opportunity of enlarging on the author of this letter[y], and may therefore at present confine our attention to the contents of it.

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[x] "p. 106, de nova stella Serpentarii."

[y] See note I.

The Sidereus Nuncius contains observations on the inequalities of the moon's surface, on stars which cannot well be separated by the naked eye, and on Jupiter's satellites. These are precisely the "threefold discoverie" alluded to; and it is remarkable that the writer had before noted the singular appearance in the sword of Orion, although neither he nor Galileo were able to distinguish the nebula, which Huyghens afterwards discovered in this part of the constellation. There is something very striking in the simple, but animated account of the news arriving just at the moment when the party were discussing the possibility of secondary bodies moving unseen about the superior planets—an idea which Harriot had thrown out from analogy. From the tenor of the letter it may be concluded, that the fame of Galileo's discoveries had reached England before the work in which he gave an account of them, and that Harriot had derived the information which he sent to his friend from some other quarter. The dedication of the Sidereus Nuncius to Cosmo de' Medici is dated "4 Idus Martii," and by "5 "nonas Maias" Kepler had published his "Dissertatio cum Sidereo Nuncio." Now Harriot was in correspondence with Kepler, and studied his work de Stella Martis with the greatest attention, repeating and often correcting the calculations as he went on. On one sheet of these the following memorandum occurs:

("1610. Oct. 6 differt a calculo pruten 3° gr.
In dissertatione cum nuncio sidereo, p. 5.")

This has been evidently added after the rest was written, and the passage occurs in the given page of the first edition of Kepler's book; it is not improbable therefore that the news reached Harriot through these means before the original narrative had found its way from Italy. It was not much longer, however, in reaching this country: for sir Christopher Heydon, in a letter dated July 6, 1610, tells his friend Camden, "I have "read Galileus, and, to be short, do concur with him in opinion; for his reasons are "demonstrative: and of my own experience, with one of our ordinary trunks, I have told "eleven stars in the Pleiades, whereas no age ever remembers above seven, and one of "these, as Virgil testifieth, not always to be seen[z]." However this may be, it is perfectly clear that Harriot and his friend had been in the habit of using telescopes before the discoveries of Galileo were known to them; and it appears likewise that in 1610 they were manufactured in England. Sir William Lower's request for "more of all sortes of "thes cylinders," seems likewise to imply that they were multiplied at no very expensive rate. It is not easy, upon any other supposition, to account for the requests not being rather directed to the quality than to the quantity of the instruments.

The discoveries of Galileo seem to have been received with enthusiasm. The first edition of Kepler's Dissertation was so soon exhausted, that he published a second in the following year. With his accustomed warmth and eagerness he describes his own ecstasy when the news first reached him from Italy: "Quod cum .... Wackherius ... "de curru mihi ante habitationem meam nunciasset tanta me incessit admiratio absur-

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[z] Camdeni Epistolæ, p. 129. The baron de Zach refers to this passage, (Corr. Astr. vol. VII. p. 123,) but only for the mention of "ordinary trunks."

"dissimi acroamatis consideratione, tanti orti animorum motus (quippe ex inopinato decisa antiqua inter nos liticula) ut ille gaudio, ego rubore, risu uterque ob novitatem "confusi, ille narrando ego audiendo vix sufficeremus." Sir William Lower does not seem to have felt less delight; and we therefore learn with regret that he was disappointed in his expectations of observing. Another of his letters in the British Museum is dated from Traventi, March 4, 1611, in which he says,

> "Concerning the Joviall starres I writte nothinge of them last, because I had nothinge to write. for indeede although both I and the young Philosopher att Haulesbrooke have often and in verie cleare nights (when Præsepe was most plaine to be seene without the cylinder) when wee I say have often diligentlie observed jupiter wee could never see anie thinge. I impute it to the dullnesse of my sighte for onlie with your greate glasse I could se them in London.
>
> "That you have made so manie excellent observations of them I am most glad of. for you have gotten the starte of all in limitinge ther periods.
>
> "ther periods are verie merveilous especiallie that of Jovi proximus whos situation also is no lesse merveilous beinge not one diameter of Jupiter of from him.
>
> "of thes thinges and thos other mirabilia quæ indies invenis I longe to bee with you to discourse of them.
>
> "my wife is well. now you know all my comfortes.
>
> "I have lost my second boy also ....[b]"

In Galileo's first telescope the objects appeared "triplo viciniora noncuplo vero majora," (Sid. Nunc.); and he says, "tandem labori nullo nullisque sumptibus parcens "eo a me deventum est, ut organum mihi construxerim adeo excellens, ut res per ipsum "visæ millies fere majores appareant, ac plusquam in ter decupla ratione viciniores quam "si naturali tantum facultate spectentur:" but he adds, that it was necessary for observations like his own to have at least, "perspicillum exactissimum quod objecta pellucida, "distincta & nulla caligine obducta repræsentet, eademque ad minus secundum quater "centuplam rationem multiplicet; tunc enim illa bis decuplo viciniora commonstrabit." Harriot evidently uses the linear dimensions in marking his magnifying powers, and the "great glass" answers to what Galileo mentions as requisite. In 1611, on the 30th of January, he describes what he saw "with great instrument and small $\frac{20}{1}$ $\frac{10}{1}$." His first observation of the moon was made with $\frac{6}{1}$, but in the following April there is a memorandum on one of the papers containing an observation of the moon, that he then had "$\frac{10}{1}$ two, $\frac{15}{1}$ one, $\frac{32}{1}$ one, $\frac{11}{1}$ one." Indeed there seems to have been considerable difficulty in finding telescopes which would act well. Peyresc was not able to obtain one, with which he could see the satellites of Jupiter, before November 1610[c], although he had endeavoured to procure them from Holland and Italy, as well as from the persons who had already begun to manufacture them at Paris. He likewise applied to his brother at Paris, and "illum sollicitare, varieque urgere nunquam destitit, donec vitra "Telescopica confici curavit, misitque ad usque quadraginta[d]." Harriot, however, saw these satellites sooner, and it was not after all so much the "dulness" of his friend's sight, as the great strength of his own, which gave him the advantage. It will be seen that he observed the sun with his naked eye. For the satellites of Jupiter he frequently used only his less powerful telescope; and it is remarkable that Harriot himself frequently saw three, if not all four of the satellites, whilst his servant Christopher, whom he left in London with his instruments [e], was only once able to see two; although through the whole time he notes the weather, as affording "reasonable," "cleare," "fayre," or "starry nights."

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[b] This refers to the loss of his eldest son, which he had mentioned in a former letter, which will be given hereafter.

[c] Gassendi Opera, vol. V. p. 275.

[d] This agrees with Sir W. Lower's desire to have "more of all sorts of these cylinders." It is possible that in those early times the manufacturers were unable to adjust, or even to

After the detail of these particulars, there can no longer be any doubt of Harriot's having no claims to any [f] competition with Galileo for the discovery of Jupiter's satellites. The Baron de Zach, indeed, seemed latterly inclined to modify his original opinion of Harriot's having been "the first discoverer of these attendants of Jupiter;" but he still thought it "assez vraisemblable que Simon Mayer en Allemagne, Galilée en Italie, "et Harriot en Angleterre, avaient vu, en meme tems et chacun de son coté, les satellites "de Jupiter [g]." He argues, indeed [h], to Harriot's independence of Galileo, from his finding "this mathematician's name never quoted" in the papers. It is not easy to understand this, after what has now been laid before the reader. My principal document, indeed, has been found in the British Museum, and it is possible that the Baron's attention was more exclusively directed to the parts which probably he selected for his own immediate use, and which he afterwards sent to Oxford; but even in these it has been mentioned [i] that reference was made to the "Dissertatio cum sidereo nuncio," and if a more express notice of a name had been required, "Galilæi prosopopœia" might have been seen on the back of the paper which contains the rough notices of the first observations of the satellites.

It is most probable that Harriot lost no time in apprising his friend of what had come to his knowledge from abroad; and although Traventi is in Carmarthenshire, we cannot, after making every allowance for the difficulty in those times of distant communication, suppose that the news reached Harriot before the beginning of June. By this time, however, Jupiter was too near the sun for them to be able to look for his satellites [k]. This is the reason why Lower, with all his zeal and delight, still talks of the winter for his observations, and why Harriot himself did not succeed in making any before the middle of October 1610. All this is perfectly consistent; but it must not be concealed that the Baron de Zach says explicitly, "his first observation of those disco"vered satellites I find to be of January 16, 1610." This is a positive fact, which is perfectly compatible with the planet's situation with regard to the sun; but the truth is that it is founded in mistake. The date, which caught the Baron's eye, belongs not to

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judge of their own work, and that it was necessary to purchase a number for the chance of having some good ones among them.

[e] See P. 22.

[f] See notes A, C.

[g] Corr. Astr. vol. VII. p. 126.

[h] See note C.

[i] P. 27.

[k] It appears from the Ephemerides of Maginus for 1610 that Jupiter in that year was in conjunction on the 13th of June, and in opposition on the 30th of December, O. S. Hence it rose and set not very long before the nebula in Cancer, which accounts for the constant reference to Præsepe made both by Harriot and Sir W. Lower.

an observation of Harriot, but to a table which he had calculated from Galileo's. To establish this point will require some calculation, and a close examination, the details of which would interrupt the present narrative: however, in explaining them, there will be an opportunity of enlarging on several other particulars, and the whole has therefore been subjoined at the end of the present tract[l].

From Sir William Lower's Letter it appears that Harriot began immediately to examine the motions of the satellites. The angular measures of them in their respective distances from Jupiter seem to have been one principal object to which he directed his observations, and the quantities which he deduced were " Semid. orb. 14′, 9′, 6′, 3′$\frac{1}{2}$." Harriot has in several instances set down distances of 12′, 12′ or 13′, and once of 13′, from the limb of the planet. This last estimate was very probably the foundation of his determination for the 4th satellite. 1611. Jan. 25. hor. 6. the distance is marked 12′, or 13′; Jan. 26. hor. 10. 13′, and Jan. 27. hor. 6. again 12′ or 13′; so that if the measure on the 26th be increased by 1′ for the semidiameter of Jupiter, there will be 14′ for what might have been considered as the greatest distance from the centre. This is clearly too large. The quantity when Jupiter is in opposition is about 10′, but great allowance must be made for the circumstances under which the observations were taken, and it must be remembered that, notwithstanding the clearer atmosphere of Italy in which Galileo observed, and the less indefinite manner of his estimating his angles, the distances in the Sidereus Nuncius are still too great[m]. Jupiter had past the opposition before he discovered the satellites in Jan. 1610, and yet he gives us distances of 10′, 11′, and on the 17th Feb. even of 12′. This is likewise the case with Simon Marius, who, in his Mundus Jovialis, makes the semidiameters of the orbits at a mean distance from the earth 13′, 8′, 5′, and 3′; and there is a circumstance connected with these last quantities, which in the present inquiry may not be unworthy of notice. Marius says, " Deprehensum " est a me frequenti, diligenti & diurna observatione Jovem sua diametro in media a " terris elongatione unum minutum circiter subtendere." Now this excess producing a compensation of errors, his semiorbits will come out, in terms of the semidiameter of the planet, 26, 16, 10, and 6.—relative measures, which are not very different from those adopted by Newton in the beginning of the third book of the Principia[n]: but after all, his absolute quantities were in themselves erroneous, and nearly as much so as those of Harriot, who seems from this to have been mistaken rather in the fundamental estimate of 1′, than in his application of it to the relative distances, which he observed.

Galileo watched the continued motions of the satellites, and in more than one instance marks the time when an emersion had just taken place. Harriot, indeed, Dec. 12, 1610, mentions one satellite, and an " other westerly, as I thought, 10″ or 15″ a ♃ " hardly seen :" but this is the only instance in which he notes any appearance even approaching to a conjunction; and he therefore had recourse, in a great measure, to the Sidereus Nuncius for determining the times of revolution. These he brought out 16.66, 7.315, 3.524 days, and 21.24 hours, or $16^{d}\,16^{h}$.; $7^{d}\,7.56^{h}$; $3^{d}\,12.56^{h}$. and

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[l] See note G.

[m] There are two which are set down as being 30′; but that of the 26th of Jan. ought clearly to be 30″, and the other of the 26th of Feb. is possibly a press error of 10′.

[n] 26.63, 15.141, 9.494, 5.965.

21.24^h. It is well known that when Galileo published his Sidereus Nuncius he had not had time to ascertain these particulars, and Sir William Lower[o], in the beginning of March 1611, congratulates his friend Harriot on having "gotten the starte of all in "limitinge ther periods." There is likewise another accidental circumstance which proves the originality, as well as the early date of his calculations. The three first periods are possibly as near the truth as it might at first have been expected that he should have arrived; but by a mistake in the number of revolutions, which he assumed in a given interval for the first satellite, he brought out its time only half of the truth. Now Simon Marius, in his Mundus Jovialis, makes the times $16^d\,18^h\,9'\,15''$; $7^d\,3^h\,56'\,34''$; $3^d\,13^h.\,18'$, and $1^d\,18^h\,28'\,30''$; and it is therefore clear that Harriot must have determined the quantities for himself previous to the publication of this nearer approximation to the truth. It is not improbable, also, that if he had not ascertained, he had at least suspected his error for the 1st sat. at an earlier time; for in a paper entitled "Jovi—primi Motus" he had originally written his quantities as derived from a revolution completed in $21^h.\,24$, but he has added another column, in which he has doubled them all, and written at the bottom "ut periodus $42^h.\,48$." This second column is evidently written subsequently to the rest of the paper, and possibly before 1614[p], when Marius's book came out; because there is no erasure, or any thing to mark that the erroneous numbers are not to be used. The error is the more difficult to understand from Harriot's having well studied the great work of Kepler, in which he had shewn that the squares of the periodic times were as the cubes of the major axes for the primary planets. There was such an obvious analogy likewise, that Galileo, when he was satisfied of the existence of the satellites, expresses it by saying, "in cœlis adesse stellas vagantes circa Jovem, instar Veneris atque Mercurii circa "solem." It may seem wonderful therefore to us that Harriot, if he entertained any doubts between the two periods, did not apply Kepler's law to them as a criterion. That a fair judgment however should be formed on a point like this, we should endeavour to lay aside all considerations but those which were then in existence. The law in question was only ascertained in Harriot's time as a fact, which had been collected from the motions of the planetary bodies; no reason for it was then known, and the astronomer might not be prepared, *à priori*, to argue on the admission of it in a system of secondary planets. The most philosophical method was to ascertain all the facts first from observation, and to inquire afterwards whether the law extended to the new case which then presented itself.

III. Next to Harriot's observations of Jupiter's Satellites, those to which the public attention has been principally called were made by him of the spots on the Sun. They are 200 in number, and are contained in seventy-three half sheets of foolscap, besides three smaller pieces of paper. A facsimile of the first of these half sheets will be found in Plate III. Sup., and the contents of it are as follow:

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[o] See P. 28.

[p] Lalande (Bibliogr. Astron. p. 158.) mentions "Simon Mayer oder Marius Frankischer "kalender, oder Practica;" which was published in 1612. He says that the author had already calculated the revolution of the "deux "extremes" of Jupiter's Satellites. By this description the third and fourth are probably meant: there is no evidence of Harriot's having seen the book, but he certainly was acquainted with the Mundus Jovialis, for there are calculations made with the tables contained in that work.

"1610 Syon December 8^d mane ♄. The altitude of sonne being 7 or 8 degrees. It being a frost & a mist I saw the sonne in this manner.

"Instrument $\frac{10}{1}$ B. I saw it twise or thrise. once with the right ey & other time with the left. In the space of a minute time. after the sonne was to clear.

"$\frac{1610}{1611}$ Syon January 19 ♄ a notable mist. I observed diligently at sondry times when it was fit. I saw nothing but the cleare sonne both with right and left ey.

"Syon 1611 Decemb. 1 ☉ mane ho. 10 0 per horologium solare

"I saw three blacke spots in such order as is here expressed as nere as I could judge. observed by $\frac{10}{1}$.

"S^r W. Lower with $\overline{\text{Chr}}$ also saw the same. at sundry times all three seen & observed at once for halfe an houre space. at which time and all the morning before it was misty.

"The greatest was that which was most oriental appearing somewhat ragged & was of apparent angle about 2′, the other two were nere of one bignes: & of 1′ magnitude or there aboutes."

The papers are in good preservation, the writing is sufficiently clear, and the drawings are well defined. The Baron de Zach says, that he "compared the corresponding ones "with these observed by Galileo, between which he found an exact agreement." This however is more than can be justly asserted of them; it should be rather said that there is that degree of general resemblance, which marks the identity of the objects that were observed. Harriot's drawings were made with a pen and common ink, which did not admit of shading. Indeed he repeatedly mentions that spots, and parts of spots were "dim," or not so dark as the rest; and he endeavours to distinguish this in his designs, but in no way that approaches to the real appearances which present themselves on the sun. The shapes likewise, when he attempts to imitate them, are very roughly delineated, nor can we always distinguish the narrower figure[q] which the spots assume, as they are nearer to the sun's limb. His drawings are certainly much superior to the coarse wooden cuts inserted in the collection of Galileo's works; but in the original edition of the Istoria e dimostrazioni intorno alle Macchie solari (Roma 1613) the phenomena are represented on copper plates, with great attention to their respective forms, and to the penumbra by which they may have been accompanied. To enable any one to judge of the fact for himself, a copy of Harriot's observation, June 26, 1612, is given in Plate IV. Sup. This particular instance was fixed upon, because it seems to have been especially noticed by the Baron de Zach, and the reference to Galileo on the side of it is in his handwriting. The representation for the 6th July (N. S.), will be found in p. 91. of the Istoria. The Baron has likewise noted p. 90, because this observation having been made about 6 o'clock in the morning of the 26th, answers to an earlier hour than that for which we have the drawing of Galileo for the 6th of July. In fact, Harriot's must be considered as a representation of the intermediate state between those in p. 90, and 91. He has left us one for June 25 which gives very nearly the position of the spots as they appear in p. 90; but it would not be a fair specimen, because it was made in a hurry, as will be seen from the memorandum on the drawing for the 26th. It appeared better therefore to add the observation of June 11, which seems to have been more nearly contemporaneous with Galileo's of the 21st[r]. (p. 77.)

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[q] He had not however failed to notice the variation; 1611, Dec. 15. "they generally "seeme bigger, being nere the middle then the "sides."

Independent of the materials with which Harriot made his drawing, there was an essential source of inferiority from his method of observation. Scheiner, in his first letter to Velserus, says, " Sol ubicunque apertus per tubum, præter convexum & concavum vitrum, " vitro insuper utrimque plano cœruleo aut viridi debite crasso munitum, ea ex parte " qua admovetur oculus, indemnes adversos servat oculos vel in ipsa meridie[s];" and in the tract, " de Maculis solaribus," which he published eight months afterwards, he mentions[t] the more easy method of observing the spots by receiving the sun's light through a small aperture, " perpendiculariter in chartam mundam." Galileo, at the suggestion of his pupil Benedetto Castelli[u], admitted the light not merely through the opening, but through a telescope, which was directed to the sun. By this means he was able to distinguish even the smaller spots, on a paper which was held perpendicular to the axis of the cone of rays. It appears from the Rosa Ursina, that Scheiner also adopted this improvement, but Harriot did not take advantage of any such assistance. There is not the slightest allusion to any thing like the camera obscura, and although he speaks in one place (1611, Dec. 13.) of observing " thorough my coloured glasses," he appears afterwards to have constantly laid aside the use of them.

Greaves says[x], that in measuring the diameter of the sun he hurt his sight, " insomuch that for some days after, to that eye, with which I observed, there appeared, as " it were, a company of crows flying together in the air at a good distance. At the first " I did verily believe I saw a company of crows flying in the air." Lalande[y] quotes from Scheiner, that the first inventor of telescopes lost his life from an inflammation of the eye, brought on by looking at the sun; he then mentions the blindness which came upon Galileo and Cassini in their old age, and warns astronomers against the fatal negligence of necessary precautions in observing. But it must have been something very different from negligence which could have operated so widely, and upon such men as we have just mentioned. Hooke says, " as to the coloured glasses I cannot at all approve " of them, because they tinge the rays into the same colour, and consequently take off " the truth of the appearance as to colour; besides, it superinduces a haziness and dim- " ness upon the figure, so that it does not appear sharp and distinct[z]." The first of these objections is not of any very great importance, but the second suggests a reason why Harriot and others may have abandoned the use of darkened glasses. If the texture of the glass were not uniform, if the colouring matter were not clearly and equally distributed through it, if the surfaces were not worked to good planes, or if the opposite sides were not parallel, the image seen through the glass would not be accurately represented. In the early state of the arts these evils were very likely to occur, and it can be easily understood that such instruments might be well thrown aside by those, whose ardour of observation overpowered the consideration of personal ease and safety. It might be from reasons of this kind that Harriot was willing to observe with his naked eye, and for that purpose took the opportunity of misty weather, or thin clouds, which might diminish the intensity of the sun's light. He preferred the latter; for, he says, (1611, Dec. 14,) "The sonne was somewhat to cleare. There being no "cloudes but only thick ayer. And then the spots seme lesse and are not also so well "seen as otherwise." In this manner the very last observations, which are recorded (for the 14th and 18th of January, 1613,) were made when there were "thick ayer & thin "cloudes," "a thin mist and thin cloudes." But he sometimes ventured to trust the strength of his sight without any of these protections. He says, (Dec. 21, 1611,) "afternoon at $2\frac{1}{4}$ when the ⊙ had no cloudes nor mist, I saw the most oriental fayrest "& the 3 orientall of the 4 in a rank;" and, (Feb. 17, 1612,) "all the sky being cleare, "and the sonne, I saw the great spot with $\frac{10}{T}$ and $\frac{20}{T}$, but no more[a], my sight was after "dim for an houre."

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[r] The memoranda annexed to the observations may not be easily legible to every one; they are as follow:

" London ♀ June 26 ho. 5½.6. mane. Thicke " ayer & thin clouds. 19 spots great & small. " places better conjectured than the day before " having had more time. the 4 small spots " not so blacke as the rest.

" ♃ June 11 ♃. ho. 3½ p. m. Thorogh thin " cloudes 11 spots. now 5 in the cluster fayre. " 6 more, of which the 2 southermost placed " by conjecture. having not time inough to " examine there places but not much amisse."

[s] p. 4.

[t] p. 40.

[u] Opere (Padoue, 1744.) vol. II. p. 109.

[x] Misc. Works, vol. II. p. 508.

[y] Astron. §. 2474.

[z] Description of Helioscopes, p. 3.

The two magnifying powers which are here noted, are the same which were used for the satellites of Jupiter, but others are mentioned in the observations of the solar spots; and he says, (Dec. 13, 1611,) "it seemed to me that a greate glasse would make them "appeare divided and more dissevered:" accordingly he used not only $\frac{8}{T}$, and[b] two $\frac{10}{T}$ as well as $\frac{20}{T}$, but $\frac{15}{T}$, $\frac{30}{T}$, and $\frac{50}{T}$[c]. But the advantages which might have been derived from powerful instruments, were more than counterbalanced by the manner of using them. When the difficulties are considered, which must necessarily occur in hastily looking at the sun, and then, with the eyes dazzled by its overpowering light, in laying down on paper the figure and position of the spots which may have been seen, it is no small praise for Harriot to have drawn them near the truth; and under such circumstances it is not wonderful that his designs should be very inferior to Galileo's. The sun's strongest light, and the clearest air, were best for shewing the picture in a darkened room; but on many occasions the smaller spots must have escaped observation when obscured by the very state of the atmosphere, which was required for a direct view of the solar disc. Thus on the 5th of Dec. 1611, there is a memorandum that "$\overline{\text{Chr}}$ "saw one more orientall sometimes as he sayd and is noted;" and on the 11th of the same month, "I thought that sometimes I saw the two which were seen on Munday "nere the middle but obscurely and for no time therefore could not note there posi- "tion." A deliberate examination with the naked eye must often have been impossible:

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[a] He had seen eight spots in the morning.

[b] This will account for his distinguishing one of them as $\frac{10}{T}$ B. (P. 32 and 35.)

[c] $\frac{30}{T}$ is spoken of on Jan. 20, and $\frac{50}{T}$ on Jan. 31, 1612. But there is no notice of his having used these more powerful instruments for the important purpose of observing Jupiter's satellites, although he continued his attention of them till the 26th of February of the same year.

thus, (Dec. 15, 1611,) "I suspected some ragges on the north side of the spot. which in "some more convenient light an other time may be discovered. The position as I con-"jectured having but little time is as in the 2 figure." But when every thing was most favourable, Harriot had no certain means of determining either the size or situation of what he observed: the field even of the smaller telescope, which he commonly used, did not take in the whole of the sun's disc, for he says, (Jan. 11, 1612,) "one of the spots "was in the verticall the semidiameter of my instrument B $\frac{1}{1}^{\circ}$ from the upper edge and "the whole diameter from the lower." The spot itself is placed accordingly on the perpendicular diameter of the circular figure which is annexed; but we find a notice in the margin, "B 30′," which, if it is meant to describe the angular value of the field of view, will be another instance, in addition to those already adduced, (P. 24,) of the little precision with which these estimates were made.

There is much obscurity with respect to the precise time when the first observations were made with telescopes of the spots on the sun. The earliest account which is known to have been printed of them is in the Tract of Jo. Fabricius Phrygius, entitled, "De "Maculis in sole observatis," which was published at Witteberg in 1611, and the dedication of which, "Ennoni totius orientalis Phrysiæ comiti," is dated "Id. Junii" of the same year. In the Memoirs of the Royal Academy of Sciences of Paris for 1778[d], Lalande gives the particulars of this little work, and an account of what it contains on the present subject. This clearly exhibits the most important facts, and saves the tædium of reading through much which is not possessed of equal interest; but it conveys no idea of the honesty with which the original is drawn up. Fabricius betrays no eagerness for putting himself ostentatiously forward, but rather deprecates the severity of any remarks to which he may be exposed. He is careful not to overstate any thing: in speaking of Galileo's discovery of Jupiter's satellites, "Quos non tantum multis visos, "sed etiam observatos aliquoties constat," he says, "verum subtilitate hic opus est sum-"ma, ne et oculus atque instrumentum nobis imponant, alioquin proclivis hic lapsus "est et facilis eorum persuasio, ob infinitas affixorum siderum luculas in planetarum "confiniis constipatas. Equidem si mihi ipsi fido, eos mihi visos non ausim simpliciter "negare. Verum quia instrumenti mei subtilitas non omnino attingebat tenuitatem "illarum stellarum, malo ista observationis fide carere, quam obscuræ licet aliquoties "repetitæ animadversionis testimonio temere famam augere novæ observationis." In the same spirit he speaks of the motion of the spots round the body of the sun[e]: "Id "vero non ex unica saltem revolutione persuadere mihi nec potui nec volui, ne me at-

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[d] P. 393.

[e] The method of observing the sun without exposing his sight to the perils of direct vision, occurred immediately to Fabricius; "Noluimus deinde oculos fatigare amplius isto "onere, sed cepimus consilium, ut si quo com-"pendio eos in posterum levare possemus, id "minime prætermitterimus. Cogitavimus igi-"tur de radiis solis per angustum foramen "intromittendis, & in obscura, clausis fenes-"tris camera observandis. Notum enim est "ex Opticis, quod omnia, quæ foris sunt & "aguntur, in tenebroso cubiculo possunt re-"præsentari, aperto solum angusto quodam "foramine, per quod species rerum ipsi fora-"mini objectarum, illabantur, ut pingant parie-"tem in cubiculo oppositum, sed omnia in-"verso situ. Hoc igitur artificio consulentes "rei difficultati, solis imaginem in Cameram "obscuram intromisimus atque illabentem in "convenienti a foramine distantia papyro ex-"cepimus."

" que alios deciperem, sed ex aliquot sequentibus, quas ab anni hujus initio ad hoc usque " tempus non tantum ego solus notavi sed alii etiam mecum ad conciliandum huic rei " fidem et persuadendum illis, qui forte hoc observationis negotium sibi molestum aut " tædiosum existimaturi sunt." This is, unfortunately, the only passage from which we can collect any thing about the time of his discovery. Lalande[e] considers that it carries us back " à peu près au commencement de 1611;" but it seems to go somewhat further. From the cautious, unassuming character of Fabricius, it is most probable that he observed long enough to be convinced of the general truth, before he began the series of observations in which he associated others with himself; and as it would require nearly four weeks for the spots to return even once to the same part of the sun's disc, on which they had been originally seen, there will be no improbability in supposing that he had discovered them in 1610. The general style of his account gives every reason to believe, that he made no unnecessary delay in admitting his friends to a share in the pursuit of this interesting subject; but, on the other hand, it must be recollected that it appears from Harriot's account that no spots vere visible on the sun in the middle of January 1611.

Scheiner claims to have seen the spots, " anno millesimo sexcentesimo undecimo mense " Martio[f]," but he probably made no observations on them before the following October: by his own account he was therefore later than Fabricius, even if he could have supported his cause in opposition to Galileo. The great philosopher of Florence did not publish on the subject before 1613, but his " Istoria è dimostrazioni intorno alle macchie " Solare," which came out in that year, contains the letter written to Velserus in May 1612[g], which speaks of his having then observed them for eighteen months. I believe myself that he had seen them at a still earlier period, but it is unnecessary to enter further into the inquiry on the present occasion[h]. The Baron fixes the epoch of Galileo's discovery to be in the month of November 1610, which is evidently anterior to Harriot's first observation; but, to avoid the obvious inference from this concession, he argues against the probability of Harriot's having any notice of the observations which had been made in Italy. Had he confined himself to the point in question, there could have been no doubt of the fact. The controversy between Scheiner and Galileo could never have existed if the discovery of the solar spots had been generally known at so early a period even on the continent; and *à fortiori* we cannot suppose that the news had reached England. This is conclusive, as far as it goes, in Harriot's favour; but the Baron seems to wish for more, and in his anxiety to get rid of Galileo, endeavours here to shew that his discoveries were wholly unknown to the English mathematician. This is sufficiently improbable in itself, and after all that has been already detailed on the subject, there can be no occasion for repeating the refutation of it; but the argument introduced on the other hand against the originality of Harriot must not pass unnoticed. The Baron says, "it rather seems that Harriot had taken the hint from Josephus à Costa's Natural "and Moral History of the West Indies; in which he relates, that in Peru there are "spots to be seen in the sun, which are not to be seen in Europe." A specific reference is given on this head to b. I. ch. 2. which shortens the search, and enables us immediately to see that the whole is founded in mistake. A Costa certainly speaks of spots, but not on the disc of the sun[i]: he distinctly describes them as seen in the heavens; and he evidently refers only to those dark parts which have been so often noticed among the constellations of the southern hemisphere[k].

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[e] Astronomie, §. 3225.

[f] Rosa Ursina, p. 28. A little higher in the same page he had said, " maculas mense Martio " anni 1610 inspexit et ostendit;" but this must have been a misprint, or must imply that Scheiner intended to designate the year which ended on the 25th of March.

[g] It may not be generally known that the original of this remarkable letter is in the British Museum.

[h] This is a question well worthy the attention of the author of the life of Galileo. There is no one in our own country better able to discuss it: we may hope to see his work in a form more suited to its merits, and that an enlarged edition will enable us to profit from his examination of this interesting question.

To return to Harriot. His observations of the solar spots are great curiosities: like those of Jupiter's satellites they are the earliest original notices which are known to exist, and there is reason to believe that the first of them was made from his own spontaneous curiosity. Absolute priority of time is not necessary to the proof of originality: the observation however cannot, with any degree of justice, be rated so highly as the Baron would place it. There is a great fallacy in saying that "they run from Dec. 8, 1610, till January "18, 1613." These certainly are the first and last dates, but the series really begins only at Dec. 1, 1611[l]. Harriot's entry of Dec. 8, 1610, is evidently worded in a manner which impresses us with the idea of his having unexpectedly seen what he could hardly persuade himself to be real. He looked at it repeatedly with each eye, till the increasing brightness of the sun prevented him from continuing his observation. Had he soon been again able to observe the sun, there can be little doubt that he would have ascertained the existence of the phenomenon; but it appears that he did not repeat his observation till the 19th of January, 1611, and then, most unfortunately, the solar spots had wholly disappeared. The consequence was, that he did not look again after them till the following December; before which time their existence had been fully ascertained by others, and the account of what had been seen on the continent must have reached England. Accordingly, he then designates the phenomena by the specific name of spots, whereas, in the first instance, (Dec. 8, 1610,) he appears to have had no appropriate terms in which he could describe them, and merely refers to his drawing for the appearance, which he was desirous of recording. His earliest observation, indeed, was no more than a transient view of a phenomenon, which had been seen before his time, even with the naked eye; and although I am not inclined to speak of Harriot as Delambre speaks of Scheiner[m], I must say that it is a misapplication of terms to call such an observation a discovery. The Georgium Sidus was seen by Flamsteed, Mayer, Le Monnier, and Bradley, but no one would claim for either of them the discovery of the planet, in preference to sir William Herschel.

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[i] See the whole passage at note F.

[k] Lalande, Astronomie, §. 840. There is also Dacosta, disputatio philosophica de Maculis Solis. It is not impossible that some indistinct traces of this title may have been floating in the Baron's mind, and have contributed to lead him into the mistake. But the tract was not published by the author, Alvarus Telles Dacosta, till 1734.

[l] Harriot marks this distinction himself. He numbered his observations from 1 to 199, but he began with that of Dec. 1, 1611. There is no number affixed to that of Dec. 8, 1610, and this is the reason why I have called the sum of the whole 200 instead of 199, which the Baron mentions.

[m] S'il avait une lunette, il a pu voir les taches; il n'y a pas grand mérite à cela. Hist. de l'Astron. Mod. vol. I. p. 631.

Against the observation of Dec. 1, 1611, there is the mark [☌ ⊙ ♀], and there can be no doubt that it refers to the conjunction predicted by Maginus, the notice of which is inserted with the same symbols against the same day in his Ephemerides. It appears to have excited considerable attention. Venus at the time was in the superior part of her orbit, and her motion was therefore direct. No transit could have taken place; but, in spite of all which telescopes had laid open, there were some who still clung to the possibility of the Ptolemaic system, and they argued very justly, that the doctrine of Venus being always below the sun would be established, if, under such circumstances, it was seen upon its disc. Scheiner, therefore, discusses the event very particularly. In his second letter to Velserus he points out that Venus was moving in a direction contrary to that in which the spots cross the disc: the one therefore could not be mistaken for the other. As Venus would advance with only the excess of her geocentric velocity above that of the sun, the phenomenon, if it took place, would extend over a considerable time; according to Scheiner it was to commence at 11 on the night of Sunday Dec. 11, (N. S.) and to last for at least forty hours. According to these numbers the termination would not take place till 3 in the afternoon of Tuesday the 13th. Monday the 12th was entirely cloudy, but, on the contrary, Tuesday was clear and fair, from the earliest dawn till late in the afternoon: Scheiner therefore, and others with him, took this opportunity of carefully observing the sun from its first rising. Of course they saw nothing, and he therefore concludes that "etsi careremus omnibus aliis argumentis, hoc uno "evinceretur solem a Venere ambiri." He afterwards entered more minutely into the question, in his "Disquisitio de Maculis Solaribus," but it is unnecessary at present to follow him any further.

Though the precise nature of the connection is not obvious, some evidently existed between the expectation of this conjunction and the renewal of Harriot's attention to the spots on the sun. His own labours and his acquaintance with the works of Copernicus, Kepler, and Galileo, make it highly improbable that he could have had any doubts with respect to the true system of the world; but others about him may not have been equally satisfied on this point, and, on their account, he may have determined to mark the result of the expected conjunction. To examine the spots which might be on the sun at the time was an useful preliminary, for which he may have taken advantage of the mist that presented itself on the Sunday morning. As there is no delineation for the 2nd of December, the weather on that day was probably unfavourable for him, as it was for Scheiner; but in the morning of Tuesday the 3rd, he observed the sun again with sir William Lower and Christopher, at 8½ and 8¾. There is no notice taken of the planets not being seen, but the recognition of the spots which had "appeared "December 1," not improbably fixed Harriot's attention, and induced him to pursue the observations. It is no proof of the truth of this conjecture, but it is perfectly in accordance with it, that the observations of Dec. 1 and 3 are written in a larger hand and manner than will quite assimilate with the form which he afterwards almost immediately adopted for his entries, and to which he uniformly adhered till the 7th of June 1612.

That the spots might be produced by some opaque bodies revolving at short distances about the sun, was an hypothesis which occurred to some of the first observers of the fact; but if it did pass through Harriot's mind, it probably did not make any permanent impression on it. The supposition was at variance with the appearances which the spots exhibited, and with the length of time which they all uniformly took up in making their revolutions round the sun. Now we find that Harriot had attended to this period, although our expectations would be much disappointed if we looked for the strict "determination of the sun's revolution round its axis." There are two half sheets containing calculations for this purpose [n], which have been noted in red ink by the baron, so that no mistake can be made with respect to the part of the manuscripts to which he intended to refer. The first of these papers is occupied by two tables, to shew the number of days which may have elapsed at the end of every month, 1st, from the beginning of January to the end of the year, and 2nd, from the beginning of Dec. 1611 to the end of Jan. 1613. The following numbers are all that are material in the second paper, the rest being nothing but the arithmetical processes by which the results are worked out.

$$\left.\begin{array}{r}1610\ \text{Dec.}\ \ 8\\ 1611\ \text{Nov.}\ 28\end{array}\right\}\ldots 355\,d=\genfrac{}{}{0pt}{}{27}{13}+\tfrac{4}{13}$$

$$\left.\begin{array}{r}\text{Dec.}\ \ 3\\ \text{Dec.}\ 22\end{array}\right\}\ldots 385\,d=\genfrac{}{}{0pt}{}{27}{14}+\tfrac{7}{14}$$

1610 Dec. 8
1612 Dec. 17

$$\left.\begin{array}{l}\text{Dec.}\ 21.20\\ \text{May}\ \ 30\end{array}\right\}162\,d=\genfrac{}{}{0pt}{}{27}{6}$$

The meaning of this is very obvious: in the first comparison he deduced from the supposition of 355 days containing 13 synodic revolutions, that each of them was in length 27 days and $\frac{4}{13}$; the others give analogous conclusions, but there is considerable difficulty in understanding the grounds on which the comparisons were made. Harriot certainly observed the solar spots on the 8th of December, 1610, but he has left us no observation of the 28th of November, 1611. Scheiner, indeed, in his "Epistolæ ad Marcum Velserum," gives some drawings made on this latter day, (Dec. 8, N. S.) but they bear no resemblance to Harriot's first design, which could lead to the supposition of the spots being identical at the two epochs. Harriot saw the spots on the 3rd of December, 1611, but he has left us no observation of the 22nd of December in the following year [o]. There is some partial resemblance in his drawings for Dec. 8, 1610, and Dec. 17, 1612, but he has deduced nothing from it,—this interval, if it is supposed to contain 27 revolutions, would give 27 days and $\frac{10}{27}$ for each. His observation of Dec. 21, 1611, compared with those of May 29 and 31, (there is none of May 30,) 1612, exhibits nothing from which any deduction can be made. There was in fact no possibility of diminishing the error of the period by the comparison of very distant observations: the advantage of such a process is familiar to every astronomer, but in the present instance the use of it was precluded by the fleeting nature of the phenomenon. Harriot's acute mind must have perceived the difficulty, especially when he had the experience of so long a series of observations; and it is impossible to consider these calculations as any thing like a precise determination of the period of the sun's rotation.

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[n] On the back of the paper containing observations from Feb. 21 to April 7, 1612, there are some numbers which seem connected with the time of the sun's revolution, but it is not easy to understand how they are supposed to contribute to the determination of it.

[o] He saw the spots on the 23rd, and there is some little resemblance in his design for that day to what he had drawn for Dec. 3, 1611.

Such are the principal astronomical observations which really exist; but there are others to which the Baron de Zach specifically alludes, and which must not therefore pass unnoticed.

Annexed to the observation of Jupiter's satellites for May 24, 1611, there is a note that "♂ by our instrument was about 31′ or 32′ g. from ♃[p]," and "the night before "about 30′ or 31′." It had been previously mentioned that the observation of the satellites was made at 9 or $9\frac{1}{4}$, "the time being twilight, and ♃ low;" there is also a rough drawing copied in fig. 2, plate IV. Sup. but there is nothing to mark more precisely the time when the two planets were observed; and this memorandum is the only observation of Mars which I have been able to find that Harriot has recorded of his own. He wrote much indeed upon the subject, for he studied (as I have mentioned before) the treatise de Stella Martis with the greatest attention; and among his papers on this subject there are many observations mentioned of the planet; but so far from any of them having been made by Harriot, he has marked them all with the very pages of Kepler's book, from which they have been taken.

"Of solstices," equinoxes, the length of the year, &c. there are many calculations from the data to be found in the writings of earlier astronomers; but I have not been able to discover a single observation connected with any of them, which was made by Harriot himself. Wood, in his Athenæ Oxonienses[q], mentions a work of his, entitled, "Ephemeris Chyrometrica, MS. in the library at Sion college, Lond." Nothing of the kind appears in Reading's catalogue of that collection, and it is probably lost; but among Harriot's papers there are some drawings of the hand, with mnemonic lines, which, being referred to the several parts and joints of the fingers, give a technical method of finding the dominical letters, the day of the week for any particular time, &c. &c. Wood's orthography requires some emendation, but the correction is so obvious, that with this explanation there can be very little doubt of the probable contents of the volume to which he alludes. There is a contemporary publication on somewhat the same plan, entitled, Pompeii Limpii Dactylismus ecclesiasticus. Ven. 1613.

"Of refractions" there is nothing of Harriot's in the Petworth papers. There are three tables indeed, but they are copied from pp. 79, 124, 280, of the Progymnasmata of Tycho Brahe: and there are two short passages, which occur besides on this subject, but they are expressly stated to have been taken from Tycho. The work is not mentioned on them, but the pages of it are noted, and the extracts were certainly made from the volume, entitled, "De Mundi ætherei recentioribus phænomenis liber secundus." There is much on refraction in the British Museum, but it is generally confined to those cases in which the rays of light pass through a transparent medium of a regular shape: of astronomical refractions I was able to find very little, and in that little nothing of any importance. From these papers it is clear that Harriot was acquainted with the law which is deduced from the sines of the angles of incidence and refraction, but I did not mark any facts which could shew that he had used it before the time when it was discovered by Willebrord Snell[r].

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[p] In P. 23 there is mention of the near approach of Venus and Saturn. These are the only observations of the kind which have been remarked.

[q] Vol. II. p. 301.

There is a book, on which is written, "The Doctrine of Nautical Triangles," and it contains one page "for the variation of the compasse by the east & west amplitude of "the sunne's risinge and settinge," but the directions do not contain, even in the form of an example, any quantity which can be supposed to be an observation which Harriot had made for himself of "the declination of the needle." In the same manner the papers in the British Museum, though several refer to the subject of magnetism, contained no observations, which I could find, that Harriot had himself made of the position of the needle.

I am fully aware of the difficulty in establishing a negative, but I trust that if a future examination shall detect any thing which I have inadvertently overlooked, the mistake will not be found in the papers, which, by lord Egremont's indulgence, I have had the opportunity of examining at leisure. These, indeed, I wish particularly to rest upon, and any one may now judge of the little call which there was for printing them. The whole deserves to be preserved with every care; but their value centres in themselves: it does not depend so much upon the knowledge which can now be acquired from them, as on the great interest of such early records. If the Baron de Zach had, in the first instance, drawn up a fair report, and had had a few of the drawings copied as specimens, all would have been done which could have been of any use: whatever might be thought of omitting the configurations of Jupiter's satellites, the delineations of the spots on the sun form the essential descriptions of the observations, and to have had them all engraved would have been an enormous waste of property, especially at a time when facsimiles could only have been taken on copper. It must likewise be kept in mind, that the Baron had published the observations of the comets of 1607 and 1618 before he sent the papers in 1794 to Oxford. It is now discovered that he had not printed them correctly; but it was highly probable that this circumstance was not even suspected at the time, especially as they appeared in a German publication, which was not likely to be so well known in the latter part of the last century as it would have been at present.

It is much to be regretted that these facts were not more generally understood; but those who had taken an inaccurate but popular view of the subject, continued long to impress the public with very erroneous and exaggerated opinions. No one, possibly, contributed more to this in our own country than Dr. Hutton. The paper which he reprinted in his Dictionary under the article Harriot, was followed up by what the Baron de Zach published in his Monatliche Correspondentz [s] for July 1803, which spread the story on the continent. Dr. Hutton's second edition of his Dictionary in 1815, contained many additions and corrections; but the account of Harriot was left untouched, although he might have easily ascertained that the proposed publication was then given up, and that the papers had been restored more than fifteen years before to the earl of Egremont. Professor Playfair recalled public attention to them in the Edinburgh Review [t], and as he wrote under the common misconception, Dr. Robertson endeavoured to correct it by communicating his own Reports to sir D. Brewster, who published them in the Ed. Phil. Journal for April 1822.[u] The evil, however, had taken too deep a root to be removed by this plain statement, which was drawn up for a special purpose. It only discussed what was really to be found in Harriot's papers, and was therefore thought by many to have omitted much which they imagined to be contained in them: and the Baron de Zach drew up a reply [x], which must not be left unnoticed on the present occasion. It would be to very little purpose if time were consumed in a critical examination of the whole of his remarks; but there are some which require explanation, and which are principally founded on the following extremely curious and important letter.

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[r] It is not conclusive, but some presumption may be formed on this point from Harriot's having been certainly familiar with the works of Snell. There is a number of papers with the calculations which he made when studying the Eratosthenes Batavus; and there is a bookseller's bill, in which charges are made for a copy of it, and also of the Observationes Hassiacæ.

"Fragment d'une lettre de Henri Percy comte de Northumberland, prisonnier d'état, détenu dans la tour de Londres, à Thomas Harriot, Sion Collège, à Londres.

"I have received the perspective cylinder that you promised me and am sorrie, that my man gave you not more warning, that I might have had also the 2 or 3 more that you mentioned to chuse for me. Hence forward he shall have order to attend you better and to defrav the charge of this an others, for he confesseth to me, that he forgot to pay the worke man.

"According as you wished I have observed the moone in all his changes. In the new I discover manifestlie the earthshine, a little before the dichotomie that spot which represents unto me the man in the moone (but without a head) is first to be seene. A little after neare the brimme of the gibbous parts towards the upper corner appaere luminous parts like starres, much brighter then the rest and the whole brimme along, lookes like unto the description of coasts, in the dutch bookes of voyages. In the full she appeares like a tarte that my cooke made me the last weeke. Here a vaine of bright stuffe, and there of darke, and so confusedlie al over. I must confesse I can see none of this without my cylinder. Yet an ingenious younge man that accompanies me here often, and loves you, and these studies much, sees manie of these things even without the helpe of the instrument, but with it sees them most planielie; I mean the younge Mr. Protheröe [y].

"Kepler I read diligentlie, but therin I find what is to be so far from you. For as himselfe he hath almost put me out of my wits, his aequants, his sections of excentricities, librations in the diameters of Epicycles, revolutions in Ellipses, have so throughlie seased upon my imagination as I doe not onlie ever dreame of them, but oftentimes awake lose myselfe, and power of thinkinge with to much wantinge to it, not of his causes for I cannot phansie those magnetical natures, but aboute his theorie which me thinks (although I cannot yet overmaster manie of his particulars) he establisheth soundlie and as you say overthrowes the circular astronomie. Doe you not here starthe, to see every day some of your inventions taken from you; for I remember long since you told me as much, that the motions of the planets were not perfect circles, So you taught me the curious way to observe weight in Water, and within a while after Ghetaldi comes out with it in print. A little before Vieta prevented you of the gharland for the greate invention of algebra. Al these were your deues and manie others that I could mention; and yet to great reservednesse hath robd you of those glories. But although the inventions be greate, the first and last I meane, yet when I survey your storehouse, I see they are the smallest things, and such as in comparison of manie others are of small or no value. Onlie let this remember you, that it is possible by to much procrastination to be prevented in the honor of some of your rarest inventions and speculations. Let your countrie & frinds injoye the comforts they would have in the true and greate honor you would purchase your selfe by publishing some of your choise workes. But you know best what you have to doe. Onlie I, because I wish you all good, wish this, and sometimes the more longhinglie, because in one of your letters you gave me some kind of hope thereof.

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[s] Vol. VIII. p. 30.

[t] Vol. XXVI. p. 36.

[u] Vol. VI. p. 314. See note H.

[x] Correspondence Astron. vol. VII. p. 105.

[y] The Protheroes are an ancient family, deriving their name in Welch from Ap Rhyderch. Among the papers in the British Museum there is "A note of the papers and bookes "in Mr. Harriot's trunke delivered to Mr. Torporley," which is marked as "copyed from "Mr. Protheroe."

"But againe to Kepler I have read him twice over cursoridlie. I read him now with calculation. Sometimes I find a difference of minutes, sometimes false prints, and sometimes an other confusion in his accounts, these difficulties are so manie, and often as here againe I want your conference, for I know an hower with you would advance my studies more than an yeare heare, to give you a tast of some of these difficulties that you may judge of my capacitie. I will send you onlie this one . . .

["C'est ici que le comte expose ses calculs au long sur le 'locum Martis' du chapitre XXVI page 137 de l'astronomia nova . . . de motibus stellæ Martis etc de Kepler."]

. . . . for his theorie I am much in love with these particulars;

"1°. His permutation of the medial to the apparent motions, for it is more rational that all dimensions as of eccentricities, apogaeies etc —— should depend rather of the habitude of the sun, than to the imaginarie circle of orbis annuus.

"2°. His elliptical iter planetarum for me thinkes it shewes a way to the solving of the unknown walkes of comets. for as his ellipsis in the Earths motion is more a circle . . ."

The Baron says, "C'est ici, hélas! que finit le fragment de cette lettre intéressante "et remarquable. C'est bien dommage! car c'est précisément ici que le savant Lord "entre en matière sur le mouvement des comètes, assurément il a devancé son siècle "dans ses idées, car le noble Comte avait déjà laché, comme l'on dit, le vrai mot du "guet, lorsqu'il a osé mettre en avant, que le chemin elliptiques des planètes montrait "celui des comètes. On peut donc deviner avec quelque certitude, et par conséquent "regretter les systèmes et les hypothèses que ce savant Seigneur aurait proposés, qui "étudiait avec une si profonde application, et la plume à la main, les ouvrages épineux "de Kepler." It is clear from the expressions here used that the Baron had never seen more than he has given us of the letter, and therefore was not in possession of the earl of Northumberland's signature to it. He could only have collected from the contents that it was his, and it is not too much to say, that the internal evidence is decidedly against such a conclusion. It will be seen that the Baron supposes the letter to have been written in 1610, but the earl was prisoner in the Tower from 1606 to 1619[z], and during that time Wood[a] tells us, that Harriot was one of his "constant companions." Under these circumstances it was impossible for him to have complained of distance, or to say "again I want your conference, for I know an hour with you would advance "my studies more than a year here." The original of what the Baron has printed is not to be found among the papers which are known to exist in England, but as there is no reason to suspect that he has not faithfully given its contents, there was the greater satisfaction from being fortunate enough to find in the British Museum the conclusion which had escaped him. It is as follows:

" . . . . . . & in mars is more longe & in some of the other planets may be longer againe so in thos Commets that appeare fixed the ellipsis may be neere a right line.

"3. His phansie of ecliptica media or his via regia of the sun. vnto wch the walke of al the other planets is obliq; more or lesse; euen the ecliptica uera vnder wch the earth walkes his yeares iournie; by wch he solues handsomelie the mutation of the starres latitudes & indeed I am much delighted with his booke but he is so tough in manie places as I cannot bite him. I pray write me some instructions in your next, how I may deale with him to ouermaster him for I am readie to take paines. te modo iura dantem indigeo, dictatorem exposco. But in his booke I am much out of loue with thes particulars. 1. First his maine and intollerable atechnies. whence derive thos maine & vncertaine assayes of calculation. 2. his finding fault with Vieta for mending the like things in Ptol. Cop. &c. but se the iustice Vieta speakes sleightlie of Copernicus a greater then Atlas. Kepler speakes as slightlie of Vieta, a greater then Appollonius whom Kepler everie where admires. for who soever can do the things that Kepler cannot doe, shalbe to him great Appollonius. but enough of Kepler let me once againe intreate your counsel how to read him with best profit, for I am wholie possessed with Astronomical speculations & desires.

"For your declaration of Vieta's appendicle it is so full and plaine as you have aboundantlie satisfyed my desire for wch I yeild you the thankes I ought. onlie in a word tell me whether by it he can solve Copernicus, 5 cap: of his, 5 booke.

"the last of Vieta's probleames you leave to speake of because (you say) I had a better of you, wch was more vniuersal and more easilie demonstrated, and findeth the point, E. as wel out of the plaine of the triangle giuen, as in the plaine. I pray here helpe my memorie or vnderstandinge, for although I haue bethought my selfe vsq; ad insaniā; I cannot remember or conceaue what proposition you meane. if I haue had such a one of you, tel me what one it is and by what tokens I may know it; If I haue not had, then let me now have it. for you know how much I loue your things and of all wayes of teaching for richnesse & fullnesse for stuffe and forme, yours vnto me are incomparablie most satisfactorie. if your leasure giue you leaue imparte also vnto me somewhat els of your riches in this argument.

"Let me intreate you to advise & direct this bearer Mr. Vaughen wher and how to prouide himselfe of a fit sphere; that by the contemplation of that our imaginations here may be releued in manie speculations that perplexe our vnderstandings with diagrammes in plano. he hath monie to prouide doe you but tell him wher the are to be had and what manner of sphere (I meane with what & how manie circles) wilbe most vsefull for vs to thes studies. after all this I must needs tell you my sorrowes. god that gaue him, hath taken frō me my onlie sun, by continual & strange fets of Epelepsie or Apolexie, when in apparence, as he was most pleasant & goodlie, he was most healthie. but amongst other things, I haue learnt of you to setle & submit my desires to the will of god; onlie my wife with more greife beares this affliction, yet now againe she begins to be comforted. let me here frō you and according to your leasure & frindshippe haue directions in the course of studie I am in. aboue al things take care of your health. keepe correspondence with Kepler. & wherinsoeuer you can haue vse of me, require it with all libertie. for I rest euer

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[z] The mistake with respect to Sion College has been already pointed out, (p. 22.) and when the letter was first printed in the Monat. Corr. (vol. VIII. p. 47.) the only designation used was "à Londres;" but still there is in this simpler address what must naturally suggest considerable doubt. The earl of Northumberland of those days (when he was himself in the metropolis) would never have barely directed his letter to "Mr. Tho^{s}. Harriot in "London."

[a] Vol. II. p. 300.

"Your assured & true frind to be
vsed in all things that you please
" Willm. Lowër."

" Tra'venti on Mount Martin
6. February. 1610."

"let me not make my selfe more able then ther is cause. I can not order the calculation by the construction you sent me of Vieta's, 3. probleme, to find the distances of C. &, D. &, B. frō the Apogeū or the proportion of *ia.* to, *ac.* the eccentricitie. I tooke Copernicus, 3. observations in the, 6. chap: of his, 5, booke. therfore helpe here once againe.

" To his especiall good
frind Mr. Tho: Harryot
at Sion neere London."

It is difficult to devise a reason for the Baron's having been led to imagine that the letter had ever been written by the earl of Northumberland. Some of this nobleman's papers [b] are still preserved at Petworth, and his handwriting is perfectly different from that of sir William Lower. There can however be now no doubt about the real author. Every thing is clear, and it even happens that the original seal remains attached to the paper, bearing the family arms [c], with a unicorn's head erased, for a crest, surmounting a knight's helmet. There is very little to be found in print about this remarkable man, and the whole of that little is not to be depended upon. This has been occasioned by his having been confounded with a nephew of the same name, a dramatic writer, of whom an account has been given by Wood [d]. What I have been able to collect about him at this distance of time cannot be much, but it will be found subjoined in another place [e]. His name [f] is new in the history of English astronomy, but it deserves to be recorded.

Several of the topics in this most curious letter are evidently continued in that of June 1610[g], and give further precision to some facts connected with the state in which science was at the time when it was written. It shews that telescopes were made in London as early as Feb. 1610, and corroborates what has been said[h] on the earlier observations of the moon: but there are points of still more importance, for which much information may be derived from it.

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[b] None of them are on scientific subjects.

[c] Sable, a chevron between three roses argent.

[d] Ath. Ox. vol. III. p. 544.

[e] See note I.

[f] He is not noticed in Sherburne's Manilius.

[g] P. 25.

The letter was republished in vindication of the high importance which the Baron de Zach attached to Harriot's papers, and in confirmation of some remarks on Dr. Robertson's publication of his Reports upon them. The present statement has certainly exceeded the bounds which I first anticipated, and possibly is already too long; but this portion of the Baron's reply must not be left unnoticed. It contains many particulars, of which the lapse of years must have weakened his recollection; but as they are stated without hesitation, the present discussion could not (as I hope it will) be final, if some of the mistakes in them were not distinctly pointed out.

Before however we enter upon the particular objections which the Baron brings forward, it may be right to notice an impression which must be received by every reader who is unacquainted with the particulars of the case. After mentioning (p. 105.) lord Egremont's supposed gift of Harriot's papers to the University, as if it had taken place in 1784, or soon after, he goes on to say, (p. 106.) " Il s'agissait dans le tems d'en publier des " extraits, mais le docteur Robertson, professeur d'astronomie en cette université, avait " declaré en 1802, que ces manuscrits ne contenaient rien qui fut digne de l'attention " publique." It would not be worth while to notice that Dr. Robertson became professor of astronomy only in 1810, if it were not that the circumstance might lead to a suspicion of his opinion, as here referred to, being of a still later date than 1802. Now the idea at present conveyed by the Baron's statement is, that the papers remained at least eighteen years at Oxford before any determination was made about what was, or was not, to be done with them. After all that has been said, it will be unnecessary to repel an insinuation, which it is impossible that the Baron could have intended to convey. He must have known that for the greater and most important part of this period, (for the first ten years,) the papers were wholly and solely at his own disposal; and, although I would not speak lightly of the time and care which would be required to ascertain how far they were fit for the press, still if this preliminary step was long before it was executed, he must have known to whom the delay was principally to be attributed. The expression of " des extraits" must likewise convey an inaccurate idea. With a view to this, the letter is printed in which the Baron made his formal proposals to the Delegates of the Press in 1786[i], and it evidently appears from it that the design was any thing rather than the mere selection of " some extracts" of particulars, which might be interesting in themselves, or calculated to convey an idea of what the papers contained. I say this to prevent mistakes, and I have no wish to impute blame to the Baron for what he may simply have left undone in the business. He embarked in it of his own freewill, and if called off to other objects would naturally be desirous of giving up the undertaking. He had imprudently raised expectations which could not be fulfilled; but those, who had most ground for complaint against his withdrawing, were the Delegates of the Oxford Press, who had been induced, at his request, to take a share in the publication. They, however, would have been the last to insist on a contract which had become inconvenient, or even unpleasant to him; and, in the exercise of reciprocal good feeling, they ought to have been the last, on whom he should have endeavoured to fix any blame for not pursuing what he had himself abandoned. Whether he has been sufficiently cautious in this respect, and whether he has had good ground for what he brought forward, is now to be examined. He reduces his remarks, on what he thought had been improperly neglected, into a series of questions; and that the reader may be best able to judge of them, I shall give each in his own words, with such observations as they suggest.

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[h] P. 20.

[i] See note B.

"[k] Il est très vrai, comme le remarque fort bien le docteur Robertson, que tous les manuscrits de Harriot ne méritent pas l'impression, et n'avaient pas été écrits dans cette vue; cependant on aurait bien pu en donner des extraits, surtout de ses observations astronomiques. Par exemple, Harriot observait avec une grande application les satellites de Jupiter tout-nouvellement découverts . . . . . Ne serait-il pas curieux, & même glorieux à la mémoire de Thomas Harriot, de faire voir, ainsi que nous l'avons fait il y a 38 ans, qu'il avait été un des premiers astronomes de l'Europe, hors l'Italie, qui eut, non seulement observé, mais aussi calculé, les révolutions périodiques de ces lunes autour de leur planète?"

If an answer was merely intended to the individual writer, it might be sufficient to suggest, that no one could have a right to blame others for not doing what he says that he had himself done nearly forty years before, and which, if properly executed, would not require to be repeated. But without dwelling on the introductory matter, it must be remarked that the two parts of the last sentence are not strictly in accordance with each other. In the early accounts (see note A) to which the Baron refers, he distinctly claimed for Harriot the honour of having discovered Jupiter's satellites even before Galileo, but in this instance he only speaks of him as among the first "hors l'Italie" who paid attention to the subject. I am not aware of any place in which the Baron has expressly mentioned this alteration in his views of the question; I may however have overlooked it, nor have I any wish to deprive him of the argument (as far as it goes) in favour of his not being guilty of intentional mistatements. Indeed he could never have urged others to the publication, or even the examination of these papers, if he had been aware of all the misconceptions which he had formed respecting their contents; but this very correction is an additional proof of his having originally given reins to his imagination, and having trusted himself to two of the most fallacious guides upon which human beings can depend——impressions at first very hastily collected, and the powers of recollection without reference to original documents. The same circumstances will be found in many of the following questions, and it may therefore be a subject of regret that the real state of the case was not more fully developed at an earlier period. It must however be recollected, that although much of this later inaccuracy might have been discovered without an examination of the original papers, yet without them no one could have ventured on a general answer to the Baron's allegations, and a special application for the use of them was what few could think themselves entitled to make. When I went to Petworth, it was only in hopes of seeing what was there; I never anticipated the indulgence of being allowed to bring them away with me, and to print any parts which I might think desirable to be laid before the public.

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[k] p. 108.

"Ne serait-il pas curieux, & même glorieux à la mémoire du comte de Northumberland, et à celle de Thomas Harriot, en faisant voir qu'en 1610, ils avaient déjà à Londres des lunettes d'approches, qu'ils appellaient en anglais 'perspective cylinder,' tandis qu'en 1622 on doutait encore en France de l'existence de cet instrument 'que l'on ne croyait pas légèrement dès deça' comme s'exprimait le célèbre Peiresc?"

Whatever was honourable in such respect for the earl of Northumberland, was still more honourable for an individual like sir William Lower; there can be no occasion therefore to cavil about this mistake: but it is remarkable that the Baron, when he had not seen the date of the letter, should have fixed so exactly upon 1610 for the time when it was written. It has been seen [l] that the case is still stronger than he makes it, for Harriot had a telescope in July 1609, but the information, by which the honour of the earl of Northumberland was to be vindicated, depended (it must be remembered) entirely upon a letter, which the Baron had kept in his own hands, and so far had he been from sending it with the other papers to Oxford, that its contents were not known till he published them in 1803, nor has any thing even now been discovered about its very existence. That telescopes were unknown at Paris in 1622 is in contradiction to Gassendi's [m] account of Peiresc's having been supplied with them from that very place at a much earlier period. Indeed, in another place[n], the Baron suggests the possibility of some mistake in this date; but that is, after all, not the true solution of the difficulty, for Peiresc [o] expresses no doubt about the existence of telescopes in general, but of some long ones, by which it was said that writing could be read at the distance of a league.

"Ne serait-il pas curieux, & même glorieux pour la nation à laquelle nous devons la connaissance des véritables lois de la nature, et de toute la mécanique de l'univers, de voir que Harriot avoit pensé & enseigné long tems avant Kepler que les orbites des planètes étaient elliptiques?"

There is nothing to which this can refer but to sir William Lower's communication, which the Baron had kept to himself, and which after all will not by any means support the implied accusation. "Utinam tam facile vera invenire possim quam falsa convincere [p];" it is clear from the indisputable testimony of this valuable letter that Harriot had shewn, from his own investigations, "that the motions of the planets were not perfect "circles;" but we have no evidence of his having advanced further. This indeed was a considerable step, but it must not be put in competition, much less be considered as identical, with the actual discovery of Kepler's great law of the universe.

"Ne serait-il pas curieux, & même glorieux pour cette même nation, en lui faisant voir qu'en 1610 un compatriote de Newton avait déjà proposé de calculer la marche inconnue des comètes dans des orbites elliptiques, tandis qu'en 1619 Kepler lui-même dans son traité "de cometis libri "tres" la croyait encore rectiligne?"

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[l] P. 20.

[m] P. 28.

[n] p. 124.

[o] Camdeni Epistolæ, p. 333.

[p] Cic. de Natura Deorum, lib. I. c. 32.

Sir William Lower's suggestion [q] with respect to the orbits of comets might almost be ranked with some of Newton's queries. To have extended to them by analogy the elliptical theory of the planets, was in the true spirit of philosophy; but if the Baron's question is meant to imply that there was any plan of reducing it practically to calculation, it may be right to explain that no traces are to be found of such an intention. The time indeed had not yet arrived for the complete investigation of this important conjecture; but there is a letter of Dr. Wallis to Oldenburgh, dated Jan. 21, 1665.[r] in which he speaks of the opinion which Hevelius had maintained of the rectilinear paths of comets; and he adds, " Since conjectures about such hypotheses are now stirring. I thought it not amiss " to revive that of Mr. Horrox, which I find among his papers. In some of his letters " to Mr. Crabtree, he intimates his conjecture of their issuing forth of the body of the " sun by a strait line. Afterwards upon examining the motion of some comets, he " addeth to that of their coming from the sun's body some curvity of motion arising " from it. At length he seems to have pitched on such an hypothesis, as the figure inclosed represents, making it issue out of the sun's body, and by an elliptical figure (or " near it) to return thither again; according to which he hath traced the motion of that " of 1577, (according to Tycho's observations, as I suppose.) But this paper being his " own hand, and (for ought I see) all that is remaining concerning this hypothesis, I " desire that it may be carefully preserved." Wallis, it may be recollected, was engaged in arranging and collecting the papers for Horrox's Posthumous Works, which he published in 1672. In that work there is [s] a letter to Crabtree, in which mention is made of comets; " quod nempe ex sole prodeant omnes, sed et a sole circumducuntur: æqualiter autem, an (quod verisimile videtur) inæqualiter, pro varia a sole distantia, non " ausim definire." The diagram, to which Wallis refers, is preserved with his letter in the archives of the Royal Society. The orbit which it delineates has an obtuse cusp at the sun, and is such as it would be impossible for the comet to have really described; but it proves that Horrox had thought of comets possibly revolving in curves which returned into themselves—an idea which, if the life of that extraordinary man had been prolonged, might have ripened into a considerable advance towards the truth, although nothing less than Newton's power was adequate to its full developement.

The Baron indeed anticipated systems and hypotheses [t] which were lost to us by his only being able to present the public with the beginning of sir William's letter. The termination of it however does not contain what was looked to with so much confidence. " Those comets which appear fixed" may possibly allude to phænomena (if any such should be observed) like the famous star of 1572 which was seen in Cassiopeia; but this is the only application of the theory which is proposed; and it may be remarked, that the proposal seems principally to have been brought forward because it does not practically introduce any wide departure from the rectilinear hypothesis.

In his letter [u] to the Delegates of the Oxford Press, the Baron says distinctly that Harriot had assigned a parabolic path to the comet of 1607—" apparet ex celeb. viri M.sptis " illum jam cometæ anni 1607 viam parabolicam assignasse." This, if the account were accurate, would have been a splendid instance of astronomical advance in the beginning of the seventeenth century. But how does this great fact appear from Harriot's manuscripts? not from any particulars[v] to that purpose having been found among them, for the Baron himself tells us in another part of the same letter, that "in Harrioti chartis "nil aliud quam hujus cometæ distantiæ a fixis variis ad nullum revocatæ calculum "extant." Neither can an argument be drawn from any subsequent use of the theory. Harriot's calculations for the comet of 1618 are all entire in the papers which are preserved at Petworth, and his method is very simple. He calculates the distances not in a parabola, but in a great circle, for the intervals between the several places in which the comet had been observed. He gives a tabulated view of the results, and compares the sum of all these quantities with the distance, still in a great circle, between the first and last places in which the comet was observed. This very table has been printed by the Baron himself[u], though he has not arranged the result in the most perspicuous manner. It should be as follows:

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[q] P. 43.

[r] Birch's Hist. of the Royal Society, vol. II. p. 11. [s] p. 321. [t] P. 43. [u] See note B.

"Summa (of the several distances)	66°	52′	22″
"Distantia a primo loco ad ultimum in circulo magno	66	35	50
Differentia	0	16	32″

Harriot likewise estimated the inclination of the orbit by calculating the angle which the ecliptic would make with a great circle passing through the first and last places in which the comet had been seen. All this was an effort of great industry, but exhibits nothing beyond the knowledge of the time in which he lived, and applied, after all, to the apparent, not to the real, path of comet.

"Ne serait-il pas curieux, et même glorieux, de faire observer, et de demonstrer par les papiers de Harriot, que ce grand et véritable génie avait long-tems avant Ghetaldi fait des expériences sur la pesanteur spécifique des corps?"

This fact is certainly very curious and interesting, but there are no traces of it in any of the papers which the Baron sent to Oxford. There are however a sufficient number bearing on the point in the British Museum, and one which corroborates in a very remarkable manner the praise which sir William Lower so justly bestows upon his friend. Ghetaldi published his Promotus Archimedes in 1603, and one of Harriot's papers connected with specific gravities, is dated Sept. 22, 1601. This was not certainly "long-tems avant," but it agrees perfectly with Ghetaldi's "within a while coming out "with it in print."

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[v] There is one half sheet of paper containing a curve, which resembles very much a semiparabola. On the axis of it are taken equal parts, and against the points of division are affixed dates from the 15th of Nov. to the 23d of Dec., which last is at the vertex of the curve. This may have suggested the via parabolica to the Baron, but it cannot represent the orbit of a comet, because it makes the abscissæ proportionate to the times; and if it had been intended for such a purpose it could not have been for that of 1607. which Harriot did not observe after the 22nd of October. Against the 15th of November is written "London & Syon," and this is about the time when the comet of 1618 was seen there, but it had then passed its perihelion. Kepler has a curve which is intersected by the rectilinear path of his comets, and we have a straight line in the same manner cutting Harriot's in two points, which answer to the places from whence perpendicular ordinates would be drawn to the 21st or 22nd of Nov. and the the 13th or 14th of Dec. There is no further explanation, but it is not improbable that this, after all, is only a graphic trial to represent a portion of the figure according to the same method with Kepler; and if so, Harriot must in 1618 have still endeavoured to calculate for the comet in a rectilinear path. It is right to add that the curve much resembles that which Kepler gives for 1607, but is very different from that which he draws for 1618, and which has more resemblance to an hyperbola than a parabola.

[u] Bode, Jahrbuch, Supp. I. p. 34.

"Ne serait-il pas curieux, et même glorieux à la mémoire de ce grand mathématicien,...... de faire voir, que près d'un siècle et demi avant James Dodson, il avait déjà calculé des tables antilogarithmiques pour 100.000 nombres?"

The account of this antilogarithmic canon is in Wood's Athenæ Oxon.[x], and he does not give the least hint of Harriot's having had any share in forming it. He distinctly says that it was calculated by Warner for all numbers to 10000, and completed many years after by Dr. Pell for the remaining 90000. Wallis, in his first English Treatise of Algebra, (1685.) does not mention this work in his XIIth chapter, but in that part of the Latin translation, (which forms the second volume of his Works, 1693.) he introduces Wood's account, and adds one or two circumstances from his own knowledge. He cannot be suspected of want of interest in the subject, but he speaks with great doubt of Harriot's having even had any share in originating the undertaking—"Canonem "illum nescio an inchoaverit D. Thomas Harriot."—He adds, "opus illud memini "me vidisse (et quidem solummodo vidisse) inter alia, sive Harrioti, sive Warneri schediasmata."

"Ne serait-il pas curieux, et même glorieux à la mémoire de Harriot, de faire voir qu'il avait observé la fameuse comète de 1607, dite la comète de Halley, et dont les observations nous ont donné la véritable orbite elliptique de cet astre remarquable?"

In answer to this inquiry, it is only necessary to refer to what has been already said on the subject in P. 41. There was no reason for the author's reflecting upon others for not doing what he had professed to have done before them.

"Enfin, ne serait-il pas curieux, et même glorieux pour un compatriote de Harriot, d'entreprendre une nouvelle révision de ce fameux procès entre lui et Descartes, sur la priorité de plusieurs découvertes algébriques; contestation à-peu-près semblable à celle entre Leibnitz et Newton au sujet du calcul différentiel et intégral?"......

To help in establishing the fair fame of Harriot would be a source of the highest gratification to me. He was not only a countryman, but a distinguished member of that university to which I have myself the honour of belonging. If truth hath obliged me to shew that his astronomical observations are not of that high character which they have been supposed to possess, he can well spare what does not belong to him. There is no degradation in being second to Galileo, and there is much praise to which he is most justly entitled. He certainly made a very early use of the great invention of telescopes; he carefully examined some of the most curious phenomena, which they enabled him to observe in the heavens; he took great pains to ascertain the laws of the planetary motions, and was not deterred from the necessary calculations, even when, before the invention of logarithms, he was obliged to work by methods of a more operose description. Could I have continued the inquiry to an examination of his other pursuits, I probably should have met with results more consonant with my wishes. Sir William Lower's letter encouraged this hope very strongly: he appears to have shared in Harriot's studies, and to have been intimately acquainted with them: his testimony therefore is of great weight to the priority over Vieta[y], and I sincerely hope that some one who has leisure, and to whom a residence in London may give opportunities which I do not enjoy, will pursue this part of the inquiry, by thoroughly examining the papers in the British Museum.

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[x] Vol. II. p. 302.

Of those papers which were sent to Oxford the description of the geometrical part may be seen in Dr. Robertson's Reports; and this was a subject on which he was most especially fitted for forming a good judgment; not only from his general knowledge, but from his having recently published his first work on Conic Sections, which contains a valuable history of that branch of science. Of the algebra he hardly says any thing: he mentions indeed the name of Vieta, but gives no particulars, probably from there being so little to remark upon. The whole which the Baron selected on this subject was confined to sixteen half sheets of foolscap. Thirteen of these are entitled, "de numerosa "potestatum resolutione[z]," and contain problems similar to those in the "Exe"getice Numerosa" of the "Artis Analyticæ praxis." Among these, several of the examples are marked as Vieta's, and this shews how necessary it is to be cautious in reasoning on what belongs to different individuals. The occurrence of this name, if we had not sir William Lower's testimony to correct it, would certainly give the impression of Harriot's having probably borrowed more than the examples with which he illustrates his methods. The remaining three papers contain the resolution of some biquadratic equations, in the way devised by Ferrari. It seemed worth while to have one of these lithographed in Pl. V. Sup. No one who may be inclined to examine the whole of it can be impeded by any serious difficulty, and the object for which it is now published may be evident to the most casual observer. M. Montucla[a] is very particular in arguing "qu' Harriot n'eût qu'une ideé peu developpée des racines négatives." This is a point which he considers that he has completely proved, and no objection can be properly brought against his argument from what was then unpublished. But it is impossible for any one to look at the example here given without seeing at once that the opinion is evidently erroneous. A modern algebraist would be less diffuse, and write his roots with more compactness, but he must bring them out upon the same principles, and could add nothing in this respect to the results.

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[y] P 43.

[z] One of these is curious, from containing the same numbers as the "Anticipationis exem"plum," in p. 143 of the "Artis Anal. Praxis."

[a] Histoire des Math. vol. II. p. 107.

NOTES.

NOTE A, p. 17.

In the Memoirs of the Royal and Imperial Academy of Bruxelles, (vol. V. 1788,) the Baron de Zach published a paper on the Georgium Sidus: he introduced into it the mention of discoveries that he attributed to Harriot, and added a note, (in p. 24,) which contains the following passage:

"C'est dans l'été de 1784 que je fis cette découverte curieux. Ayant passé cette saison sur les terres de Milord Egremont à Petworth, dans le comté de Sussex, je trouvai dans la bibliothèque de son chateau de vieux manuscrits du célèbre Thomas Harriot. M. le comte de Bruhl envoyè extraordinaire de la cour de Saxe à la cour de Londres, à qui ce trésor étoit connu, m'en avoit donné la connaissance. Parmi ces papiers reellement précieux, je trouvai que Harriot avoit observé les taches du Soleil et les quatre satellites de Jupiter avant Galilée. La découverte de ces manuscrits n'est pas seulement intéressante et curieux pour l'histoire de l'astronomie, mais infiniment utile et précieux pour les différentes observations qu'ils contiennent, et que je compte de donner un jour au public."

Count Lucchesini inserted some remarks on this statement in Tiraboschi, Storia della litteratura Italiana, in which he says,

" Aspetto con ansietà che il Sig. Zach dia in luce questi manoscritti; ma credo che intanto
" ci sarà lecito di dubitare un poco, che forse quelle osservationi dell' Harriot non sieno state
" esaminate abbastanza, nè ben consideraté i tempi, nè quali furano fatte[a]."

NOTE B, p. 18.

[b] " Illustrissimis, clarissimis, et doctissimis viris celeberrimæ ac antiquissimæ academiæ Oxoniensis, preli curatoribus, Franciscus de Zach, ejusdem ecademiæ legum doctor[c], serenissimi ducis Gothani astronomus aulicus, scientiarum academiarum quæ Bruxellis, Lugduni-Galliæ, Massiliæ, et Divioni florent, socius—Salutem et felicitatem."

" Reperienti mihi (annus est) et perlustranti autographa summi et sagacissimi mathematici et astronomi nunc iterum in literarum Republica redivivi Thom. Harrioti injectum est illico desiderium moliendi opus publico jure dignum, in quo ad multorum honorum vindicationem scripta tanti viri una cum controversiis et commentationibus ad ea pertinentibus quanta possem eruditione colligerem et ex oblivione ad apricum producerem.

"Accinxi me ad opus hoc explorando summa cura manuscripta quotquot inveneram, perlectisque quotquot nancisci potui de authoris vitæ et manuscriptorum argumentis voluminibus; sed quo tempore et quo auxilio in lucem proferretur, nulla erat, post tot tantosve conatus spes relicta, nullum relictum consilium.

" Postquam vero, vos, viros illustrissimos doctissimos almæ et celeberrimæ academiæ rei typographicæ curatores, non tam patronos ac vindices, quam æstimatores et judices nactus sim, qui ad

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[a] Vol. VIII. p. 358.

[b] This document is very long, and contains much which does not bear on the present purpose. It would have been desirable not to have printed the whole of it; but the reader might not have been satisfied of the fairness with which any part had been omitted.

[c] In Feb. 1786 the complimentary degree of honorary Doctor in Civil Law had been conferred on the Baron.

vulgandum opus sumptus summa cum liberalitate et munificentia suppeditare decreveritis, id mihi officii credidi dari ut quæ publicatu digna, luce non indigna mihi viderentur, paucis his vestro submitterem judicio, sic non modo monumentum in summi ingenii virum e gremio vestro natum æternumque meæ in vos observantiæ pignus habebitis, sed me ipsum et nuperrime gremio vestro adscriptum de ejus honore et gloria summopere sollicitum perpetuo retinebitis.

"Valete, viri illustrissimi, clarissimi, doctissimi, cum ad amplitudinem celeberrimæ academiæ, tum ad scientiarum et bonarum artium incrementum et præsidium diu felices. Dabam Londini Idibus Aprilis MDCCLXXXVI.

"I. *Authoris Vita.*

"Autographa Thom. Harrioti edituro primo omnium in limine operis vita viri omnis eruditionis et communis literarum sui ævi cultoris contexenda venit. Sic non solum iis qui ortus et progressus artium et inventionum curiosius investigant, verum etiam iis, qui historiam et incrementa scientiarum, imprimis algebraicæ scientissime perscrutantur, ex omni parte erit consultum, quo et fiet, ut æqua disquisitione merita inventori tribuatur cuiquæ gloria. Sat erit mihi hic indicasse actum non agi si vitam hanc ad modum Gassendi in vitam Purbachii, Regiomontani, Copernici et Tychonis conscribendam susceperim cum ex fontibus per sesquiseculum reconditis et ab omni hominum cognitione remotis, doctissima felicissimaque inventa solertis ingenii viri in lucem nunc primum proferre valeam.

"II. *Observationes Astronomicæ Cometæ* 1607.

"Docet philosophia Newtoniana cometas equidem ac planetas attractionis vi, quæ in ratione duplicata distantiarum reciproca a sole est, in orbibus ellipticis circa solem in communi foco positum revolvi; Discrimen unicum quod cometarum inter et planetarum orbes interest illud est, ut illi sint ellipses protractæ seu ingentis excentricitatis, isti ellipses ad circulos quam proxime accedentes, jam vero in definiendis planetarum semitis orbes tanquam ellipses, quales revera sunt, spectantur, illorumque formæ ex observationibus definiuntur, atque porro axes et cætera elementa calculo haud ingrato deducuntur; Non ita res est in cometarum determinandis orbibus namque inextricabilis immensique esset laboris illorum orbitas per ellipsin ad unguem investigare, commodiorem sibi itaque aperuerunt astronomi recentiores viam in cometarum motibus supputandis, ausus est jam summus astronomus Braheus et post hunc Mæstlinus cometæ anni 1577 circularem assignare orbitam; ausus est celebr. Keplerus cometæ motum anni 1618 rectilineum facere, ast quis astronomorum primus fuit, qui cometarum motus parabolæ adaptavit Hevelium inter et Doerfeldium lis penes judicem hæret, Litem vero hanc sagacissimus mathematicus et astronomus Thom. Harriotus componit, apparet enim ex celeb. viri M.sptis illum jam cometæ anni 1607 viam parabolicam assignasse. Hevelius de cometarum parabolico motu tantummodo in cometographia anno 1668 typis edita verba facit, Doerfeldus primum in cometam anni 1681 parabolæ suppositionem confinxit, ab hoc tempore omnes astronomi tum circulares tum rectilineos imprimis Newtoniana doctrina traducta ellipticos neglexere motus, qui ob prope infinitos calculos et prorsus ingratos vix Herculeo labore tractabilis esset, feliciter vero et mirabili cum circumstantiarum consensu evenit, ut cometarum orbes in solis vicinia, hoc est in systemate nostro planetarum decursu vix a parabolicis abludentes, cum parabolæ unicæ curvæ tetragonismo mirifice convenirent, quod et methodum expeditum et simplicem suppeditat illorum orbitarum elementa non operose adeo calculo ex datis geocentricis observationibus depromere.

"Quandoquidem cometa quam diu nobis conspicuus per parabolam apprime possit representari, attamen ut per se liquet, periodicum tempus totius revolutionis per orbem nequaquam per parabolam obtineri potest, per ellipsin vero, ultra immensum calculorum farraginem parum admodum accurationis inde spectare perito astronomo fas est, siquidem vel minimus in observando irrepserit error totius ellipseos formam ultra modum immutat, protendit vel contrahit, ut nil exinde certi colligi possit; ad stabiliendum itaque cometarum reditum et tempus revolutionis periodicum ad aliam confugiendum erat methodum, nam theoria Newtoniana perpetuum cometarum in orbem recursum postulabat, obtulit se illico collatio cometarum orbium et illorum elementorum inter se si enim invicem vel consentiant vel parum discrepent, concludendum jure erit, unum eundemque esse cometam orbesque eorum specie tenus pro iisdem haberi posse, et si præterea tempora post quorum lapsum cometa ad perihelium iterum regreditur, inveniantur æqualia, nulla tunc sane supererit incertitudo aut ullum de hujus cometæ identitate dubium, indeque motus revolutionis periodicus citra omnem hæsitationem colligi poterit.

"Talem cometarum orbium mirabilem consensum primus detexit, primus exinde reditum cometæ prædixit homo Oxoniensis in alma hac academia geometriæ professor Savilianus Dr. Edmundus Halleius Societatis Regiæ scientiarum quæ Londini floret e secretis, immortali quoque Flamstedio in specula astronomica regia Grenovicensi successor meritissimus. A° 1705 clariss. vir ille insigne hoc inventum publici juris fecit, simulque Synopsin elementorum 24 cometarum in orbibus parabolicis tradidit, et per orbium congruentiam potissimum ostendit cometam in una et eadem revolvi ellipsi qui A° 1305, A° 1380, A° 1456, A° 1531, A° 1607, A° 1682 ad solem maxime appropinquavit, quapropter hujus cometæ reditum audenter definire cœpit, assumpta itaque periodo media 75½ annorum, quæsivit orbitam ellipticam, habitaque ratione deflexus a vi perturbante Jovis procedentis in cujus vicinia cometa per plures versabatur menses, tandem per calculum maxime laboriosum conjecit, posteritatemque monuit, ut reditum ejus circa initium anni 1759 invigilaret, quemadmodum reipsa contigit, et de quo summo ingenii humani et astronomiæ conatu non injuria gloriatur Britannia.

"De hujus cometæ motu, periodo, perturbatione, incredibili industria et ingenii sagacitate egit summus geometra Clairautius, qui in eximio edito opere 'Theorie du Mouvement des comètès: immani calculorum labore omnes illius cometæ perturbationes, et inde ortam temporis protractionem ad stupendam revocavit veritatem, ex sola Saturni attractione retardationem trium mensium invenit, sed magnæ adhuc restat inter astronomos de mole Saturni incertitudo facit illam Newtonus $\frac{1}{3021}$ posita hac solis=1. Invenit vero clar. de la Lande ex quarto ejus comite $\frac{1}{3394}$ mediam vero ex quinque $\frac{1}{3927}$. Putat itaque eximius Geometra D. de la Grange ex collatis Harrioti in cometam anni 1607 quadam accuratione habitis observationibus cum iis quæ annis 1682 et 1759 institutæ fuerant, tempus periodicum et ex ejus inæqualitate et protractione quæ ad annum integrum et menses 7 assurgit, Saturni molem (habita æqua ratione de Veneris ac Jovis cognita actione) posteriori via colligi ac comprobari posse.

"Ex prædictis itaque apparet qualis quantæque notæ cometa anni 1607 sit, et quamquam Hallei sententia imprimis in cometis annorum 1531 et 1607 fulciatur, tamen observationes Apiani et Kepleri in hos cometas oculorum fide et crasse institutæ ad tam subtile negotium enucliandum haud satis idoneæ sunt. Nam Petrus Apianus cometam anni 1531 a die 13° Augusti ad 23tiam tantummodo ejusdem mensis observavit. Keplerus et Longomontanus vero cometam anni 1607 a Sept. 16 ad Octob. 19 observarant et quidem nulla subtilitate ut ipsius Kepleri verba (de cometis, pag. 30.) 'nec enim opus est subtilitate cum observatio sit crassa' ita quidem ut elementa inde deducta multum a vero abludere possint. Affirmat Longomontanus in Appendice ad Astronomiam Danicam cometam hunc non eluxisse ante diem 25tam Septembris siquidem die 24ta observavit Jovem et nihil novi in cœlo vidit quoniam vero media oppositio solis cum Jove die 25ta valde consentaneum putat, ut cometa tunc primum fulserit quo tempore fuit conjunctio Mercurii cum Sole et trinus adspectus Saturni cum Marte quæ ratio tunc temporis superstitionem subolet.

"Refert Keplerus in libro suo de cometis, pag. 4. cometam de 23tia Septb. animadversum fuisse, sibi vero (pag. 25.) primum visum ait Pragæ die 26ta Septb. hora media post octavam. Ex Thom. Harrioti M.sptis pretiosissimis vero apparet cometam hunc primum in Anglia die jam 13a. Sept. a quodam Torporleio Æn. [d] Nas. Coll. Oxon. socio, amico familiari Harrioti viro in omni mathematum doctrina versatissimo conspectum fuisse, primam vero astronomicam et exactam observationem Oxonii instituit quidam Standish cujus nomen forsan illam in qua inest obscuritatem meretur, a die 15 Sept. ut epistola Torporleii ad Harriotum die 30 Sept. data habet, instituit Torporleius observationes decem, Harrioti accuratæ observationes sunt a 21 Sept. ad 22 Oct. usque, ubi disparuit cometa. Modus illi observationis ille distantiarum a fixis fuit, et quo Tycho, Hevelius et Flamstedius relicto veterum astrolabio ad astrorum longitudines et latitudines determinandas, usi sunt & qui primæ astronomiæ recentioris fundamentum fuit. Maximum itaque operæ pretium erit Harrioti observationes ad recentiores calculos nostros revocare quod maximo ut cum fructu fiat, in æquam necesse erit observationum disquisitionem et correctionem inire, aggrediar itaque opus: Reductione distantiarum apparentium ad distantias veras, reformatione locorum fixarum a quibus cometæ distantiæ captæ erant ad epocham observationum, ex catalogis fixarum recentissimis, adhibita æquinoctiorum præcessione, luminis aberratione, axeos telluris nutatione, cometæ in distantiam parallaxeos annuæ et refractionis astronomicæ effecta, correctis insuper erroribus in ipsis observationibus ex collimationis difficultate et ex transversalium inæquali subdivisione in instrumenti limbo commissis. In Harrioti chartis nil aliud quam hujus cometæ distantiæ a fixis variis ad nullum revocatæ calculum exstant, sed dubium mihi nullum est virum clarissimum calculos hos instituisse, quoniam illos in cometam anni 1618 in optimo ordine digestos reperi, verisimilius est dispersas aut perditas nonnullas esse chartas, ast etiamsi exstarent prædictis indigerent correctionibus, nam Harrioti tempore non alius nisi Tychonis catalogus fixarum exstabat, quo igitur usus est ad longitudines et latitudines cometæ ex observatis distantiis depromendas, et quamquam Tycho asserit positiones fixarum ad veritatem minuti primi circuli maximi accedere tamen accuratissimus Flamstedius ostendit (Historia cœlestis) Tychonem sæpe cum in locis tum latitudinibus duos, tresve, interdum quatuor aut quinque totos scrupulos a vero aberrasse. Refractiones quoque tunc temporis primum a Tychone repertæ multum a vero aberant, supponit enim Braheus altitudines astrorum ultra gradus 20 non amplius infici astronomica refractione quandoquidem revera $5'\frac{1}{4}$ adhuc sit. Solis parallaxis ultra modum quoque assumpta erat, nam tribus constare minutis supposuit. Reformatio horum errorum post Harriotum evenit, nam A° 1660 Clar. Dominicus Cassini primus ostendit refractionem augeri, imminui debuisse solis parallaxin horizontalem, accedit ad hoc Tychonem obliquitatem eclipticæ plus justo majorem 2′ 30″ assumpsisse.

"Quæ tandem organa et instrumenta attinet quibus clar. Harriotus usus est in observationibus suis peculiarem materies hæc meretur disquisitionem, ex manuscriptis apparet illum tum armilla sicut in Brahei Astronomiæ mechanica instauratæ descriptam vides, tum Baculi Astronomici quem Angli 'staff' vel 'cross-staff' nuncupant, et quem apud Bernardum Waltherum, pag. 11. descriptum habes tum Radii Astronomici de quo Thom. Diggesius in Ala seu Scala Mathematica, pag. 2. post partem practicam, passim usum fuisse. Observationes vero his instrumentis institutæ incertæ evadunt, tum ob humani oculi excentricitatem in collimando, tum ob transversalium inæquali divisione, quibus accidit ut præcisio non raro citra 10′ aut circa sit; teste Hevelio in Machina sua Cœlesti docemur nobilem Danum in primæ magnitudinis stellarum distantia trium minutorum errores commisisse quos clar. Flamstedius soli collimationis errori adscribit. Notandum quoque est quod anno 1653 transversalium æquatio in divisione instrumentorum primum incœpit cogitari; Norwoodius in libro suo "Military Architecture" pag. 81. et post illum quidam Wm. Leybourn in opere cui titulus "Compleat Surveyor," lib. II. pag. 39. hanc monent divisionum inæqualitatem, tradit primus trigonometricam alter geometricam seu mechanicam demonstrationem, ad quod tamen si attentionem convertes, non ad amussim, sed multum poteris instrumenti errores imminuere et observationes pro re nata castigare ad majoremque producere accurationem, quibus denique in æquiorem problematis indagationem inire licebit. Eodem ferme modo purgandæ veniunt.

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[d] This is a mistake, Torporley was never fellow of Brazen-nose College, see P. 61; and the observation was made by sir William Lower. See Bradley's Misc. Works, p. 513. note g.

III. *Observationes Cometæ anni* 1618.

"Qui hujus anni est tertius, clarissimus et caudatissimus, apparitionis dies est Jovis 29^a. Novb. hora media post 6^{tam} ex Harrioti vero schedis apparet se illi adspiciendum obtulisse jam die 15 Novbris quod his habet verbis, "It was seen at Syon the 15th Nov. I did not observe it before the 20th." Refert Struyckius (pag. 262.) cometam hunc in India prope Jacatra die 7^a Nov. conspectum fuisse, quod in dubium vocat clar. Pingreius in Cometographia, tom. II. pag. 8. et secundum loco tertii acceptum fuisse suspicatur, tamen in Harrioti adnotationibus reperi cometam hanc ante hebdomadas tres in Indico mari navigantibus apparuisse. Kepleri et aliorum observationes sunt crassæ et dissidentes, Harrioti observationes vero sunt accuratiores et majorem orbitæ tractum exhibent, ac propterea orbitam a Halleio definitam ad majorem perducent perfectionem atque cum cœlo consensum.

"Volumen primum edituro tria hæc argumenta in votis sunt—
sed de hoc judicent viri clarissimi doctissimique."

NOTE C. p. 17, 29, 56.

"Translation of a paper given by Dr. Zach, astronomer to his highness the reigning Duke of Saxe Gotha, member of the Imperial Academy of Sciences at Brussels, of the Royal Academies of Sciences in Lions, Dyon, and Marseilles; printed in the Astronomical Ephemeris of the Royal Academy of Sciences at Berlin, published for the year 1788."

"I here present to the world a short account of some valuable and curious manuscripts, which [c] I found in the year 1784, at the seat of his lordship the earl of Egremont, at Petworth in Sussex, in hope that this learned and inquisitive age will either think my endeavours about them worthy of its assistance, or else will be thereby induced to attempt some other means of their publication. The only undeniable proof I can give now of the usefulness of such an undertaking, is by giving a succinct report of their contents, and by shewing briefly what may be effected with these materials: And although I come to the performance of such an enterprise with much less abilities than the different parts of it require, yet I trust that my love for truth, my design and zeal to vindicate the honor due to an Englishman, the author of these manuscripts, which are the chief reasons that have influenced me in this undertaking, will serve as my excuse.

"A predecessor of the family of lord Egremont, that noble and generous earl of Northumberland, named Henry Percy, was not only a generous favourer of all good learning, but also a

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[c] See P. 17.

patron and Mæcenas of the learned men of his age. Thomas Harriot, the author of the said manuscripts, Robert Hues, and Walter Warner, all three eminent mathematicians, who were known to the earl, received from him yearly pensions; so that when the said earl was committed prisoner to the Tower of London in the year 1606, our author, Hues and Warner, were his constant companions; and were usually called the earl of Northumberland's three magi.

"Thomas Harriot is a known and celebrated mathematician amongst the learned of all nations, by his most excellent work, entitled, 'Artis Analyticæ Praxis, ad æquationes algebraicas nova expedita et generali methodo, resolvendas. Tractatus posthumus.' Lond. 1631. Dedicated to Henry earl of Northumberland; published after his death by Walter Warner. It is remarkable that the fame and the honor of this truly great man was constantly attacked by the French mathematicians; for they could not bear that Harriot should in any way diminish the fame of their Vieta and Descartes, especially the latter, who is openly accused of plagiarism from our author. [See Montucla's Histoire des Mathématiques, part III. pag. 485 & seq.—Lettres de Mr. Descartes, tome III. pag. 457. Ed. Paris, 1667. 4to.—Dict. de Moreri, word "Harriot."—Encyclopedie, word "Algébre."—Lettres de M. de Voltaire, sur la nation Angloise, lettre 14.—Mémoire de l'abbé de Gua, dans les Mém. de l'Acad. des sciences des Paris pour 1741.—Jer. Collier's great historical dictionary, word 'Harriot.'—Dr. Wallis's preface to his Algebra[d].] Descartes published his Geometry six years after Harriot's work appeared, viz. in the year 1637; sir Chas. Cavendish, then ambassador at the French court at Paris, when Descartes's Geometry made its first appearance in publick, mentioned it to the famous geometrician Roberval, that these improvements in Analysis have been already made these six years in England, and shewed him in consequence Harriot's Artis Analyticæ Praxis, which, as Roberval was looking over at every page he cried out, 'Oui! Oui! il l'a vu!' 'Yes! Yes! he has seen it!' Cartesius had also been in England before Harriot's death, and had heard of his new improvements and inventions in Analysis. A critical life of this man, which his papers would enable me to publish, will shew more clearly what to think upon this matter, which I hope may be discussed to the due honor of our author.

"Now all this belongs to Harriot the celebrated analyst; but it has not hitherto been known that Harriot was an eminent astronomer, both theoretical and practical, which first appears by these manuscripts; amongst which, the most remarkable are 199 observations of the Sun's spots[e], with their drawings, calculations, and determinations of the sun's revolution round its axis. There is the greatest probability of Harriot's being the first discoverer of these spots before Galileo Galilei, or Scheiner[f]. The earliest intelligence we have of the first discovered solar spots, are of one Joh. Fabricius Phrysius, who in the year 1611 published at Wittemberg a little treatise, entitled, 'De Maculis in sole observatis et apparente eorum cum sole conversione narratio.' Galilei, who generally is taken for the first discoverer of the solar spots, published his book, 'Istoria e dimonstrazioni intorne alle Machie solare e loro accidenti,' in Rome in the year 1613. His first observation in this work is dated June the 2d, 1612. Angelo de filiis, the editor of Galilei's work, who wrote the dedication and preface to it, mentions, pag. 3. that Galilei had not only discovered these spots in the month of April in the year 1611, in Rome, in the Quirinal garden, but had shewn them several months before (molti mesi innanzi) to his friends in Florence: And that the observations of the disguised Apelles [he calls himself Apelles post tabulam] (the jesuit Scheiner, a pretender to this first discovery) were not later[g] than the month of October in the same year, by which the epoch of this discovery was put to the beginning of the year 1611. But a passage in the first letter of Galilei's works [h], pag. 11. gives a more precise term to this discovery. There Galilei says, in plain terms, that he had observed the spots in the sun before these 18 months. The date of this letter is of May the 14th, 1612, which brings the true epoch of this discovery to the month of November 1610. But, however, Galilei's first produced observations are only from June the 2d, 1612, and those of father Scheiner of the month of October, in the same year [i]. But now it appears from Harriot's manuscripts, that his first observations of these spots are of December the 8th, 1610. It is not likely that Harriot could have this notice from Galilei, for I find this mathematician's name [j] never quoted in Harriot's papers. I find him mentioning Josephus a Costa's book I. chap. II.[k] of his Natural and Moral History of the West Indies, in which he relates that in Peru there are spots to be seen in the sun which are not to be seen in Europe. It rather seems that Harriot had taken the hint from thence; besides, it is very likely that Harriot, who lived with such a generous patron to all good learning and improvements, had got the new invention of telescopes in Holland [l] much sooner in England than they could reach Galilei, who at that time lived in Venice. Harriot's very careful and exact observations of these spots, shew also that he was in possession of the best and most improved telescopes of that time; for it appears he had some with magnifying powers of 10, 20, and 30 times [m]. At least there are no earlier observations of the solar spots extant than his: they run from December the 8th 1610[n], till January 18, 1613. I compared the corresponding ones with these observed by Galilei, and found betwixt them an exact agreement [o]. Had Harriot had any notion about Galilei's discoveries, he certainly would have also known something about the phases of Venus and Mercury [p]; especially about the singular shape of Saturn, first discovered by Galilei; but I find not a word in all his papers about the particular figure of that planet.

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[d] The reference should rather be to chap. LIII. of the Algebra, especially in the Latin translation, which has many additions to what is contained in the English.

[e] See P. 32.

[f] See P. 37.

[g] "earlier."

" Of Jupiter's satellites—I found amongst his papers a great set of observations, with their drawing, position, and calculations of their revolutions and periods: his first [q] observation of those discovered satellites I find to be of January the 16th, 1610, and they go till February the 26th, 1612. Galilei pretends to have discovered them January the 7th, 1610: there is then all probability of Harriot's being likewise the first discoverer of these attendants of Jupiter [r].

" Amongst his other observations of the moon, of some eclipses [s], of the planet Mars [t], of solstices [u], of refraction [x], of the declination of the needle [y], there are most remarkable ones of the famous comets of 1607 and of 1618,[z] the later, for there were two this year, [Kepler de Cometis, p. 49 :] they were all observed with a cross staff, by measuring their distances to fixed stars, which makes these observations the more valuable, because they had but grossly been observed; Kepler himself observed the comet of 1607, but with the naked eye [a], pointing out the place where it stood by a coarse estimation, without an instrument; and the elements of their orbits could, in defect of better observations, only be calculated by them. The observations of the comet of the year 1607, are the more of importance and consequence, even now for modern astronomy, as this is the same comet that fulfilled Dr. Halley's most wonderful prediction of its return in the year 1759. Halley's prediction was only grounded upon the elements these coarse observations of it could give him; so he only assigned the term of its return to a space of a year. The most intricate calculations of the perturbations of this comet, done afterwards by M. Clairaut, reduced these limits to a month's space. We may now throw a greater light upon this matter by the more accurate observations on this comet by Mr. Harriot. In the month of October, in the year 1785, when I conversed upon the subject of Harriot's papers, and especially upon this comet, with the celebrated and eminent geometrician M. de la Grange, director of the Royal Academy of Sciences at Berlin, he suggested to me at that time an idea, which, if brought into execution, will clear up an important point in astronomy. It is known to astronomers how difficult a matter it is to determine Saturn's mass, and how little satisfactory the notions of it are hitherto; the whole theory of perturbations of comets depending upon this uncertain datum, several attempts and trials have been made towards this exact determination by the most eminent geometricians of this age, and especially by M. de la Grange himself; but being never satisfied with the few and uncertain data by which this problem may be resolved, he thought that Mr. Harriot's observations on the comet of 1607, and the modern ones of the same comet in the year 1759, would suggest a way to resolve the problem *à posteriori*; that of determining by them the elements of its ellipsis, the retardation of the comet compared to its period, may clearly be put to the account of the attraction and perturbation he has undergone in the region of Jupiter and Saturn; and as the part Jupiter acts in that is thoroughly known, the remainder will be Saturn's share, from which the mass of the latter may be inferred. In consequence of this consideration I have already begun to reduce most of Harriot's observations of this comet in order to calculate by them the true elements of its orbit in an elliptical hypothesis, to complete M. de la Grange's idea upon this matter.

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[h] " Work :" the page referred to is that of the Istoria.

[i] " The preceding year."

[j] See P. 29, 36.

[k] See P. 37. No paper of Harriot's has been found with this reference on it.

[l] See P. 20.

[m] See P. 20, 28, 34.

[n] See P. 37.

[o] See P. 32.

[p] Galileo was never able to see the phases of Mercury. Opere, tom. IV. P. 244.

[q] See P. 29.

[r] See P. 29.

[s] See P. 21.

[t] See P. 40.

[u] See P. 40.

[x] See P. 40.

[y] See P. 41.

[z] See P. 19.

[a] There were no telescopes in 1607; but Kepler's sight was not good, and he sometimes used a lens to assist it.

"I do not mention here more of Harriot's analytical papers, which I found in a very great number; they contain partly several elegant solutions of quadratic, cubic, and biquadratic equations, partly other solutions, and loca geometrica, which manifest his eminent attainments, and will serve to vindicate them against the attacks of several French writers, who refuse him the justice due to his skill and eminences, merely to save Descartes's honor, who yet, by some impartial men of his own nation, was accused of publick plagiarism.

"Thomas Harriot was born at Oxford in the year 1560. After he had been instructed in grammar learning, be became a batteler or commoner at St. Mary's Hall: he took the degree of bachelor of arts in 1579. Soon after he came to the knowledge of the heroic knight sir Walter Raleigh, who allowed him a yearly pension. In 1584 he went with the said knight, and first colony, into Virginia, where he was employed in the discovery and surveying thereof; maps[b] of which I have found very neatly done amongst his papers. After his return he published 'A brief and true report of the Newfoundland of Virginia, of the commodities there found to be raised, &c. London, 1588.' It was put into Latin, and printed at Frankfort in the year 1590. Sir Walter Raleigh introduced him into the acquaintance of the earl of Northumberland, who did allow him a yearly pension of 300*l.* Wood, in his Athen. Oxonien. mentions only 120*l.* but by some of his receipts[c], I found amongst his papers, it appears he had 300*l.* which indeed was a very large sum at that time. Wood, in his Athen. Oxon., mentions nothing of Harriot's manuscripts; he only tells us of a manuscript in the library at Sion college, London, entitled, Epheremis Chyrometrica[d]. I got an access to this library and manuscripts, and was indeed in hopes of finding something more of Harriot's; for most of his observations are dated from Sion college[e]; but I could find nothing from Harriot himself. I found some other papers of his friends; he mentions in his observations one Mr. Standish, at Oxford, and Nicol. Torperly[f], who also was of the acquaintance of the earl of Northumberland, and had a yearly pension; from the former I found two observations of the same comet of year 1618[g], made at Oxford, which he communicated to Mr. Harriot. Thomas Harriot died the 2nd of July, 1621. His disease was a cancerous ulcer in the lip, which some pretend he got by having had the custom of holding the mathematical brass instruments, when working, in his mouth. I found several letters of his, and answers to them, from his physician Dr. Alexander Rhead, who in his treatise mentions Harriot's disease. His body was conveyed to St. Christopher's church, in London. Over his grave was soon after erected a monument, with a large inscription thereon, but destroyed with the church itself by the dreadful fire of Sept. 1666. He was but 60 years of age. [See Wood's Athen. Oxonien. word 'Harriot,' p. 390, 391, 392."]

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[b] These maps were searched for in 1820, when only two were to be met with, one of the American hemisphere, the other of North America only. The following is the account drawn up at the time by the gentleman who made the inquiry. "These maps are on vellum, neither of them finished, nor on such a scale as to deserve the name of a survey, nor indeed to be of any use in determining the precise form of a coast, as will be understood when I say, that though the names are crowded together, and written in a very minute character, no more than twenty names are inserted from C. Florida to the Chesapeak." Indeed Wood was mistaken in the view which he took of Harriot's mission. Oldys describes him as "that virtuous and religious man, who, being sent over by sir Walter Ralegh with his first colony to Virginia, but did not bear him company, as the said author (Wood) mistakes, to survey the bounds of the English discoveries, with the commodities and customs of the country, did first communicate the gospel to those heathen inhabitants." (Life of Sir. W. Ralegh, p. 177. Oxf. Ed.)

NOTE D, p. 18.

The following are copies of Dr. Robertson's two reports[h].

"October the 20th, 1794.

"The following are the titles of the papers put into my hands in July last, in order to be examined:

1. Περὶ χωρίου ἀποτομῆς: seu de spatii resectione: Propositio generalis: ex lib. 7. Pappi.
2. De centro gravitatis pyramidis.
3. Ptolomaicum elementum de compositione rationum.
4. Theoremata ad subtensas periferiarum.
5. Lemmata.
6. Problemata.
7. De Parabola.

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[c] These documents have not been met with. Probably there is some mistake: we could hardly expect to find the acquittances among the papers of the individual who received the money.

[d] See P. 40.

[e] See P. 22.

[f] Torporley's name was Nathaniel, but by some mistake, possibly of the printer's, he is also in one place called Nich. (Wood Ath. Ox. vol. II. p. 300.) The university registers having been consulted to ascertain which was right, another correction was also found necessary. Wood says (p. 524) that after having been with Vieta, "he returned to the University, and "entring himself into Brazennose Coll. did as a member thereof take the degree of master of arts, an. "1591." Now there are no traces in the college books of his having belonged to B.N.C. but it happened that the person was of that society whose name stands immediately before Torporley's in the register for degrees, and Wood's eye, by passing from one to the other, most probably occasioned the mistake. Torporley certainly became M.A. as a member of Christ Church, where he originally entered. See also note [b], P. 53.

[g] "1607."

[h] The object of these reports has been mistaken. Dr. Robertson was merely applied to for an opinion on the expediency of printing the whole or any part of Harriot's papers, and he confined himself in his answer to this specific point. It may be seen, for example, that he mentions Oct. 17, 1610, as the first day on which Harriot saw a satellite of Jupiter, but he takes no notice of the B. de Zach having spoken of an observation in the preceding month of January. In a communication of this kind Dr. Robertson kept to the inquiry which was submitted to him, and did not embarrass the question by any unnecessary historical disquisitions. It ought possibly to be explained that the long interval between the reports was occasioned by the second portion of the papers having originally been put into the hands of another gentleman, who was prevented from attending to them.

8. De centro gravitatis trianguli.
9. De centro gravitatis parabolæ.
10. De Asymptotis.
11. De reflectione corporum rotundarum.

"These papers, excepting the last, are in no point of view fit for publication. The greatest part of them consist of detached and unfinished explanations of the authors which he read; begun, according to all appearance, with the design of satisfying his own mind upon the subject before him, and dropped abruptly as soon as this satisfaction was obtained.

"The 1st, 2nd, 3rd, 4th, 5th, and 6th, of the abovementioned articles are of this kind. The 7th, 8th, 9th, and 10th articles, seem to have been entered upon with an intention of treating the subjects in a more perspicuous way than any which had been pursued before his time; and had he written the 7th and 10th with a view to publication, there is every reason to suppose that in these two he would have succeeded in his design. In point of matter, as far as they extend, they are ingenious improvements upon Apollonius; but the same improvements, fully and elegantly demonstrated, are to be found in Mydorgius's Conic Sections, published in 1631. I should suppose that Harriot had not read Archimedes when he wrote the 8th and 9th articles, as they are so very much inferior to what we have by that celebrated mathematician on the subject.

"To these remarks on the substance of the first ten articles, it may be proper to add, that they are destitute of the principal particulars requisite in regular mathematical composition. No first principles are laid down; due arrangement is overlooked; and the demonstrations, often defective, are expressed in a kind of algebraical shorthand. In saying this, I by no means intend to insinuate any thing disrespectful to the memory of Harriot. I offer these observations as reasons for my firm persuasion that he never intended the papers for publication; and that it would be injurious to his reputation to print them.

"The paper 'de reflectione corporum rotundorum,' when compared with those already mentioned, may be considered as highly finished, but to its publication some strong objections may be made. Harriot himself states its imperfections in his letter, which accompanies this; and to these imperfections it may be added, that every thing depending upon the composition and resolution of forces is so much better understood, and more clearly treated, since the discoveries of sir Isaac Newton, that it would suffer much upon a comparison with modern publications. The subject is more fully and more elegantly handled in Keill's Introduction to Natural Philosophy."

"Feb. 15, 1798.

"To a great many of these papers there is no title, nor do they admit of any specific description. They appear to be rough calculations of some particulars which he wished to ascertain, without any allusion to the data with which he set out, or obvious tendency to the object in view. Others admit of classification, and afford abundant proof of Harriot's zeal in the cause of science, and of his unremitting attention to its improvement.

"The first class of this description relates to the spots on the sun. From these papers it appears that he first began to observe the spots on the 8th of December in 1610, and that he continued to observe them, at irregular intervals, to the 18th of January 1613. The observations recorded are 199 in number, and the accounts of them are accompanied with rough drawings, representing the number, position, and magnitude of the spots. From the manner, however, in which these observations appear to have been made, and also from that in which I find them recorded, I do not think that Harriot ever intended them for publication; nor do I think that the publication of them now would either satisfy rational curiosity, or contribute, in the smallest degree, to the advancement of astronomy. The circumstances under which the observations were made are very briefly and very vaguely recorded, and consequently, no calculations can be founded upon them likely to lead to accurate conclusions. These deficiencies I am inclined to attribute, partly to his having had no intention to publish on the subject, and partly to the imperfection of astronomical and philosophical instruments in his time.

"From this class of papers of which I am now speaking, it plainly appears that Harriot had no coloured glass to defend the eye, for the following expressions frequently occur, 'A mist,'—'a notable mist,'—'misty and cloudy,'—'the sonne was somewhat to cleare. There being no cloudes but only thick ayer,'—'convenient thin cloudes,' &c.

"As there is no reason to doubt of his diligence, I think that such expressions as the following are to be attributed to the imperfections of his instruments, and not to his want of care: 'The altitude of the sonne being 7 or 8 degrees.'—'the sonne being 3 or 4 degrees hy,' &c.

"The next class of papers (fixed together with a pin) which met my eye, is entitled, 'Descriptio parabolæ per circularem motum.' This I proceeded to examine, with sanguine hopes of finding a practical method of describing a parabola by an uninterrupted motion. My hopes, however, quickly vanished; for this class only contains diagrams, so rough and confused, that it does not appear from them upon what property of the circle he founded his attempt.

"After this disappointment I proceeded to examine a class of papers entitled, 'De Jovialibus Planetis.' From two pages of these papers, it appears that he first observed Jupiter's satellites on the 17th of October, 1610, for both of them have this date; and at the top of one of them there is this expression, 'My first observation of the new planets;' and at the top of the other, 'My first observation and others following of the new found planets about Jupiter.' At the top of another page there is this expression, 'The second years observations, being anno 1611, of the Joviall Planets;' and on this same page, there are the following dates prefixed to his accounts of observations, 'Syon Octob. 1'—'Octob. 6'—'Dec. 11'—'Syon $\frac{1611}{1612}$ January 12'—'January 13'—'January 26'—'February 15'—'February 17'—'Feb. 26.'

"Rough drawings of the positions of the satellites, and rough calculations of their periodical revolutions, accompany the brief statements of the observations; but, in my opinion, astronomy could not be advanced by the publication of any part of them.

"The other papers which admit of classification, may be entitled, Observations of the Moon—Observations of the comets of 1607 and 1618,—An Examination of Willebrord Snell's observations for determining the length of a degree of a meridian,—'The effects of the Decree of the councell of Neece for the observation of Easter-day,'—'The doctrine of Nautical Triangles'—Remarks on Eratosthenes, Tycho, Kepler, Paul Hainzelus, and Vieta.

"It is needless to enter into a minute description of these papers, as they are not drawn up with any degree of regularity and precision, and as it evidently appears that they never were intended for publication.

"Upon the whole it is my opinion that the publication of the papers mentioned in this report could only tend to prove that Harriot was very assiduous in his mathematical studies, and in his observations of the heavenly bodies; it could not contribute to the advancement of science."

NOTE E, p. 19.

Dr. Hutton's addition is as follows:

"As to his (Harriot's) manuscripts lately discovered by Dr. Zach, as above mentioned, it is with pleasure I can announce, that they are in a fair train to be published: they have been presented to the University of Oxford, on condition of their printing them; with a view to which, they have been lately put into the hands of an ingenious member of that learned body, to arrange and prepare them for the press."

NOTE F, p. 37.

"... I say moreover, to confirme this truth, that the heavens move and in them the starres march in turning, the which we cannot easily discerne with our eyes, seeing we see that not onely the starres do move, but also the regions & whole parts of heaven: I speake not onely of the shining & most resplendent parts, as that which we call Via Lactea, and the vulgar, S Jaques way, but also of the darker & obscurer parts in heaven. For there we see really, as it were spots and darkenes, which are most apparent: the which I remember not to have seene at any time in Europe, but at Peru, and in this other hemisphere I have often seene them very apparent. These spots are in colour and forme like unto the Eclips of the Moone, and are like unto it in blacknes and darkenes: they march, fixed to the same starres, alwaies of one forme and bignes, as we have noted by infallible observation."

The Naturall & Morall Historie of the East and
West Indies by the R. F. Joseph Acosta
4to. London 1604. Booke I. Chap. 2. p. 8.

NOTE G, p. 30.

From the regular series of entries it does not appear that Harriot observed the satellites of Jupiter in any year on the 16th of January. It was necessary therefore to look to the loose papers which contain his calculations. On one of these the following passage occurs:

" De planetis circa ♃. pro periodo. a.
1610

from the 17th of Jan.		to the 19th	Feb.	33^d	2 periods	occid.
24		26		33	2 per.	or.
15		17		33		

7						
17	Ja.	26	Dec.	353	21 per.	occ.
16		1611		$16\frac{1}{3}$	1 per.	
26	Ja.	3	Jan.	351	21 per.	or.
				$16\frac{8}{21}$	1 per.[i]	

The Baron could hardly have derived his date from this paper. The observation which it contains was evidently made on the 26th of January, in some place where the reformation of the calendar had already been adopted, and 16 was only written over the other numbers as a reduction to the old style. Likewise if he had disregarded this circumstance, he would certainly, in taking his earliest date from this place, have assumed it to be the 7th of January, which stands in a line immediately above the other. We may therefore safely conclude that the cause of the error must be looked for in some other place, and the 16th of January, 1610, occurs nowhere else but under the following circumstances. On another paper we find :

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[i] The periods $16\frac{1}{3}$ and $16\frac{8}{21}$ are the results of the intervals having been taken originally as 343 and 344 days, for which the other numbers 353 and 351 were afterwards substituted. But 353 is the interval between the 7 of Jan. and the 26th of December, which shews that the latter observation was made in England, and the former on the continent.

De Jove primo

1610
Feb. 11^{o}. stilo novo

or.	☌ perig.	occ.	☌ apogæa.
Feb. 11^{d} 11^{h},86	11^{d} 17^{h},17	11^{d} 22^{h},48	12^{d} 3^{h},79
21, 24	21, 24	21, 24	21, 24
10 14, 62	10 19, 93	11 1, 24	11 6, 55
9 17, 38	9 22, 69	10 4, 00	10 9, 31
8 10, 14	9 1, 45	9 6, 76	9 12, 07

In this manner the numbers are continued on a half sheet of foolscap, till at the bottom we arrive at the day in question.

[Jan.] 17 17, 14	17 22, 45	18 3, 76	18 9, 07
16 19, 90	17 1, 21	17 6, 52	17 11, 83.

There is another paper on which this last line is copied out at the top, but the numbers on it are then carried back to the 7th of Jan. 1610. There remains therefore but one supposition on which to rest—the numbers from which the above extract has been made, most probably caught the Baron's eye; and hastily running down the column of dates, he must have taken out the last as the earliest record of Harriot's having seen a satellite of Jupiter. This is the only means which the papers afford of accounting for his assertion.

The memorandum of "stilo novo" might be expected to have suggested that the numbers did not belong to observations, which Harriot would have made in England ; and it is absolutely impossible that any one could have continued to see, in such an uninterrupted series, the conjunctions and elongations of the satellite. Even if this had been possible, his observations would not have all differed from each other by intervals which were identical to the second place of decimals ; and it may be seen that the quantities in each line have a common difference of 5^{h},31, while they uniformly differ from those, which are next above or below them in the same column, by 21^{h},24. All this, however, admits of a clear explanation from the real origin of the numbers. They are continued on a third paper to the 2nd of March ; and prefixed to this continuation we find the following memorandum :

De Jove primo

1610
Stilo novo Feb. 11^{d} 8^{h}, 3′
vel 11 8, 05
−1, 50
11 6, 55 ☌ apogæa

In the margin there is a drawing like that in Fig. 3, Plate IV. Sup. and by the side is written :
"motus a centro fuit 1^{h},50."

Galileo informs us in his Sidereus Nuncius, that in Feb. 1610, "Die 11 . . . . hora "tertia stella quarta Jovi proxima ab oriente visa est, reliquis minor, a Jove dissita per "min. 0. sec. 30." This will agree with the 30″ which is written under the mark for the satellite, but the time of the observation is set down by Harriot to be $8^h,3'$, which requires some further explanation. Galileo speaks of "hora prima, tertia, &c.—noctis," and, after the custom of his country, he reckoned not from noon or midnight, but from sunset[h]. Harriot seems commonly to have taken his data from the tables of Maginus; if therefore we make use of his Ephemerides [4to Franc. 1610.] for the obliquity of the ecliptic, the sun's longitude and the latitude of the place, the sun will be found to have set at Venice on the day in question at $5^h, 1'$ after noon, which, added to the 3 hours mentioned by Galileo, make up $8^h, 1'$. No allowance is here made for refraction, for the equation of time, or the sun's motion during the interval from noon; these quantities were probably not attended to in Harriot's calculation, but the result is sufficiently near to identify the observation: $1^h, 50$ was subtracted for the time which is supposed to have elapsed before the satellite was seen after the conjunction, and $6^h,55'$, which then remained, were assumed to be the epoch of "☌ apog." for the 11th of February. This is the quantity in the foregoing extracts from which the whole computation appears to have been derived.

Again, on another paper we find as follows:

De periodo Jovium primi.

1610 stilo novo

$$\left.\begin{array}{lrrrr} \text{A} & \text{Jan.} & 26^d & 9^h & 40' \\ & & 15 & 22 & 24 \\ \text{B} & \text{Feb.} & 11 & 8 & 4 \end{array}\right\} 382^h\tfrac{40}{100} = 18 \text{ period.}$$

$$\left.\begin{array}{lrrrr} & & 15 & 22 & 27 \\ \text{C} & \text{Feb.} & 27 & 6 & 31 \end{array}\right\} 382\tfrac{45}{100} = 18 \text{ period.}$$

ut proxime

$$31 \quad 20 \quad 51 \qquad 764,85 = 36 \text{ period.}$$

bg 3′½, *bf* 1′ 30″, ergo *xe* $25°\tfrac{38}{100}$

A ♃ 10°54′ ♊ R	B ♃ 10°51 ♊ Dir.
hoc est 10,90 ♊ *zab*	hoc est 2^s 10,85 ♊
+ 25,38	+ 25,38
A (¹♃) 3^s6,28 ♋ *ebd*	B (¹♃) 3 6,23 ♋
C ♃ 11°36′ ♊ Dir.	$\left.\begin{array}{r}360\\18\end{array}\right\} = 6480°$
hoc est 2^s 11,60 ♊	
+ 25,38	$\left.\begin{array}{r}360\\36\end{array}\right\} = 12960°.$
C (¹♃) 3^s6,98 ♋	

A. B.

$$6479°,95 : \underbrace{382^h,4000}_{15^d\ 22^h\ 24'} :: 360°,00 : 21^h,2446$$

A. C.

$$12960,70 : \underbrace{764^h,8500}_{31^d\ 20^h\ 51'} :: 360°,00 . 21,2447$$

By the side is a drawing like that in Fig. 3. Pl. IV. Sup., and against 30″ is written; "hoc est 1′½ a centro Jovis."

The smaller letters (*b*, *g*, &c.) refer to Fig. 4. Pl. IV.

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[h] Lalande, Voyage en Italie, Pref. xxxii.

The longitudes of Jupiter are taken from the Ephemerides of Maginus, and Harriot assumes $3'\frac{1}{2}$ as the greatest apparent distance of the first satellite from the centre of the planet: from these data he calculates that its revolution is performed in $21^{h},24$. His method of computation is sufficiently clear, but it may be useful to add some remarks on the particulars to which he applies it. B is evidently the same observation of Galileo, which has just been discussed; it is now set down 1′ later, but if the rough manner is considered in which the time has been found, this will not be material. C is likewise from the Sidereus Nuncius, where it is said in 1610, Feb. "die 27 hora 1 min. 4 ..... "orientalior a Jove min. 10, sequens Jovi proxima min. 0 sec. 30." Calculating again with the numbers found in Maginus, the sun set at Venice on this day at $5^{h}, 25'$ after noon; to which if $1^{h}, 4'$ be added, the sum will be $6^{h}, 29'$, or only 2′ less than the time given by Harriot. A occurs also in the same work, where it is said, Jan. 1610, "die "26 . . . hora quinta . . prope Jovem quarta stellula ex oriente emergebat cæteris minor, a Jove tunc remota m. 30." The distance is obviously wrong in its designation. It most probably was intended, like the others, to have been written "min. 0. sec. 30," from which the middle has been omitted by some mistake[i]. It is clear that Harriot at least understood it so, (which is sufficient for the present purpose,) for he assumes the relative position of the satellite to be the same in each of the three observations, and consequently makes no allowance in any of them for the time which might have elapsed from the central conjunction. Now the sunset at Venice for this day comes out $4^{h}, 40'$ from noon, which added to 5^{h}, make the time of observation $9^{h}, 40'$, exactly as it is here set down.

There is a separate paper, on which Harriot has divided the interval of 382,4 days by a series of numbers from 16 to 25. No reason is given for fixing on them, and they led him into a mistake, which he might have avoided by the trial of some smaller divisors. Of all the periods which he arrived at in this manner there was none which could agree with the time of the other conjunctions, excepting that which supposed the intervening repetitions to be 18. This being a multiple of the real number, might answer to all which were observed, and only made it necessary to imagine that others had occurred without having been seen. By this means, however, the duration of the revolution was reduced to one half of what it ought to be, and a fundamental error was in consequence introduced into the table which contains the supposed observation of the 16th of Jan. 1610. It follows also that all the quantities in the first and third columns are wrong, and that for 1610, Jan. "$16^{d}, 19^{h}. 90$" could not even be supposed to represent the time of either the satellite's conjunction or elongation.

The first and the last observations of the satellites which Galileo gives in the Sidereus Nuncius are on the 7th of January and the 2nd of March, 1610, which are precisely the days on which Harriot's table begins and ends. It is clear, therefore, although we find calculations "ex propriis observationibus," that in this case he depended rather on Galileo's observations than on his own—or the table might have been constructed before he began himself to observe.

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[i] The error occurs in the first edition, and has been copied in all the rest.

NOTE H. p. 42.

See note D.—Dr. Robertson has thrown his two reports together in his communication to sir David Brewster, and he prefixed the following reason for publishing them. The Baron de Zach says, with reference to this publication, "nous ignorons comment "et pourquoi après trente-huit ans de repos, on est revenu sur ces manuscrits;" but the cause existed in the misapprehensions which had been occasioned by his accounts of the papers, and which would not suffer the question to remain at rest.

In No. LI. art. 2. of the Edinburgh Review, when speaking of Baron de Zach, it is said, "Several years ago he visited England, and resided there for a considerable time. He lived "much in the family of lord Egremont; and we owe to him the discovery of several unpublished "MSS. of Harriot, one of the ablest and most inventive mathematicians of the age in which he "lived. These the Baron found among the papers of the nobleman just named. They have "since been consigned to the care of the University of Oxford; and are now, we have no doubt, "in the progress toward publication."

The belief implied in the conclusion of what has been quoted, ought not to have been entertained; for the manuscripts in question had been examined in Oxford, had been declared to be unfit for publication, and had been returned to the nobleman to whom they belonged more than sixteen years before the Edinburgh Review had arrived at No. LI.

NOTE I. p. 26.

The Lowers were a very old and respectable family in Cornwall, which in 1620 could trace up a pedigree[k] of thirteen generations. Carew records in his Survey of Cornwall, that Thomas was in 1602 settled at St. Winnow, between Lostwithiel and Fowey; and from an epitaph in the church of Landulph[l], it appears that his eldest son, sir William Lower, knt. died in Carmarthenshire. Gilbert[m] mentions, that sir William "served in "parliament for the borough of Lostwithiel in the 1st of James the Ist;" and this is nearly all the authentic information which I can find collected about him.

The parish registers of St. Winnow unfortunately do not go back far enough to give us any information with respect to the time of his birth; but it appears from the Oxford Matriculation books that William Lower, the son of a gentleman from Cornwall, entered at Exeter College June 10, 1586, and that he was then sixteen years of age. The college books for this year are not to be found, but the name appears subsequently till the summer of 1593, when it was removed without any indication of the individual's having taken a degree. We have indeed no direct evidence from which it can be certain that this is the same person, but it is not improbable; for if sir William was born about

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[k] Lysons's Magna Britannia, Cornwall, p. cxl.

[l] "Here lyeth buried the body of sir Nicholas Lower of Clifton, Knight, descended of the house of St. Winnowe, the sonne of Thomas Lower and Jane his wife, one of the coheyres of Reskymer, who had issue six sonns, viz. Sir William Lower Knight, deceased in Carmarthenshire, John Lower, the said sir Nicholas Lower, sir Francis Lower Knight, Thomas Lower, deceased in London, and Alexander Lower. He married with Elizabeth, one of the daughters of sir Henry Killigrew of London Knight, and dyed without issue, surrendering his soule to his Redeemer at Clifton the 17th day of Maye A. D. 1655."

[m] Historical Survey of Cornwall, vol. II. p. 192.

1570, he would have been of the very suitable age of thirty-one, when he was returned in 1601 as member for Bodmin to the last parliament of Queen Elizabeth. It has been mentioned that at the accession of James the First he was returned for Lostwithiel. He was knighted in the latter end of March 1604; this is clear from the Journals of the House of Commons, for he is called on the 26th Mr., and on the 28th sir William Lower[n]. King James did not dissolve the parliament till 1614, so that he was a member during the time that we have notices of his correspondence and intercourse with Harriot.

In 1607[o], in passing over from Ilfracombe to Carmarthenshire, he saw the comet, of which he sent the observations to Harriot. It is highly probable that he was then married, for he speaks of observing with the cross-staff that he "sent down for measuring of "land;" and it does not appear that the family had any property in Wales, excepting what came to them by the marriage of sir William with Penelope, the heiress of the Perrots. By this connexion he became possessed of a fine estate of 3000 acres, near Llanfihangel Abercowen, and one of the farms on it still retains the name of Trefenti, or Treventi[p], which is evidently the same place from which sir William dates his letters. There is also another of the farms, called Lower Court, which is said upon traditional authority to have been inhabited about 150 years ago by a person of the name of Thomas Lower.

Sir William was familiar with Latin, for he frequently uses it. Besides those which have been selected upon the present occasion for publication, there are several of his letters in the British Museum, and one of them is written to Harriot in that language. There can be no doubt of his attachment to science, and we find him interesting himself in all Harriot's pursuits. It appears likewise that the "Traventane philosophers[q]" had observed the moon before the discoveries of Galileo were known in this country. It was no small effort in those days for a country gentleman to study Kepler, and the sound judgment with which he seized what was really valuable in that extraordinary writer is worthy of remark. "I cannot phansie," he says, "those magneticall natures[r];" but he catches the true spirit of Kepler's genius, admires, as they deserve, the great laws which he elicited, and reasons from them in a truly philosophical manner, when he proposes to extend the elliptical theory from the planets to the comets.

Whitlock[s] mentions that when Goring, with the king's troops, attacked Christ Church in 1644, he was beat off by major Lowre. Gilbert[t] supposes this to have been the sir William Lower of whom we are now speaking, and says, "it appears pretty certain that "he afterwards engaged in the king's service;" but even if he was alive at the time, (of which there may be some reasonable doubts,) it is not likely that when turned of seventy he could have taken an active part in the civil wars. The person alluded to was most probably his nephew, (a son of his brother John,) the dramatic writer of the same name;

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[n] He was called Lowre in the first instance, but this variation of orthography is of no importance: nothing was more common in those days. Besides, there was no other member of the same name in that parliament, and on the 2d of April we find him again called sir William Lowre.

[o] Bradley's Misc. Works, p. 512.

[p] At p. 514. of Bradley's Misc. Works the name is printed Irakenti, but a further examination by more experienced eyes clearly made out that the first letter was T, not I, and the fourth (which in form is not unlike a x) was either v or u.

[q] P. 25.

[r] P. 43.

[s] Memorials, p. 119.

[t] Survey of Cornwall, vol. II. p. 192.

for although Wood[u] vaunts his loyalty, it is clear from the Journals of the House of Commons that he was made governor of Winchester castle in 1645, upon the recommendation of sir William Waller. He left his party, however, very soon after, and joined the king; for we find him in Jan. 1646 a prisoner at Abingdon, where Brown commanded for the parliament. How he managed to escape with his life is uncertain; but in the beginning of the following year he was permitted to go beyond seas, upon giving security not to act any thing against the parliament[x]. This leaves little doubt about the individual, for it is known that the younger sir William Lower lived for many years, previous to the restoration, in Holland.

Sir William, in his letter of the 6th of Feb. 1610, mentions the death of his "onlie "son;" and in March 1611 he tells Harriot, "I have lost my second boy also." What other family he might have had is not known, but he certainly left a son and a daughter. Sir Nicholas Lower makes a bequest (in a will executed Jan. 30, 1653.) of estates and property to Thomas Lower, esq. of St. Winnow, "son of my brother sir William "Lower," who appears to have been returned for East Looe to the long parliament of 1640. When Charles the First called upon the members to join him at Oxford, he obeyed the summons: he was there created Master of Arts in January 1643, and Wood in his Fasti[y] says, "I take this to be the same, who was son and heir of sir W. Lower, "of St. Winnow in Cornwall, knight, which Thomas dying a bachelor 5. Feb. 1660, "was buried in the parish church of St. Clement's Dane, within the liberty of Westminster." In consequence of his joining the king, the parliament expelled him in Jan. 1644: and he is possibly the individual, who, at the recommendation of sir Thomas Fairfax in Feb. 1647, with some other Cornish gentlemen, (whose interests and endeavours had been very useful in "reducing the west,") was allowed to compound "at "two years' full value, for taking off their respective delinquencies, and the sequestration of their estates[z]."

On his death the family property appears to have been inherited by his sister, who married a brother of Drummond, the earl of Perth. Her husband was knighted by James the First, and Lady Drummond's estates were divided among her four daughters. The lands in Wales descended to the second, who married Charles Trevanion of Carhays, esq., and the fourth, who became the wife of Francis Plowden, Esq. of Shiplake, near Henley in Oxfordshire. The Trevanion family have long parted with their share, but the other moiety, in which is the farm of Treventi, has lineally passed to the present possessor of it, Edmund Plowden, esq. of Haughton Hall, near Shifnal.

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[u] Ath. Ox. vol. III. p. 544.

[x] Journals of the House of Commons.

[y] Vol. IV. Part. II. p. 33.

[z] Journals of the House of Commons.

CORRECTIONS.

P. 13, line 12, *for* −2.5739 *read* +2.5739
P. 20, note*, *for* Lestwithiel *read* Lostwithiel
P. 28, l. 34, *for* Peyresc *read* Peiresc
P. 31, l. 34, *for* III. *read* IV.
P. 48, l. 20, *for* suggests *read* on this very account suggests

Apt. Z. Dist. Æquator — 51.27.22
Apt. Z. Dist of Pole — 38.30.38
89.58.0

The Sum of the Refractions = 1.54
½ Z = 0.57
½ Diff 0.13
1.10
0.44

38.30.38
Refr. + 44
38.31.22
Lat. 51.28.38

10.098797 9.900778 -1.311976 2.060798 .649500 44.6	125530 .79575 205105

A.	92080 64628 56708 81.55.24 46.26	2984 .1388 .1772 1612 .3298 44	42554 92080 34634 38.25.41 4.59	92180 64628 56808 81.55.58 45.52	2984
B.	92080 64628 56708 81.55.24 46.26	29836 13054 16782 16125 32907 0.44	92080 42554 34634 38.25.41 4.59	92080 32930 25010 19.39 7.51	1676 3586 5,264 1.9″⅓

Z. Dist.	obser.	compt.
38.30.40	0.44	0.44
51.27.30	1.10	1.9⅓
75.57.30	3.45½	3.38
76.55.20	4.4	3.54
82.41.50	6.50	6.50
82.41.50	7.10	

C.	92080 68253 60333 32.49− 24.41+	1678 6562 0240 3.38	92080 85874 77954 28.49− 26.31+	1678 8873 0551 3.54	92180 64628 56808 81.55.58 .45.52	1252 3190 1554 3492 46	34734 38.25.45− 4.55+ 35634 26.18½ 9.21½	95550 64628 60178 82.15.10 .26.40	8897 3190 9109 3402 0.45.

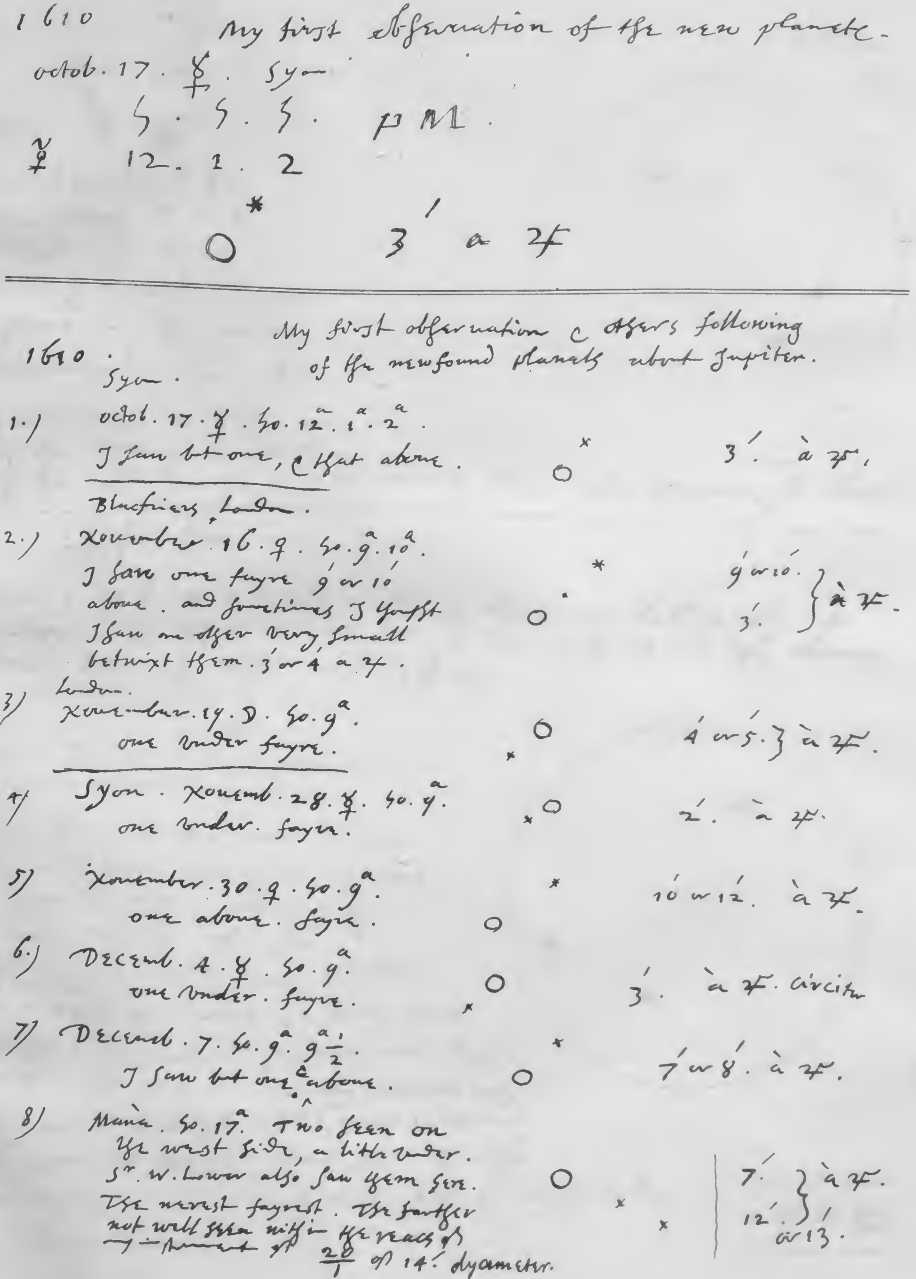
D.	93080 64628 57708 82.1.1 40.49	.1305 .3190 1885 1612 3497 46.	0745 3190 1030 3775 46−	97080 64628 61708 82.24.10 17.40	7109 3190 7280 3361 45−	42554 97080 39634 28.50− 1.50+	95550 42554 38104 27.52 2.48	99200 68253 67453 .54.58 2.31½	1636 8659 .0295 3.41

E.	99180 64628 63808 82.36.48 5.2	1656 3190 1769 3303 44+	99180 42554 41734 38.30.9 0.31	99200 64628 63828 36.55 4.55	1554 3190 1636 1699 3335 44½	99200 42554 41754 30.9½ 0.30½	77.1.16 76.55.52 5.24 1.26 Ref. 6.50	Capella Z. Dist 82.41.50

Oct. 27. mane
Bar. 29.47
Th. 22.

93091 75877 68968 83.9.31½ .48.28½	4.7015 1.3424 2.5739 7.2744 168539 29594 158945 841055	7.5825 1.4694 2.5807 6.4712 7.2012 9.2700 .1492 .4192 9.1½	 Z. R. = 2 C Z

J. Netherclift Lithog.



1610 My first obseruation of the new planete.
octob. 17. ☿. Syon
5. 5. 5. p M.
☿ 12. 1. 2

3′ a ♃

1610. My first obseruation & others following
Syon. of the newfound planets about Jupiter.

1.) octob. 17. ☿. ho. 12^{a}. 1^{a}. 2^{a}.
I saw but one, & that aboue. 3′. à ♃,

Blackfriers, London.
2.) Nouember. 16. ♀. ho. 9^{a}. 10^{a}.
I saw one fayre 9′ or 10′
aboue. and sometimes I thought
I saw an other very small
betwixt them. 3′ or 4′ a ♃.
9′ or 10′. } à ♃.
3′.

3) London.
Nouember. 19. ☽. ho. 9^{a}.
one under fayre. 4′ or 5′. } à ♃.

4) Syon. Nouemb. 28. ☿. ho. 9^{a}.
one under. fayre. 2′. à ♃.

5) Nouember. 30. ♀. ho. 9^{a}.
one aboue. fayre. 10′ or 12′. à ♃.

6.) Decemb. 4. ☿. ho. 9^{a}.
one under. fayre. 3′. à ♃. circiter

7) Decemb. 7. ho. 9^{a}. $9^{a}\frac{1}{2}$.
I saw but one & aboue. 7′ or 8′. à ♃.

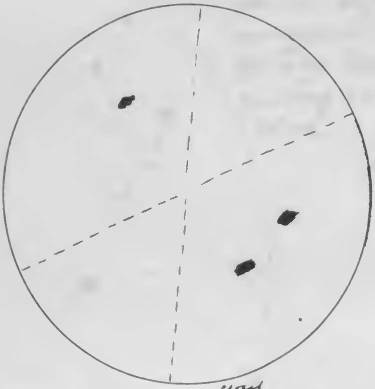
8) Mane. ho. 17^{a}. Two seen on
the west side, a little under.
Sr. W. Lower also saw them here.
The nerest fayrest. The farther
not well seen with the reach of
my instrument of $\frac{20}{1}$ of 14′ dyameter.
7′. } à ♃.
12′. } or 13′.

J. Netherclift Lithog.

1610. Syon.

Decemb. 8. mane.
♄.

The altitude of the
sonne being 7 or 8
degrees. It being
a frost & a mist. I saw
the sonne in this manner.
Instrument. $\frac{10}{1}$. B.
I saw it twise or thrise. once
with the right ey & other time
with the left. In the space of a minute time, after the sonne grew to cleare.

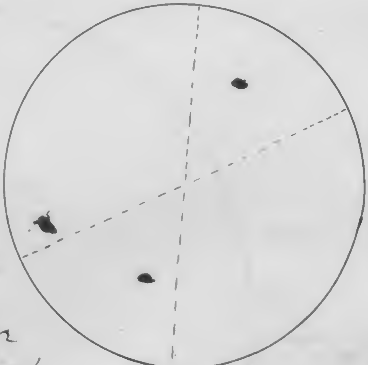


$\frac{1610}{1611}$. Syon.

January. 19. ♄. a notable mist. I observed diligently at sundry times when it was fit. I saw nothing but the cleare sonne both with right and left ey.

Syon.
1611. Decemb. 1. mane. ho. 10. 0'.
per horologium solare.

I saw three blacke spots in such
order as is here expressed as neare
as I could iudge. observed $\frac{10}{1}$.
5°. a towre. with Chr. also saw
the same. & observed at sundry times all three
seen at once for halfe an houre space.
at which time and all the morning before
it was misty.
The greatest was that which was most oriental appearing somewhat ragged, & was of apparent ~~angle~~ about 2'. The other
two, were neare of one bignes: & of 1' magnitude.
[illegible] three abouts



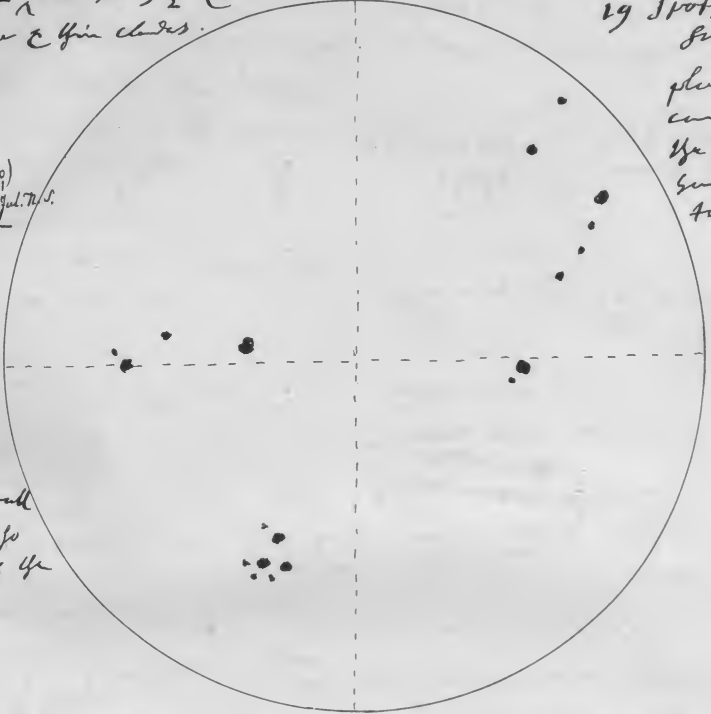
♂ ☉ ♀

Syon. ♀. June 26. ho. 5½. (6. mane.
thick vapor & thin cloudes.

19 Spots great & small.

places better coniectured then the day before seeing I had more time.

(Galil. 90
91)
♃. 5 Jul. N.S.
6 —



The 4 small spots not so black as the rest.

♃. June 11. ♃. ho. 3½. p.m.
through thin cloudes.
11. Spots.

now 5 in the cluster there. 6 more, of which the 2 southermost placed by coniecture. having not time inough to examine their places but not much amisse.

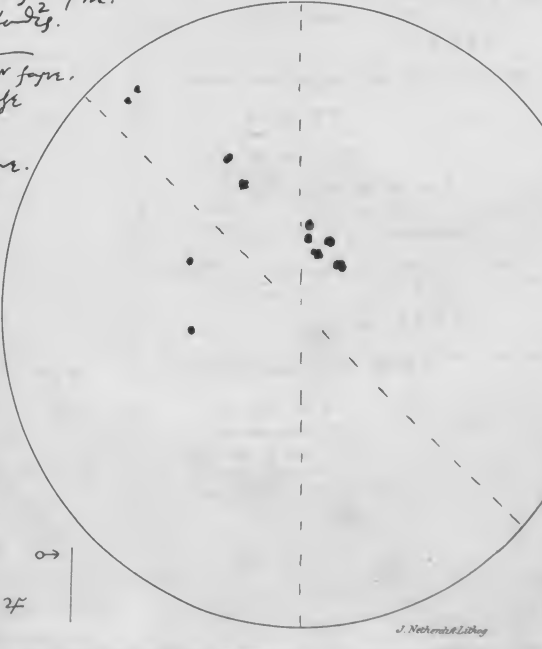


Fig. 1.

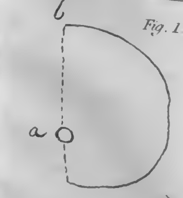


Fig. 2.

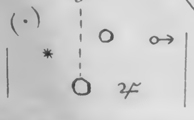


Fig. 3.

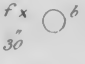
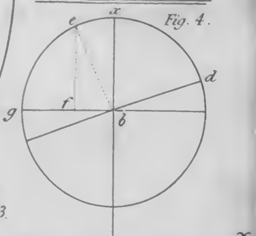


Fig. 4.



J. Netherclift Lithog

$a \sqcap +5, -7.$ $\qquad a \sqcap +7. -5$

$aaaa - 6,aa + 136a \sqcap + 1155 \sqcap - 136,a - 6aa + aaaa.$

$aaaa \sqcap + 6,aa - 136,a + 1155.$

$aaaa + 00,aa + 0000 \sqcap + 6aa - 136,a + 1155.$

Insigne exemplū propter sequentia.

$$\begin{array}{cccc} ee & \frac{eeee}{4} & ee & \frac{eeee}{4} \\ \sqcap & \sqcap & \sqcap & \sqcap \\ -2. & +1. & -2 & +1. \end{array}$$

$aaaa - 2aa + 1 \sqcap + 4aa - 136a + 1156.$

1°. $aa - 1 \overset{1^{o}}{\sqcap} + 2a - 34$

$33 \sqcap + 2a - aa.$

$aa - 2a \sqcap - 33$

$aa - 2a + 1 \sqcap + 1 - 33$

$a - 1 \sqcap \sqrt{-32}.$

$1 - a \sqcap \sqrt{-32}.$

$a \sqcap 1 + \sqrt{-32}.$

$a \sqcap \underline{1 - \sqrt{-32}}$

1°. $aa - 1 \overset{2^{o}}{\sqcap} + 34 - 2a$

$aa + 2a \sqcap 35.$

$aa + 2a + 1 \sqcap 1 + 35.$

$a + 1 \sqcap \sqrt{36}.$

$a \sqcap \sqrt{36} - 1 \sqcap + 5$

$-a - 1 \sqcap \sqrt{36}$

$a \sqcap -\sqrt{36} - 1 \sqcap -7.$

2°. $1 - aa \overset{1^{o}}{\sqcap} + 2a - 34.$

$35 \sqcap + 2a + aa.$

$35 + 1 \sqcap + 1 + 2a + aa$

$\sqrt{36} \sqcap 1 + a$

$\sqrt{36} - 1 \sqcap + a.$

$5 \sqcap a.$

$\sqrt{36} \sqcap - 1 - a.$

$-\sqrt{36} - 1 \sqcap + a.$

$-7 \sqcap a.$

2° $1 - aa \overset{2^{o}}{\sqcap} - 2a + 34.$

$2a - aa \sqcap 33.$

$aa - 2a \sqcap - 33.$

$aa - 2a + 1 \sqcap + 1 - 33.$

$a - 1 \sqcap \sqrt{-32}$

$a \sqcap + 1 + \sqrt{-32}$

$1 - a \sqcap \sqrt{-32}$

$a \sqcap + 1 - \sqrt{-32}.$

$$\begin{array}{l} \frac{eeee}{4} + 1,155 \\ ee + 6. \end{array} \sqcap \begin{array}{c} 68 \\ 68 \end{array} \sqcap \begin{array}{c} \frac{136}{2} \\ \frac{136}{2} \end{array} \sqcap 4624.$$

$$\frac{eeee,ee}{4} + 1155,ee$$

$$+ \frac{6eeee}{4} + 6930 \sqcap 4624.$$

$$\frac{eeeeee}{4} + \frac{6,eeee}{4} + 1155,ee \sqcap \begin{array}{r} +4624 \\ \underbrace{-6930}_{} \\ -2306 \end{array}$$

Insignis aequatio.

$-eeeeee - 6eeee - 4620,ee \sqcap + 9224. \qquad ee \sqcap -2$

vel:

$-eee - 6ee - 4620,e \sqcap + 9224. \qquad e \sqcap -2$

Tum

$eee - 6ee + 4620,e \sqcap + 9224, \qquad e \sqcap +2.$

$$\begin{array}{llll} vvv - 12v & \sqcap +9224 & \sqcap +12v & \\ +4620,v & +16 & -4620,v & -vvv \\ & -9240 & & \end{array}$$

$vvv + 4608v \sqcap + 0000 \sqcap - 4608,v - vvv$

$v \sqcap +0. \qquad\qquad v \sqcap -0.$

$e \sqcap +v+2 \qquad\qquad e \sqcap -v+2$

$e \sqcap +0+2 \sqcap +2 \qquad\qquad e \sqcap -0+2 \sqcap +2$

vel per Canonem

$$\begin{array}{llll} vv,vv,vv - 52vv & \sqcap +18496 & \sqcap +12vv & - vv,vv,vv. \\ +4620,vv. & -27720 & -4620,vv & \\ & -16 & & \\ & +9240 & & \end{array}$$

$vv,vv,vv + 4608,vv \sqcap + 0000 \sqcap - 4608,vv - vv,vv,vv$

$vv \sqcap +0.$

$ee \sqcap + vv - 2.$

$ee \sqcap +0 - 2 \sqcap -2.$

[illegible] Lithog.

ACCADEMIA
R. DELLE SCIENZE
DI TORINO